Female Leadership and Gender Equity:

Evidence from Plant Closure[∗]

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First Version: February 2011
Current Version: July 2011

Abstract

We use unique worker-plant matched panel data to measure the impact of female leadership on the relative pay of men and women. We measure differences in the wage changes experienced by newly hired men and women displaced from closing plants. We correct for endogenous selection of both the original and new employer, comparing the wage changes of men and women who move from the same closing plant to the same new firm. We observe larger wage losses among women than men immediately upon re-entering the workforce and continuing for the following three years. However, we find a significantly smaller gap between men and women who move to a new firm with a higher fraction of female managers: the magnitude of the extra losses to women is cut in half. The result is robust to including hiring firm fixed effects – a firm pays more equal wages to newly hired workers when it has more women in top positions. Our results suggest an important externality to having women in leadership positions: they improve the prospects of other women inside their firms. To the extent that gender wage differences are not driven by differences in productivity, removing these distortions can improve firm value.

I. Introduction

Women appear to suffer disadvantages in the labor market relative to men. In the cross-section, women receive 22% lower wages than men, controlling for differences in individual and occupational characteristics (Altonji and Blank (1999)). They are also less represented in upper levels of the corporate hierarchy: women hold only 6% of U.S. corporate CEO and top executive positions (Matsa and Miller (forthcoming)). These differences could arise from differences in the optimal job choices of men and women. But, they could also reflect differences in the work environment which favor men over women or even explicit firm preferences for male workers. In the latter case, women in leadership positions may improve the prospects of other women in their organizations.

We use newly available worker-firm matched panel data from the U.S. Census Bureau's Longitudinal Employer Household Dynamics (LEHD) Program to measure the effect of female leadership on gender equity in U.S. firms. We find that firms with more women in leadership roles have smaller pay gaps between men and women (controlling for worker characteristics) and also offer more equal pay to newly hired employees. The effects are distinct from time-invariant differences in firm cultures: changes in the gender composition of a firm's leadership team are associated with changes in the gender pay gap and the hiring practices of the firm.

The causes of gender pay differences within and across firms remain a topic of debate. Since women on average have shorter expected work lives and higher job turnover rates (Gronau (1988)), they may invest less in training and other forms of firm-specific human capital than male colleagues. These differences in turn can lead to differences in worker productivity across men and women in the same firm, even controlling for observables like age, education, and tenure inside the firm. Moreover, the choice of job is itself endogenous, making comparisons of men and women across firms problematic. For example, women may prefer to work in firms which are more family-friendly or which minimize commute times. Anticipating lower investments in firm-specific capital, women may choose to work in firms in which such capital carries less of a premium. Alternatively, men and women may differ in their attitudes towards competition or

negotiation (Bowles, Babcock, and Lai (2007), Niederle and Vesterlund (2007)), causing women to shy away from highly competitive industries like investment banking. Finally, women may make different job choices from men in response to discrimination in the labor market. If these differences in job choices are related to the sorting of women into leadership positions, then it is difficult to assess whether female leadership causes a reduction in pay disparity between men and women.

We use involuntary displacement due to plant closure as a way to address the endogeneity of job changes, comparing wage changes for men and women around the event. We use the Census Bureau's Longitudinal Business Database (LBD) to identify closures of U.S. plants between 1993 and 2001. We then link a subset of these plants to detailed worker-level information on demographics and quarterly wages from the LEHD data. The result is a novel panel dataset of 461,449 workers in 9,244 closing plants covering 23 states. Because the LEHD wage data extends to the first quarter of 2004, we are able to track workers displaced from closing plants for at least two full years following the closure.

By measuring wage changes following job loss due to plant closure, we distinguish voluntary from forced job changes. If men and women voluntarily change jobs at different rates, then wage changes around the full set of job changes (or new hires) will be difficult to interpret. We also remove differences between men and women in unobserved, time-invariant skills (or preferences) which might lead to differences in wage levels. Since such differences could also affect wage changes, we control for the pre-closure wage level to capture these effects. We also correct for differences in the job choices of men and women by estimating a pair fixed effects model which compares men and women from the same closing plant who move to the same new firm-unit in the year following closure. Thus, we estimate the difference in differences between men and women subjected to the same shock (i.e., the exact same involuntary job change), controlling for standard observable worker characteristics. We find a significant residual difference in the wage changes of men and women which, if anything, grows over the three years following the job change.

Our results suggest that a significant component of the wage gap between men and women cannot be explained by differences in worker choices. Next, we ask whether

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women in power mitigate these gender differences in employment outcomes. We use pay rank within the firm to identify the top management of each firm that hires displaced workers. We then classify hiring firms based on the percentage of women on the top management team. We find that the wage gap between displaced men and women is smaller if they are hired by female-led firms. The result is particularly strong if women comprise the majority of the new firm's management team. We also find that the results are strongest for women in the middle of the age distribution (beyond the main years in which career interruptions for child birth are a prime concern) and extend to women at the lower reaches of the wage distribution. Moreover, the results are robust to including hiring firm fixed effects; that is, individual firms pay more equal wages to newly-hired workers when they have more women in power than they do when men hold the top positions. Thus, our results are not due to women holding more top positions in female-friendly firms or industries.

As a final step, we ask whether similar leadership effects exist for minority groups among which we observe lower average pay, controlling for worker and job characteristics. Specifically, we repeat our analysis for Black and Hispanic workers. Black and Hispanic representation among firms' top employees is rarer than female representation, presenting an identification challenge. Yet, we find evidence of a similar effect of Black leadership, in particular, on the wage deficit of other Black workers in the organization. These results suggest commonality between the factors which drive pay differentials among women and racial minorities.

Overall, we find that women suffer disadvantages in the labor market that are not easily explained by differences in their job choices relative to men, but that women in leadership positions increase gender equity in their firms (and new hires). A caveat to our analysis is that women are not randomly assigned to the top positions inside their firms. Moreover, we do not have a natural experiment analogous to plant closure to provide exogenous variation in the within-firm composition of the leadership team. However, our results including hiring-firm fixed effects suggest at a minimum that women are necessary to implement shifts toward more egalitarian hiring and compensation policies.

Our paper contributes to the growing literature on "CEO style." Several studies find evidence of managerial fixed effects on a variety of corporate outcomes (Weisbach (1995); Chevalier and Ellison (1999); Bertrand and Schoar (2003); Bennedsen, Perez-Gonzalez, and Wolfenzon (2006); Frank and Goyal (2007)). We address a more focused question, asking whether a specific managerial characteristic (gender) has an impact on a specific corporate policy to which it has a natural link (pay differences between male and female workers). Several recent studies look at the link between female leadership and corporate decisions more generally, focusing on women serving on boards of directors (Adams and Ferriera (2009); Ahern and Dittmar (2011); Matsa and Miller (2011)). Most related to our results, Matsa and Miller (forthcoming) and Bell (2005) show that women top executives earn more in female-led firms. We extend their analyses of "women helping women" to the entire firm, looking at the impact of female leadership on the hiring of women throughout the organization and controlling carefully for endogenous differences in job choices by gender. Our results suggest an important interaction between managerial style and corporate culture (Kreps (1990)).

We also contribute to the extensive literature measuring gender differences in the labor market, surveyed by Altonji and Blank (1999) and Bertrand (2010). A key issue in this literature is distinguishing whether men and women are paid differently due to differences in qualification – the "human capital" hypothesis (Mincer and Polachek (1974) and Becker (1985)) – or due to differences in labor market treatment – the "discrimination" hypothesis (Becker (1957), Aigner and Cain (1977), and Bergmann (1974)). To control for qualifications and minimize the effect of gender differences in unmeasured characteristics, several papers have constructed "homogeneous" samples for young graduates out of college and tracked their career outcomes many years later (Wood, Corcoran and Courant (1993), Weinberger (1998), and Bertrand, Goldin and Katz (2009)). They show that women graduates earn significantly less than their male counterparts later in their careers. Although some of this difference can be explained by choices made, such as hours worked and career interruptions, a large portion (about 10-15%) remains unexplained. We take a different approach to separate the effects, looking at shocks due to job loss and using fixed effects to correct for endogenous selection.

Our analysis also relates to the literature that focuses directly on measuring the costs of worker displacement. Existing evidence using data from unemployment administrative records typically focuses on smaller samples consisting of a single state (Jacobson, Lalaonde, and Sullivan (1993) and Couch and Placzek (2010)). To our knowledge, ours is the first study to link administrative data across a wide panel of states to plant-level data. Our approach yields several new insights relative to such studies. For example, Jacobson et al do not find evidence of gender differences in displacement costs using a sample of Pennsylvania workers. Using our broader data, we overturn this result, showing that women suffer more from displacement in every region of the country except the Northeast. A small number of papers do find evidence of gender effects on the cost of displacement using survey data from the National Longitudinal Survey (NLS) or the Displaced Worker portion of the Current Population Survey (CPS) (Koeber and Wright (2006), Madden (1987), Maxwell and D'Amico (1986)). The samples of workers in these studies are typically small and the data do not allow the researchers to match workers to specific closing firms or to new post-displacement firms. Thus, they cannot separate selection effects from the unexplained impact of gender.[1]

The remainder of the paper is organized as follows. In Section II, we describe the data we use in our analysis. In Section III, we estimate the effect of female leadership on the pay gap between men and women using a random sample of LEHD data worker-quarters. In Section IV, we address endogeneity concerns by looking at differences in the outcomes of men and women displaced from closing plants. Finally, Section V concludes.

II.   Data

We use worker-, firm-, and plant-level data from the U.S. Census Bureau to estimate the impact of gender and female leadership on wages. We identify individual plants and their ultimate owners (firm), geographic locations (state and county) and industries (4-digit SIC) using the Longitudinal Business Database (LBD). The LBD covers all non-farm establishments with paid employees in the U.S. since 1976. It provides information

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[1] The papers using UI administrative records also fail to address these issues.

on plant-level employment and payroll as well as information on plant birth or closure (if any). We retrieve individual worker-level information – including employment, wage, gender, race and age – from the Longitudinal Employer Household Dynamics (LEHD) data. The LEHD data is constructed using administrative records from the state unemployment insurance (UI) system and the associated ES-202 program. The coverage of the state UI system is broad and generally comparable from state to state: it contains about 96% of total wages and civilian jobs in the U.S.[2] Wages reported to the state UI system include bonuses, stock options, profit distributions, the cash value of meals and lodging, tips and other gratuities in most of the states, and, in some states, employer contributions to certain deferred compensation plans such as 401(k) plans.[3] The U.S. Census Bureau negotiates agreements state-by-state to provide research access to UI data through the Census Research Data Centers (RDC). Currently, 23 states allow such access to their data: Arkansas, California, Colorado, Florida, Iowa, Idaho, Illinois, Indiana, Maryland, Maine, Montana, North Carolina, New Jersey, New Mexico, Oklahoma, Oregon, South Carolina, Texas, Virginia, Vermont, Washington, Wisconsin, and West Virginia.

Our identification strategy requires us to link worker data from the LEHD program to "plants" (or physical establishments) whose closing dates we observe in the LBD. Because the LBD and LEHD data share federal employer identification numbers (EINs) as a firm identifier, we can immediately link workers to their plants for single-unit firms. For multi-unit firms, however, it is not generally possible to assign individual workers uniquely to LBD plants since the LEHD data report tax units and the LBD reports physical business establishments. The internal bridge file at the Census, the LEHD Business Register Bridge (BRB), provides a link between the LEHD data and the LBD at various levels of aggregation. Its finest partition is at the EIN, state, county, and four-digit SIC code level. Thus, to achieve a match of workers (from the LEHD data) to a

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[2] Workers not covered by the state unemployment insurance system include many agricultural workers, independent contractors, some religious and charitable organizations, the self-employed, some state government workers, and employees of the federal government (who are covered under a separate insurance system). For detailed information on UI covered employment, see *The BLS Handbook of Methods:* http://www.bls.gov/opub/hom/homch5_b.htm.

[3] See http://www.bls.gov/cew/cewfaq.htm#Q01 for additional details.

unique plant (from the LBD), we require that the LBD plant is unique within this partition.

We impose several additional filters to arrive at our final sample of worker – plant matched data. First, we require that the closing plant has at least 50 employees. Second, we require that the SEIN(s) to which we link the closing plant disappear from the LEHD data in the LBD-identified closing year or within the first three quarters of the following year to avoid "closures" due to changes in administrative records. Finally, we consider workers who are employed in the closing plant two quarters prior to the last quarter the SEIN appears in the LEHD data. Workers may begin to exit a dying plant in the months preceding closure. To the extent that such exit is not random, it may bias our estimates of ex post wages and employment outcomes if we consider only the workers remaining at the closing date.

The LEHD wage data is currently available from 1992 through the first quarter of 2004.[4] Thus, we restrict our sample to plant closures between 1993 and 2001 so that we can obtain wage information prior to plant closure and track the outcomes of all sample workers for (at least) two full years following the closure.

Since the Census Bureau currently only provides access to employment records from 23 states in the LEHD data, we generally overstate unemployment rates in our sample: a worker may have a job record in one quarter, but disappear from the data the next due to either job loss or migration to an uncovered state. Since most of our analysis concerns changes in wages, our estimates should not suffer from selection bias as long as the factors affecting a state's decision to be included in the LEHD program are orthogonal to the determinants of (changes in) wages. Moreover, the within-sample rate of migration to a new covered state – even following plant closure – is low (approximately 2.5%). Thus, the potential impact of unobserved migration on our analysis appears to be small.

We make several adjustments to the reported wages for our analysis. We use the quarterly consumer price index to compute real quarterly wages in beginning-of-1990 dollars. We also aggregate quarterly wages into annual real wages. Because of annual bonuses and other predictable seasonal variation, quarterly wages may not provide an accurate reflection of the worker's earnings and quarterly wage changes may not reflect

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[4] States differ in their beginning years in the LEHD Program.

real changes to the compensation contract. We also require at least three consecutive quarters of wage data from the same firm and use only interior quarters in the computation. The latter restriction is necessary since the first or last quarter's wage reflects payment for an unobserved fraction of the quarter. Finally, we exclude workers younger than 16 or who earn less than $10,000 from our analysis.

We identify the top five managers in each firm-unit as the individuals who have the five highest annual real wages in the prior year. This definition is a natural extension of the typical notion of "managers" in the corporate finance literature. For example, Compustat's Execucomp database provides compensation information for the top five earners in the 1,500 largest publicly traded U.S. companies.[5]

In Table I, we provide plant-level summary statistics of the data. In Panel A, we provide summary statistics for a random sample of 655,929 plants from the LBD between 1993 and 2001. The average plant has 194 workers and a payroll of $6.83 million. 58% of plants are part of multi-unit firms and 42% are part of firms which operate in at least 2 distinct 2-digit SIC codes. In Panel B, we see that plants from multi-unit firms do not have significantly larger employment (mean = 202), but have larger payrolls (mean=$7.59 million). 55% of the plants come from the 23 states covered by the LEHD data.

We also consider a random sample of 143,370 closing plants from the LBD over the same time period. Relative to the average plant, closing plants appear to be smaller (mean employment = 188) and have smaller payrolls (mean = $5.3 million). Only half come from multi-unit firms, but the fraction from diversified firms is similar to the overall sample (39%). There are no obvious regional patterns in closure rates, but we observe a clear spike in closures in the recession year of 2001.

Finally, we provide summary statistics of the closing plants in our matched LBD-LEHD sample. Our matched sample has a similar industry distribution to the closing and random samples from the LBD. One consequence of our restriction to plants which are unique within their firm, county, and 4-digit SIC is that our matched data significantly under-represents plants from multi-unit firms (15% as compared to 49% in the total closing sample). However, conditional on being part of a multi-unit firm, the fraction of

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[5] We do not observe job titles directly in the LEHD data.

plants which are part of a diversified firm is 69%, which is similar to the overall LBD sample (71%) and only slightly lower than the LBD closure sample (79%). Matched sample plants are also smaller than the typical LBD (closing) plant, both among single- and multi-unit firms. In the full matched sample, mean employment is 134 and average payroll is $2.333 million. The matched sample also significantly under-samples the Northeast, most likely due to the exclusion of New York from the LEHD universe. Surprisingly, we do not observe a large spike in closures in 2001.

In Table II, we provide summary statistics at the worker level. In Panel A, we present statistics for a random sample of 251,440 worker-quarters from the LEHD data. The average worker is 41 years old with 3.4 years of tenure in the SEIN. Women make up 46% of the workforce. 10% of the workforce is Black, 4% Asian, 9% Hispanic, and 5% other non-white. The mean annual wage is $34,999.

On average, roughly one of the top 5 highest paid workers in each included firm is a woman (mean percentage female among top 5 = 0.15). 43% of the firms have at least one woman among the top 5 earners, 19% of the firms have at least two, and 8% of the firms have more than two. Because our sample consists of all public and private firms instead of the subsample of the largest public firms covered by standard data sources like Compustat's Execucomp database, we observe a somewhat higher frequency of women in top positions than prior studies. However, racial minorities are quite rare in these positions. In particular, only 12% (5%) of firms have more than one Black (Hispanic) worker among their top 5 earners.

In Panel B, we provide summary statistics for the workers in the LBD – LEHD matched sample of closing plants. The mean worker is one year younger and women make up only 41% of the workforce. Most noticeably, mean wages are smaller ($29,933), likely reflecting the smaller plant size in the matched sample (Table I). The frequency of women and racial minorities in the highest paid positions is similar to the random sample, though it appears women and Hispanics are somewhat more common among the leadership of firms which close plants.

Overall, our analysis reveals some non-random selection as a result of limitations in our ability to merge the LBD with LEHD data. However, it is unclear how or why these selection effects would interact with the impact of gender on wages or, further, on the

impact of female leadership on wage disparities. Our main tests use plant fixed effects as a way to correct for the non-random selection of closing plants into our sample.

III. Female Leadership and Worker Wage Levels

To begin, we explore the determinants of wage levels using the random sample of worker-quarters from the LEHD data described in Section II. We regress the natural logarithm of annual real wages on gender, race (broken into dummies for Black, Hispanic, Asian, and other non-white workers), tenure, age, education, and an indicator for whether the worker is native to the state in which his or her plant is located.[6] We also control for firm size (the natural logarithm of aggregate firm employment) and include an indicator for diversified firms (i.e., firms which operate in at least two distinct 2-digit SIC codes). Finally, we include state, 2-digit industry, and year fixed effects. We cluster standard errors at the SEIN (or, firm-unit) level. We report the results in Column 1 of Table III.

Our estimates are consistent with existing evidence on wage determinants. We find that Black and Hispanic workers earn significantly less, on average, than other workers. Our estimates of the magnitude of the effect are substantially larger than the estimates in Altonji and Blank (1999) using data from the March 1996 Current Population Survey (CPS); however, we also estimate the intercepts for Asian and other non-white workers separately from white workers, resulting in different comparison groups. We also confirm that older workers, workers with more experience in the firm, workers with more education, and workers from larger firms earn significantly higher wages. Workers who are native to the state in which they work earn significantly lower wages, suggesting a premium to mobility. Finally, we find that women earn roughly 29% less than men, which is somewhat larger than the 22% gap estimated by Altonji and Blank. In Column 2, we add firm-unit fixed effects to the specification, so that we estimate the impact of gender comparing only men and women working in the same firm-units. The gender gap drops to roughly 27% in this specification.

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[6] In the LEHD data, tenure is left-truncated. That is, we do not know how long workers have been with their firms prior to the beginning of our data sample. Moreover, education is an imputed variable. Thus, the coefficient is, at best, estimated with error. We include these variables simply to soak up variation potentially explained by factors other than gender (and note that it is not obvious why the problems with the variables would be correlated with gender).

Having confirmed the similarity of our sample to standard data sources, we turn to the effect of interest. In Column 3, we re-estimate the specification from Column 1, but including the percentage of women among the firm's top 5 earners and its interaction with the gender indicator as additional independent variables. We find a strong and significant positive effect of female leadership on the relative wages of women in their firms. Adding an additional woman executive (i.e., increasing the percentage of female top earners by 20%) decreases the gap between the wages of men and women by 15% (or, roughly 5 percentage points). At the limit, a firm with 100% women among its top 5 earners would have a mean gender wage gap of roughly 8%.

An important concern is the endogeneity of the gender composition at the top of the firm. For example, firms in consumer-oriented industries, pharmaceuticals, and telecommunication have higher rates of female managers (Dezso and Ross (2011)). If these lines of business generally attract higher quality women, then such firms may also pay women more competitive wages lower down the hierarchy. But, the link between female leadership and the wages of other women would not be causal. To test this hypothesis, we add firm-unit fixed effects to the specification. We report the results in Column 4. We find that our results are virtually unaffected: within-firm, wages are more egalitarian when women fill more of the top 5 positions than when men hold those same positions. Here, increasing the percentage of women among the top earners to 100% would virtually eliminate the wage gap (mean gap = 3%).

In Table IV, we dig deeper into the mechanism by which female leadership improves relative female wages, providing additional evidence on the marginal effects of each additional female leader. In Column 1, we test whether having a woman in the top position in the hierarchy has a significant impact on the wages of other women in the organization. We re-estimate the regression specification from Column 3 of Table III, but replacing the percentage of women among the top 5 earners and its interaction with the female indicator with an indicator for the top earner being female and its interaction with the female indicator. We find a positive association between the top leader being female and the relative wages of women in the firm. In Column 2, we repeat the estimation including firm-unit fixed effects. We find that changing from a male to female leader reduces the gender gap in the firm by 7.5 percentage points (or, by roughly 27%). In

Column 3, we report the results of a specification which estimates separate marginal effects for each 20% increment in the percentage of females among the top 5 earners and also for a woman at the top of the corporate hierarchy. We find a positive and significant marginal effect of roughly 5 to 7 percentage points for each additional woman added to the top 5 earners, starting from the first woman and continuing through the third woman. However, once women make up the majority of the top 5 earners, the marginal effect of adding an additional woman is small and statistically insignificant. In Column 4, we report the results from including firm-unit fixed effects in the regression, so that we use only within-firm variation in the composition of the leadership team for identification. The estimates are virtually unchanged. Interestingly, we do not find a significant effect of having a woman at the top of the hierarchy, once we control for the gender composition of the "leadership team."

IV.   Female Leadership and Displacement Costs

So far, our results suggest that women in power are an important factor in narrowing the wage gap between men and women in their firms. An important distinction for the policy implications of our results is whether women in power change the labor supply choices of other women or whether they "extend a helping hand," mitigating unfavorable labor market treatment that women cannot undo in the market by making different job choices. Distinguishing these possibilities (and, in particular, uncovering evidence of the latter) presents a significant identification challenge.

An immediate issue is the well-known difficulty in comparing wage levels across individuals: the potential for differences in unobservable quality. To deal with this concern, we focus on job (and wage) changes in the remainder of our analysis.[7] An advantage of this approach is that we can control for the pre-job change wage as a way to

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[7] This concern is exacerbated in our sample because we do not directly observe worker education. Though we control for imputed education, it remains possible that a portion of the measured difference in wage levels across men and women can be explained by measurement error: men may attain a higher level or quality of education on average than we capture with our control. Our measure of worker tenure inside the firm is left-censored since we do not observe the tenure of workers inside their current firms at the beginning of the sample period. If men, on average, have higher tenure in their firms than women, then this censoring could also bias upward our measurement of the wage gap between men and women. Our focus on job changes in the remainder of the paper also addresses this source of measurement error.

capture unobserved, productivity-relevant differences across men and women.[8] A disadvantage, however, is that the decision to change jobs is endogenous. For example, if men are more likely to make voluntary job changes than women and voluntary job changes yield better wage outcomes than layoffs, then on average women will have worse outcomes than men around job changes. Following Gibbons and Katz (1991), we address this concern by focusing on the (plausibly exogenous) subset of job changes among workers displaced as a result of plant closure.

Among the subset of displaced workers, a remaining issue is the endogenous matching of workers to firms. On the supply side of the labor market, men and women may have different preferences over career paths and working environments. For example, women may prefer flexible hours to accommodate family demands outside of the workplace. They may also anticipate making fewer ongoing investments in training or firm-specific capital than their male colleagues due to a shorter expected working life. In either case, these differences may lead to differences in job choices ex ante or ex post. These differences, in turn, may be correlated with the gender composition at the top of the firm. An advantage of our data relative to alternative sources like the CPS Displaced Worker Survey is that we observe all workers displaced from each closing plant and the identity of the new firms in which they are employed. Thus, we can construct a differences-in-differences estimator to correct for differences in job choices between men and women. Our main identification strategy is to compare the wage changes of men and women displaced from the same closing plant who move to the same new firm within the first four quarters following displacement. We then examine whether that gap is mitigated if the hiring firm has more women in leadership roles.

A. *Job Choices*

To begin, we illustrate the importance of correcting for differences in job choices between men and women. In particular, we consider the rates at which men and women change states or industries following plant closure. We estimate logit regressions on the

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[8] An added advantage is that studying job changes mitigates the effect of not observing hours worked on the interpretation of our results. Within our framework, we can compare the outcomes of men and women making similar pre-closure wages at higher levels of the wage distribution, where employees are likely to be salaried.

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sample of workers from closing plants. In Columns 1 and 2 of Table V, the dependent variable indicates that the worker moved to a different (LEHD-covered) state following displacement. Mirroring Section III, we include controls for the pre-closure wage, race (broken into dummies for Black, Hispanic, Asian, and other non-white workers), tenure, age, and firm size. We also include indicators for whether the worker is native to the state in which the closing plant is located and whether the firm is diversified. We also add two additional controls. First, we control for the number of plants in the same 2-digit industry and county as the worker's closing plant as a way to capture local labor market opportunities. And, second, we allow for a differential effect for the top earner in the SEIN (the firm's "manager"). Finally, we include state, 2-digit industry, and year fixed effects. We report the coefficient estimates as log odds ratios and cluster standard errors at the plant level. [9]

We observe a number of interesting patterns. Minority workers are significantly less likely to change states, as are younger workers, low-wage workers and "local" workers. Workers are also significantly less likely to change states if there are more local plants operating in their current 2-digit industry, suggesting that location and industry are substitutes. This effect is consistent with higher displacement costs among workers who move to new industries (Neal (1995)). Turning to the coefficient of interest, we find that the odds of women moving to a new state following plant closure are roughly 16% less than the odds among men, an effect that is significant at the 1% level. In Column 2, we partition the gender dummy by categories of worker age. We find that the effect of gender is stronger among older workers than among younger workers and is strongest for women between the ages of 35 and 45. This result is consistent with family considerations generating a more binding constraint among women than among otherwise similar men.

Next, we re-estimate the logit model using the same set of controls, but considering worker mobility across industries rather than states. In Columns 3 and 4 of Table V, the dependent variable indicates that the worker moved to a different 2-digit SIC following displacement. We again see some evidence of less mobility among minority workers and younger workers, though the effects are substantially weaker than the effects we see on

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[9] As is the case throughout the paper, our results are unchanged if we instead cluster at the firm-level to allow for correlation across different closing plants from the same multi-unit firm.

geographic mobility. We see that low-wage workers are more likely to change industries (as are managers, interestingly). Workers in diversified firms are also significantly more likely to change industries, suggesting a role for organizational structure in facilitating the redeployment of human capital (Tate and Yang (2011)). Finally, we see that women are marginally more likely than men to move to a new industry following plant closure. Here the effect is mainly driven by younger workers.

Overall, we find that the new jobs of men and women following displacement are systematically different. This result suggests that there are serious selection biases in comparing wage changes (or levels) across men and women in different firms. Thus, we take careful steps in the remainder of our analysis to isolate the portion of the gender effect that is not explained by different job choices among men and women.

B. Wage Changes

Next, we measure the differences in wage changes for men and women displaced by a plant closure. In Column 1 of Table VI, we report the estimates from an OLS regression of the change in wage around plant closure on a gender indicator. We measure wage changes using the difference in the natural logarithm of the annual real wage in quarters t to $t+4$ and $t-5$ to $t-2$, where quarter t is the quarter of closure. We also restrict the sample to workers who re-enter the workforce by quarter $t+3$. As controls, we include the set of race dummies from Tables IV and V, age, tenure in the closing plant, pre-closure wage, firm size, an indicator for the top earner in the SEIN (or, manager), and an indicator for diversified firms.[10] We also include state, 2-digit industry, and year fixed effects. We cluster standard errors at the plant level. We find that women experience a significant 4% decline in wages relative to men.

In Column 2, we address the first selection concern: women may work in different firms from men ex ante. For example, women may anticipate investing less in on-going training due to family considerations and, as a result, choose firms which place a lower premium on such investments. Then, wage changes following displacement are difficult

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[10] We do not include the imputed education control in these regressions or our indicator for workers native to the state in which the closing plant is located. The inclusion of these additional controls has no impact on our estimates of the gender effect, consistent with the effectiveness of pre-closure wages as a sufficient statistics for unobserved differences across workers.

to compare across groups since pre-event wages are set in systematically different firms. We correct for this source of selection by including a plant fixed effect in the regression. That is, we identify the gender gap by comparing only men and women originating in the same closing plant, and controlling for pre-closure wages and demographics. Note that including plant fixed effects makes the industry and state fixed effects redundant since we do not observe workers who appear in multiple closing plants. It also makes year fixed effects redundant since each plant appears in the sample only in the year in which it closes. We find that removing ex ante selection effects actually increases the magnitude of our estimate of the gender effect.

In Column 3, we address the second selection concern: women may choose different firms from men after plant closure. We include fixed effects for the closing plant, new SEIN pair. [11] This specification implements our main differences-in-differences identification strategy: we compare women to men who are impacted by the same shock (closure of the same plant) and who move to the same new business unit. We continue to find that women perform significantly worse than men. Moreover, we verify that the differences are not due to differences between men and women in the likelihood of moving to a new, non-closing plant within their original firm.

We also see some interesting patterns in the control variables. After controlling for selection effects, we see that minorities perform worse than white workers, though the magnitude of the effects is less than the gender effect in all cases. We also see that older workers and higher wage workers suffer more. The latter effect is interesting since men are higher paid than women in the cross-section. Thus, despite being higher wage workers, on average, men still outperform women following closure. We also see that workers with longer tenure in the closing plant suffer more, which is not surprising if longer tenure allows workers more time to accumulate firm-specific capital prior to closure. Finally, we see that managers outperform other workers from their closing plants who move to the same new business unit, despite being the highest paid worker (by definition) in the closing plant.

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[11] For the new firm, we only observe the worker's SEIN of employment. This is a lower level of aggregation than the firm (it is common for firms to have many SEINs), but a higher level of aggregation than the plant.

In Columns 4 to 6, we re-estimate the regressions from Columns 1 to 3, respectively, but using the two-year wage change as the dependent variable (i.e., the natural log of the annual real wage from quarters $t+4$ to $t+8$ minus the natural log of the annual real wage from $t-5$ to $t-2$). We only include workers reemployed by quarter $t+3$ (those included in Columns 1 to 3) to study the continuing change in wages. We also include a control variable which indicates whether the worker remained in the same new firm between quarters $t+4$ and $t+8$ and allow for a difference in the gender effect depending on whether the worker experienced an additional job change. We find that the qualitative patterns from the first set of regressions continue to hold when we look at longer term wage changes. The effect of age appears to deepen substantially over the longer term. One possible explanation is that older workers with better outcomes leave the sample due to retirement as we move further from the closure event. Turning to the effect of interest, we see that the difference in wage losses between men and women also modestly increases: the gap in wages over two years is 5 to 7 percent, depending on the specification.

Finally, in Columns 7 to 9, we repeat the set of three regressions using the three-year wage change as the dependent variable. Among workers who remain employed by the original post-displacement new firm, we do not see any additional growth in the gender wage gap: the gap is roughly 5.5 to 7 percent, depending on the specification.[12] However, the gap continues to grow among workers who experience at least one additional job change subsequent to plant closure: the gap ranges from 6.7 to 8.2 percent.

In untabulated analyses, we partition the age and wage distributions and allow for differences in the wage gap across groups.[13] We find that women experience larger wage losses than men across all groupings. Our result is also robust to considering only the subsample of "stayers," who worked in the closing plant for at least 5 years prior to closure. Finally, we consider the outcomes of workers who re-enter the labor force one or two years after the plant closure. The difference in wage changes between men and women substantially increases as the length of the unemployment spell increases. Thus,

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[12] This result is important as it suggests that the relative decline in wages among women in the first two years following the job change is not part of a larger continuing trend.

[13] We use the same groupings as in Tables VIII and IX, described in Section IV. Tables are available from the authors upon request.

our reported results provide a conservative measure of the impact of gender on wage changes. Overall, we conclude that women suffer more as a result of plant closure than men, even controlling for selection of the closing plant and the new firm.

Despite these controls, it remains possible that men and women differ in the terms at which they are willing to re-enter the labor market following displacement. An immediate concern is that women are more risk averse than men and therefore accept lower offers than otherwise similar men to avoid the possibility of prolonged unemployment. In our data, we do not see large differences between men and women in the rate at which they re-enter the workforce following plant closure. We estimate a slightly higher re-entry rate in the first year among women; however, women are also less likely to change states following plant closure (Table IV). So, even this near-zero effect is confounded by the possibility that men more often move to states which we do not observe in our data sample. Moreover, this story would imply that women hired at a given wage following plant closure are higher in quality than men. If this is the case, we would expect to observe convergence of the wages of women towards the (higher) wages of men over time. We instead find the opposite: the relative losses of women increase over the two years following the acceptance of a new job.

C. Wage Changes and Female Leadership

So far, we have shown that the cost of involuntary job loss due to plant closure is higher among women than men, even correcting for selection into different firms ex ante and ex post. Next, we tie the evidence back to our key result from Section III. That is, we test whether more women among the leadership team of hiring firms mitigates the wage differences among newly-hired men and women from closing plants. In addition to potentially identifying an important aspect of the "female leadership style," this test provides more direct evidence of the role of employer tastes on gender wage differences.

To measure the prevalence and impact of women in top positions of hiring firms, we adapt our strategy from Section III. First, we compute the percentage of women among the top five earners in the hiring firm in the year prior to hiring workers from a closing plant. On average, one of the top five managers is a woman in this sample of firms. 25% of the firms have at least two female managers and 12% of the firms have more than two

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female managers. Note these numbers are slightly higher than the corresponding numbers for the random sample reported in Table II. Otherwise, worker characteristics are not substantially different across hiring firms and the random sample.

Next, we re-estimate our workhorse regression (Table VI, Column 2), but including the percentage of women in the hiring firm's top 5 positions and its interaction with the gender dummy as additional independent variables. In Column 1 of Table VII we report the estimates. We continue to find that women fare worse than men following plant closure: the one-year wage change is six percentage points lower for women, a difference which is significant at the 1 percent level. However, displaced women who move to firms with a higher percentage of women in leadership roles do significantly better relative to men than women who move to firms with male-dominated leadership. At the limit, a woman hired by a firm with 100% female leadership would experience only a 1.5% larger wage loss than her male colleagues. We also find a significant negative level effect of the percentage of female managers. That is, workers of both genders experience larger wage losses when they move to female-led firms. This result is not surprising, given that female managers are more likely to be in smaller firms and there exists a strong positive correlation between wages and firm size.

As in Section III, we also estimate a set of regressions using separate indicators for different levels of female leadership. Given our earlier findings, we tabulate the estimates using an indicator for a percentage of women in the top 5 positions greater than 50%.[14] In Column 2 of Table VII, we re-estimate the regression from Column 1 with this alternative explanatory variable. We find that the wage losses among women are cut by nearly 60% among firms with a majority of women in the top leadership roles. In Column 2, we implement our full differences-in-differences specification, controlling for ex ante and ex post differences in the job choices of men and women. That is, in all cases we compare the one-year wage changes of men and women displaced from the same closing plant who move to the same new firm unit. We find that in cases in which the displaced men and women move to a firm with a majority of women in the top leadership positions,

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[14] As in Section III, we find that female top leaders do not have a significant impact on the wage gap once we include measures of the frequency of women on the top leadership team. Thus, we do not report results on female top leaders in Table VII.

the difference between their wage changes is cut nearly in half relative to cases in which they move to male-led firms.

An important question is whether women in leadership roles cause more egalitarian wage-setting practices or whether women are simply more common in the leadership roles of female-friendly firms. For example, women might more often lead firms which produce consumer products or products that are targeted towards women, and those firms might also have more favorable compensation policies towards women, particularly if women have specific knowledge or skills which make them more valuable as employees in such firms. To distinguish these possibilities, we re-estimate the regression including plant fixed effects and SEIN fixed effects separately. Thus, we estimate the impact of female leadership on the wage differential paid to newly-hired displaced workers using only variation in the leadership team within the SEIN. We report the results in Column 4 of Table VII. We find that the pay offered to men and women is more equal (the gap is roughly half as large) when women comprise the majority of the leadership team compared to times when men lead the same firm. We conclude that female leadership does indeed matter. Our results cannot be explained by sorting into firms with female-friendly cultures; instead, women in power are necessary to foster such cultures.

A natural explanation for our results is that women in power mitigate the bias in firms' hiring policies and – linking the results here with our results in Section III – in firms' compensation practices more generally. These biases need not indicate a direct preference for men, but might also arise due to a failure to recognize and correct for differences in the negotiation styles of men and women. One possibility is that (1) hiring firms on average expect lower investment in firm-specific capital (and, hence, productivity) from newly hired women than men (even controlling for observables including pre-closure wage and comparing only men and women hired from the same closing plant), (2) women in leadership roles encourage other women in their firms to reduce this deficit in productivity relative to men (either actively or by example), and, crucially, (3) women in leadership roles anticipate this effect and adjust the wages of newly-hired women accordingly. To test this conjecture, we re-estimate the impact of female leadership separately for breakouts of the age and wage distribution. In Table VIII, we report the results of re-estimating the specifications in Columns 2 to 4 of Table VII, but breaking

the continuous age control into five categorical variables and estimating the impact of majority female leadership in the hiring firm separately across categories. In particular, we consider separately workers under 25, workers between 25 and 35, workers between 35 and 45, workers between 45 and 55, and workers over 55. The distribution of our sample over the five age categories is similar across gender: the percentages are 7%, 30%, 32%, 21%, and 10% for men and 7%, 28%, 31%, 23% and 11% for women. For brevity, we tabulate only the level effects of the age categories, the interactions of the gender dummy with the age categories (i.e., the gender gap by age group) and the triple interactions with the female leadership dummy.[15] We find that women in leadership have the strongest effect on the relative wages of women over the age of 35. For women in the two oldest categories, the gender gap is no longer statistically significant in firms with majority female leadership. Interestingly, we do not find a significant effect on the relative wages of women under the age of 25 and a relatively weak effect on the wages of women between the ages of 25 and 35. If both the importance of investments in human capital and family pressures are maximized for women below 35, our results suggest that mitigating biases in wage-setting practices may be the primary channel through which women in power help their female colleagues.

In Table IX, we consider instead the wage distribution, dividing workers into seven wage groups – less than $20,000, $20,000 - $30,000, $30,000 - $40,000, $40,000 - $50,000, $50,000 - $75,000, $75,000 - $100,000, and greater than $100,000. We find that women in power indeed extend a helping hand to the lower reaches of their organizations and not just to other women in positions of power: the impact of female leadership on the relative wages of women is strongest for women earning between $20,000 and $50,000 annually. In this portion of the distribution, we do not observe a statistically significant gender wage gap among hiring firms with a majority of women in leadership positions. We do not find a significant effect of female leadership for women in the upper reaches of the wage distribution. However, we also have little power in this portion of the distribution.[16] There are relatively fewer women earning these high salaries and there are

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[15] The estimates of the controls are not materially different from the estimates reported in Columns 2 to 4 of Table VII.

[16] Note the very large standard errors on our estimates in these categories.

relatively few firms with majority female leadership. Even our weakest identification strategy requires sufficient observations in the intersections of these sets.

As a final step, we consider the impact of racial minorities in leadership positions on the wages of displaced minority workers hired by their firms. Our results thus far (see, e.g., Tables III and VI) suggest that Black workers receive wage discounts relative to white workers that are of the same order of magnitude as the gender wage gap. We identify firms with at least one Black worker among the top 5 earners. Using our differences-in-differences framework, we find that Black workers experience a significantly bigger wage loss compared to their white co-workers, but that the gap drops significantly (from 3.9% to less than 1%) when they move to a firm with at least one Black worker in a leadership role. The result is robust to including fixed effects for the closing plant, new firm-unit pair or to including separate fixed effects for the closing plant and new firm.[17]

Our finding that racial minorities in leadership positions increase the relative wages of displaced minority workers suggests demand-side biases as a component of observed wage differences between workers with differing demographics. We observe a commonality in the wage patterns among women and racial minorities. Yet, many of the other candidate explanations for a wage discount among women – related, for example, to child birth and family responsibilities – are unlikely to generate corresponding wage gaps for racial minorities.

V. Conclusion

Our results identify an important component of the "female leadership style": women in leadership roles lessen the compensation gap between men and women inside their firms. We use a unique employer-worker matched data set – drawing on data from the Longitudinal Employer-Household Dynamics (LEHD) Program and the Longitudinal Business Database (LBD) – to examine wage differences between men and women and the impact of women in leadership positions on those differences. Comparing wage levels across men and women is generally problematic due to differences in unobserved productivity-relevant factors. To avoid this problem, we compare changes in wages

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[17] These estimates are untabulated, but are available from the authors upon request.

between men and women involuntarily displaced from their jobs due to plant closures. Because of the richness of our data, we are also able to correct for endogenous matching of workers to firms both before and after plant closure. Our main differences-in-differences estimates compare the wage changes of men and women displaced from the same closing plant who move to the same unit of the same new firm within the first year following closure.

We uncover significant differences in the impact of closure on men and women which cannot be explained by differences in job choices. Given the divergence in wages between otherwise similar men and women at the time of hiring, the differences are also difficult to reconcile with rational expected differences in productivity. Controlling for worker and firm characteristics, we find that women suffer an additional one-year wage loss of roughly 5 percent compared to men. The difference is persistent and exists throughout the age and wage distributions. However, the gap is significantly reduced when women hold a greater fraction of the top 5 positions in the hiring firm. Moreover, the latter result is robust to comparing only new hires by the same firm at times when it is run by more or less women, confirming that our results are driven by managerial rather than firm styles.

Our results have important policy implications. Improving the ability of women to break through the "glass ceiling" and attain top leadership positions has positive externalities on other women. In particular, it improves the opportunities of women lower in the corporate hierarchy. Thus, changing leadership may be a mechanism to change the culture of the firm in a direction which is friendlier to female workers (or other workers impacted by labor market discrimination). Moreover, if differences in the treatment of men and women in the labor market reflect (implicit) employer tastes rather than expected differences in productivity, these changes may improve firm value by removing distortions in worker incentives.

An interesting direction for future research is to dig deeper into how women in leadership roles change the policies of their firms in a way which benefits other women. We show that firms with men in leadership roles exhibit a preference towards men over otherwise similar women at the time of hiring and, conversely, that women in power reduce gender disparity. A possible mechanism through which these preferences manifest

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is differences in the bargaining styles of men and women: women may be less aggressive than otherwise identical men in negotiating over initial salary. However, they may be more comfortable bargaining with another woman or, alternatively, women in power may be less likely to reward aggressive bargaining. If firms do not recognize and undo the resulting differences in wages (given that they are not due to productivity differences), then this friction in the hiring process can generate wage discrimination. Another possibility is that women in power change the ways in which social networks affect access to jobs and status within their firms. Prior studies suggest that an impediment to women attaining positions at the top of corporate hierarchies is limited access to social networks. In addition, recent research in finance finds that network connections play an important role in determining board appointments, with negative value consequences for shareholders (Fracassi and Tate (forthcoming)). To what extent do women in power increase the access of other women to such networks or reduce the influence of existing networks which impose barriers to entry? Though fruitful questions, such analyses require data with more information at the individual worker level than is currently available through the LEHD program.

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Table I

Summary Statistics: Plant Level

Panel A reports summary statistics of all closing plants in the LBD, a random sample of non-closing plants from the LBD, and the subsample of closing plants from the LBD that we match with worker-level data from the LEHD program. Panel B reports the corresponding statistics for the subsamples of plants from multi-unit firms. We define multi-unit firms as firms which operate at least two distinct plants. Standard deviations are reported in parantheses for continuous variables.

	Panel A: All Firms			Panel B: Multi-Unit Firms Only		
	Random Plants in the LBD (N=655,929)	Closing Plants in the LBD (N=143,370)	Closing Plants in the LBD Matched with the LEHD (N=12,439)	Random Plants in the LBD (N=383,238)	Closing Plants in the LBD (N=70,811)	Closing Plants in the LBD Matched with the LEHD (N=1,850)
Plant Employees	194	188	134	202	187	142
	(514)	(647)	(292)	(473)	(565)	(224)
Firm Employees	25,765	22,084	4,780	43,968	44,521	31,379
	(83,464)	(57,124)	(26,992)	(105,480)	(74,912)	(63,789)
Payroll (in $ 000's)	$6,830	$5,299	$2,333	$7,590	$6,676	$3,703
	($383,230)	($66,606)	($6,709)	($178,102)	($92,809)	($9,611)
% of Multi-Unit Firms	0.58	0.49	0.15	-	-	-
% of Diversified Firms	0.42	0.39	0.10	0.71	0.79	0.69
Industry Distribution						
SIC = 1	0.05	0.04	0.09	0.02	0.02	
SIC = 2	0.08	0.08	0.08	0.09	0.08	
SIC = 3	0.10	0.08	0.07	0.10	0.09	
SIC = 4	0.06	0.07	0.05	0.08	0.08	N/A*
SIC = 5	0.29	0.27	0.28	0.36	0.30	
SIC = 6	0.06	0.09	0.04	0.07	0.10	
SIC = 7	0.13	0.19	0.24	0.13	0.18	
SIC = 8	0.21	0.16	0.13	0.15	0.13	
Geographic Distribution						
LEHD State	0.55	0.57	-	0.55	0.57	-
Region = NE	0.22	0.22	0.08	0.21	0.22	0.09
Region = MW	0.25	0.21	0.16	0.25	0.22	0.18
Region = S	0.23	0.24	0.23	0.24	0.24	0.26
Region = SW	0.12	0.13	0.19	0.12	0.12	0.19
Region = W	0.14	0.16	0.29	0.14	0.15	0.22
Region = RM	0.04	0.03	0.05	0.04	0.03	0.06
Yearly Distribution						
Year = 1994	0.10	0.08	0.08	0.10	0.07	0.05
Year = 1995	0.11	0.08	0.10	0.10	0.08	0.07
Year = 1996	0.11	0.11	0.12	0.11	0.11	0.13
Year = 1997	0.11	0.10	0.09	0.11	0.10	0.07
Year = 1998	0.11	0.11	0.13	0.12	0.11	0.12
Year = 1999	0.12	0.12	0.12	0.12	0.14	0.10
Year = 2000	0.12	0.12	0.14	0.12	0.13	0.22
Year = 2001	0.12	0.21	0.14	0.12	0.17	0.17

*Some industries have a limited number of firms. Due to potential disclosure risk, we cannot report the industry distribution for this subsample.

Table II

Summary Statistics: Worker Level

Panel A reports summary statistics for a random sample of workers from the LEHD data. Panel B reports summary statistics for workers from the LEHD data matched to closing plants in the LBD. Annual wage is the mean real wage over the preceding four quarters multiplied by 4. To be included in the annual wage computation, a quarter cannot be the worker's first or last quarter in his/her current employment spell. Tenure is artificially set to zero for the first year each state appears in the LEHD universe. Education is imputed using an algorithm constructed by the LEHD program. % x Leaders measures the percentage of workers of type x among the top 5 earners in the worker's state employer identification number (SEIN).

	Observations	Mean	Std. Dev.	Observations	Mean	Std. Dev.
	Panel A: Random Sample			Panel B: Closing Plants		
Worker Characteristics						
Annual Wage	251,440	34,999	92,402	461,449	29,933	54,517
Age	251,440	41.33	11.10	461,449	39.68	11.43
Tenure (in yrs)	251,440	3.36	2.61	461,449	2.57	2.20
Education (in yrs)	251,440	13.79	2.60	461,449	13.66	2.66
Female	251,440	0.46		461,449	0.41	
Race = Black	251,440	0.10		461,449	0.10	
Race = Asian	251,440	0.04		461,449	0.04	
Race = Hispanic	251,440	0.09		461,449	0.12	
Race = Other	251,440	0.05		461,449	0.06	
Native to State	251,440	0.14		461,449	0.19	
Firm-Unit Characteristics						
% Female Leaders	235,822	0.15	0.21	438,298	0.20	0.25
% Black Leaders	235,822	0.03	0.10	438,298	0.03	0.11
% Hispanic Leaders	235,822	0.01	0.06	438,298	0.04	0.12

Table III
Female Leadership and Wage Levels

The dependent variable is the natural logarithm of the annual wage (defined on Table II). The omitted race caegory is "White". Age is worker age. Tenure is measured as the number of quarters that a worker has spent in the firm. Native is an indicator for workers who were born in the state in which they are currently employed. We define diversified firms (Diversified) as firms that operate in at least two distinct two-digit SIC codes. Firm Employment is measured as the total number of workers for the entire firm (across all its plants). % Female Leaders measures the percentage of women among the top 5 earners in the worker's state employer identification number (SEIN). Standard errors are clustered by SEIN and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)
Race = Black	-0.209 ***	-0.187 ***	-0.208 ***	-0.187 ***
	(0.005)	(0.006)	(0.005)	(0.006)
Race = Asian	-0.033 ***	-0.052 ***	-0.033 ***	-0.052 ***
	(0.008)	(0.010)	(0.008)	(0.010)
Race = Hispanic	-0.280 ***	-0.194 ***	-0.279 ***	-0.194 ***
	(0.005)	(0.006)	(0.005)	(0.006)
Race = Others Minorities	-0.035 ***	-0.023 ***	-0.035 ***	-0.023 ***
	(0.001)	(0.008)	(0.001)	(0.008)
Native	-0.040 ***	-0.041 ***	-0.040 ***	-0.041 ***
	(0.005)	(0.006)	(0.005)	(0.006)
Education	0.035 ***	0.027 ***	0.035 ***	0.027 ***
	(0.001)	(0.001)	(0.001)	(0.001)
Ln(Age)	0.279 ***	0.296 ***	0.278 ***	0.295 ***
	(0.006)	(0.008)	(0.006)	(0.008)
Ln(Tenure)	0.081 ***	0.086 ***	0.079 ***	0.086 ***
	(0.002)	(0.003)	(0.002)	(0.003)
Diversified	0.015 **	0.004	0.014 **	0.004
	(0.010)	(0.011)	(0.006)	(0.012)
Ln(Firm Employment)	0.029 ***	0.000	0.028 ***	0.000
	(0.001)	(0.003)	(0.001)	(0.003)
Female	-0.290 ***	-0.273 ***	-0.321 ***	-0.310 ***
	(0.004)	(0.005)	(0.004)	(0.006)
% Female Leaders			-0.257 ***	-0.159 ***
			(0.012)	(0.015)
(% Female Leaders)*(Female)			0.240 ***	0.278 ***
			(0.013)	(0.017)
Year Fixed Effects	yes	yes	yes	yes
Industry Fixed Effects	yes	no	yes	no
State Fixed Effects	yes	yes	yes	yes
SEIN Fixed Effects	no	yes	no	yes
R^2	0.308	0.595	0.311	0.596
N	235,822	235,822	235,822	235,822

Table IV
Female Leadership and Wage Levels (cont.)

The dependent variable is the natural logarithm of the annual wage (defined on Table II). Worker and Firm Characteristics are the control variables from Table III: race indicators (Black, Asian, Hispanic, and Other Minorities), Native, Education, ln(Age), ln(Tenure), Diversified, and ln(Firm Employment). Female Top Leader is an indicator equal to one if the top earner in the worker's state employer identification number (SEIN) is female. % Female Leaders measures the percentage of women among the top 5 earners in the worker's SEIN. Standard errors are clustered by SEIN and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)
Female	-0.297 ***	-0.281 ***	-0.325 ***	-0.312 ***
	(0.004)	(0.005)	(0.004)	(0.006)
Female Top Leader	-0.078 ***	-0.050 ***	0.001	0.000
	(0.007)	(0.008)	(0.007)	(0.009)
% Female Leaders > 0			-0.042 ***	-0.024 ***
			(0.005)	(0.006)
% Female Leaders > 0.2			-0.058 ***	-0.041 ***
			(0.007)	(0.008)
% Female Leaders > 0.4			-0.069 ***	-0.048 ***
			(0.011)	(0.013)
% Female Leaders > 0.6			-0.052 ***	-0.016
			(0.016)	(0.021)
(Female Leader)*(Female)	0.075 ***	0.075 ***	0.000	-0.013
	(0.008)	(0.010)	(0.009)	(0.010)
(% Female Leaders > 0)*(Female)			0.054 ***	0.057 ***
			(0.006)	(0.007)
(% Female Leaders > 0.2)*(Female)			0.065 ***	0.074 ***
			(0.008)	(0.010)
(% Female Leaders > 0.4)*(Female)			0.044 ***	0.055 ***
			(0.013)	(0.016)
(% Female Leaders > 0.6)*(Female)			0.018	0.025
			(0.018)	(0.023)
Worker and Firm Characteristics	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Industry Fixed Effects	yes	no	yes	no
State Fixed Effects	yes	yes	yes	yes
SEIN Fixed Effects	no	yes	no	yes
R^2	0.309	0.595	0.311	0.596
N	235,822	235,822	235,822	235,822

Table V
Changes of State and Industry among Displaced Workers

The table reports the estimated log odds ratios from logit regressions. In columns 1-2 (3-4), the dependent variable is an indicator variable that equals one if a worker moved to a different state (industry) four quarters following plant closure and zero otherwise. The omitted race category is "White." Age is computed as the worker age two quarters prior to plant closure. Wage is the annualized wage (defined on Table II) two quarters prior to closure. Manager is defined as the highest paid employee in the plant. Tenure is measured as the number of quarters that a worker has spent in the firm. Native is an indicator variable that equals one if a worker was born in the state in which the closing plant is located. We define diversified firms (Diversified) as firms that operate in at least two distinct two-digit SIC codes. Firm Employment is measured as the total number of workers for the entire firm (across all its plants). NPlants_SIC_County measures the number of plants located in the same industry (2-digit SIC) and county as the closing plant. All standard errors are clustered by closing plant and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

	ΔState	ΔState	ΔIndustry	ΔIndustry
	(1)	(2)	(3)	(4)
Race = Black	-0.150 *	-0.154 *	0.042	0.043
	(0.080)	(0.080)	(0.037)	(0.037)
Race = Asian	-0.373 ***	-0.361 ***	-0.065	-0.059
	(0.086)	(0.086)	(0.055)	(0.055)
Race = Hispanic	-0.455 ***	-0.443 ***	-0.095 ***	-0.087 ***
	(0.058)	(0.058)	(0.033)	(0.033)
Race = Others Minorities	-0.286 ***	-0.280 ***	-0.086 ***	-0.087 ***
	(0.059)	(0.058)	(0.027)	(0.026)
Ln(Age)	-1.132 ***		-0.471 ***	
	(0.059)		(0.036)	
Ln(Wage)	0.451 ***	0.430 ***	-0.250 ***	-0.250 ***
	(0.037)	(0.036)	(0.037)	(0.037)
Manager	0.216 **	0.202 **	0.298 ***	0.289 ***
	(0.085)	(0.084)	(0.050)	(0.050)
Ln(Tenure)	-0.342 ***	-0.348 ***	-0.239 ***	-0.241 ***
	(0.021)	(0.021)	(0.021)	(0.021)
Native	-1.119 ***	-1.110 ***	0.002	0.005
	(0.041)	(0.041)	(0.016)	(0.016)
Diversified Firms	0.398 ***	0.397 ***	0.713 ***	0.713 ***
	(0.134)	(0.134)	(0.155)	(0.155)
Ln(Firm Employment)	0.000	0.000	-0.118 ***	-0.119 ***
	(0.024)	(0.024)	(0.034)	(0.034)
Ln(NPlants_SIC_County)	-0.095 ***	-0.095 ***	-0.021	-0.021
	(0.024)	(0.024)	(0.022)	(0.022)
Female	-0.159 ***		0.041 *	
	(0.035)		(0.022)	
25 ≤ Age < 35		-0.235 ***		-0.222 ***
		(0.050)		(0.023)
35 ≤ Age < 45		-0.542 ***		-0.312 ***
		(0.057)		(0.030)
45 ≤ Age < 55		-0.669 ***		-0.364 ***
		(0.064)		(0.036)
Age ≥ 55		-0.987 ***		-0.494 ***
		(0.085)		(0.042)
(Female)*(Age < 25)		0.041		0.050
		(0.067)		(0.032)
(Female)*(25 ≤ Age < 35)		-0.107 **		0.082 ***
		(0.047)		(0.025)
(Female)*(35 ≤ Age < 45)		-0.319 ***		0.013
		(0.058)		(0.027)
(Female)*(45 ≤ Age < 55)		-0.207 ***		0.028
		(0.066)		(0.033)
(Female)*(Age ≥ 55)		-0.205 *		-0.016
		(0.122)		(0.044)
Year Fixed Effects	yes	yes	yes	yes
Industry Fixed Effects	yes	yes	yes	yes
State Fixed Effects	yes	yes	yes	yes
R^2	0.100	0.099	0.107	0.107
N	343,050	343,050	343,206	343,206

Table VI
Wage Changes among Displaced Workers

The dependent variable is the difference in the natural logarithms of the pre- and post- plant closure wage. The pre-closure wage is the annual wage (defined on table II) two quarters prior to plant closure and the post-closure wage is the annual wage four, eight, or twelve quarters following the plant closure (indicated in the Column title). The omitted race caegory is "White". Age is worker age. Wage is the pre-closure annual wage. Tenure is measured as the number of quarters that a worker has spent in the firm. All worker-level variables are measured two quarters prior to plant closure. Manager is defined as the highest paid employee in the plant. We define diversified firms (Diversified) as firms that operate in at least two distinct two-digit SIC codes. Firm Employment is measured as the total number of workers for the entire firm (across all its plants). Same New Firm indicates that the worker remains in the same firm from quarter 4 to 8 in Columns 4 to 6 and from quarter 4 to 12 in Columns 7 to 9. Standard errors are clustered by closing plant and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$\Delta Wage_{t-2,t+4}$			$\Delta Wage_{t-2,t+8}$			$\Delta Wage_{t-2,t+12}$		
Race = Black	-0.050 ***	-0.042 ***	-0.034 ***	-0.053 ***	-0.048 ***	-0.044 ***	-0.060 ***	-0.053 ***	-0.046 ***
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)
Race = Asian	0.002	-0.003	-0.014 ***	0.009	0.001	-0.013 **	0.009	0.000	-0.009
	(0.006)	(0.004)	(0.004)	(0.006)	(0.005)	(0.006)	(0.008)	(0.005)	(0.007)
Race = Hispanic	-0.039 ***	-0.032 ***	-0.031 ***	-0.051 ***	-0.043 ***	-0.039 ***	-0.062 ***	-0.050 ***	-0.046 ***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.005)	(0.005)	(0.004)	(0.006)
Race = Others Minorities	-0.014 ***	-0.015 ***	-0.014 ***	-0.011 ***	-0.012 ***	-0.012 ***	-0.006	-0.008 **	-0.005
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Ln(Age)	-0.095 ***	-0.071 ***	-0.072 ***	-0.168 ***	-0.146 ***	-0.135 ***	-0.242 ***	-0.218 ***	-0.201 ***
	(0.004)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Ln(Wage)	-0.088 ***	-0.130 ***	-0.116 ***	-0.111 ***	-0.153 ***	-0.134 ***	-0.123 ***	-0.168 ***	-0.151 ***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.005)	(0.005)	(0.008)
Manager	-0.040 ***	0.020 ***	0.026 ***	-0.021 ***	0.037 ***	0.041 ***	-0.022 **	0.033 ***	0.044 ***
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.011)	(0.010)	(0.010)	(0.014)
Ln(Tenure)	-0.005 ***	-0.005 ***	-0.015 ***	-0.022 ***	-0.021 ***	-0.025 ***	-0.026 ***	-0.028 ***	-0.033 ***
	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Diversified	-0.036 ***			-0.012			-0.017		
	(0.010)			(0.016)			(0.013)		
Ln(Firm Employment)	0.005 ***			-0.003			0.001		
	(0.002)			(0.004)			(0.003)		
Female	-0.041 ***	-0.052 ***	-0.037 ***	-0.059 ***	-0.071 ***	-0.050 ***	-0.070 ***	-0.082 ***	-0.067 ***
	(0.002)	(0.002)	(0.002)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
Same New Firm				0.109 ***	0.122 ***	0.110 ***	0.109 ***	0.127 ***	0.133 ***
				(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.007)
(Same New Firm)*(Female)				0.005	0.005	0.003	0.011 **	0.011 **	0.011 *
				(0.006)	(0.004)	(0.005)	(0.005)	(0.004)	(0.007)
Year Fixed Effects	yes	no	no	yes	no	no	yes	no	no
Industry Fixed Effects	yes	no	no	yes	no	no	yes	no	no
State Fixed Effects	yes	no	no	yes	no	no	yes	no	no
Plant Fixed Effects	no	yes	no	no	yes	no	no	yes	no
Plant - New SEIN Fixed Effects	no	no	yes	no	no	yes	no	no	yes
R^2	0.046	0.170	0.640	0.096	0.212	0.599	0.107	0.212	0.542
N	359,537	359,537	359,537	316,950	316,950	316,950	275,109	275,109	275,109

Table VII

Female Leadership and Wage Changes for Newly-hired Displaced Workers

The dependent variable is the difference in the natural logarithms of the pre- and post- plant closure wage. The pre-closure wage is the annual wage (defined on table II) two quarters prior to plant closure and the post-closure wage is the annual wage four quarters following the plant closure. The omitted race caegory is "White". Age is worker age. Wage is the pre-closure annual wage. Tenure is measured as the number of quarters that a worker has spent in the firm. All worker-level variables are measured two quarters prior to plant closure. Manager is defined as the highest paid employee in the plant. % Female Leaders is the percentage of females among the top-five earners in the worker's post-closure employer. All standard errors are clustered by closing plant and are reported in parentheses. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)
Race = Black	-0.043 ***	-0.043 ***	-0.032 ***	-0.037 ***
	(0.004)	(0.004)	(0.005)	(0.005)
Race = Asian	-0.005	-0.005	-0.016 ***	-0.015 ***
	(0.004)	(0.004)	(0.006)	(0.005)
Race = Hispanic	-0.035 ***	-0.035 ***	-0.029 ***	-0.031 ***
	(0.003)	(0.003)	(0.005)	(0.004)
Race = Other Minorities	-0.015 ***	-0.015 ***	-0.013 ***	-0.013 ***
	(0.003)	(0.003)	(0.004)	(0.003)
Ln(Age)	-0.072 ***	-0.072 ***	-0.069 ***	-0.068 ***
	(0.004)	(0.004)	(0.005)	(0.004)
Ln(Wage)	-0.145 ***	-0.144 ***	-0.128 ***	-0.139 ***
	(0.004)	(0.004)	(0.007)	(0.006)
Manager	0.031 ***	0.030 ***	0.036 ***	0.042 ***
	(0.008)	(0.008)	(0.012)	(0.011)
Ln(Tenure)	-0.007 ***	-0.007 ***	-0.014 ***	-0.014 ***
	(0.001)	(0.001)	(0.002)	(0.002)
Female	-0.060 ***	-0.057 ***	-0.040 ***	-0.045 ***
	(0.003)	(0.002)	(0.003)	(0.003)
% Female Leaders	-0.190 ***			
	(0.010)			
(% Female Leaders)*(Female)	0.045 ***			
	(0.008)			
% Female Leaders > 0.5		-0.117 ***		-0.021
		(0.007)		(0.015)
(% Female Leaders > 0.5)*(Female)		0.033 ***	0.017 **	0.021 ***
		(0.006)	(0.008)	(0.007)
Plant Fixed Effects	yes	yes	no	yes
Plant - New SEIN Fixed Effects	no	no	yes	no
New Firm Fixed Effects	no	no	no	yes
R^2	0.194	0.191	0.701	0.616
N	256,881	256,881	256,881	256,881

Table VIII

Table VIII

Female Leadership and Wage Changes for Newly-hired Displaced Workers by Age Group

The dependent variable is the difference in the natural logarithms of the pre- and post- plant closure wage. The pre-closure wage is the annual wage (defined on table II) two quarters prior to plant closure and the post-closure wage is the annual wage four quarters following the plant closure. Worker and Firm Characteristics are the control variables from Table VII: race indicators (Black, Asian, Hispanic, and Other Minorities), ln(Wage), Manager, and ln(Tenure). % Female Leaders is the percentage of females among the top-five earners in the worker's post-closure employer. All standard errors are clustered by closing plant, and are reported in parentheses. *, ** and *** represent significance at 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)
$(25 \leq \text{Age} < 35)$	0.009 **	-0.012 **	-0.002
	(0.004)	(0.005)	(0.004)
$(35 \leq \text{Age} < 45)$	-0.007 *	-0.029 ***	-0.019 ***
	(0.004)	(0.005)	(0.004)
$(45 \leq \text{Age} < 55)$	-0.028 ***	-0.044 ***	-0.034 ***
	(0.004)	(0.005)	(0.005)
$(\text{Age} \geq 55)$	-0.102 ***	-0.094 ***	-0.088 ***
	(0.006)	(0.007)	(0.006)
$(\text{Female})*(\text{Age} < 25)$	-0.067 ***	-0.065 ***	-0.057 ***
	(0.006)	(0.008)	(0.007)
$(\text{Female})*(25 \leq \text{Age} < 35)$	-0.070 ***	-0.056 ***	-0.060 ***
	(0.003)	(0.005)	(0.004)
$(\text{Female})*(35 \leq \text{Age} < 45)$	-0.055 ***	-0.033 ***	-0.038 ***
	(0.003)	(0.004)	(0.003)
$(\text{Female})*(45 \leq \text{Age} < 55)$	-0.053 ***	-0.033 ***	-0.041 ***
	(0.004)	(0.004)	(0.004)
$(\text{Female})*(\text{Age} \geq 55)$	-0.033 ***	-0.028 ***	-0.033 ***
	(0.006)	(0.007)	(0.006)
$(\text{Female})*(\text{Age} < 25)*(\% \text{ Female Leaders} > 0.5)$	0.009	-0.008	-0.014
	(0.012)	(0.017)	(0.016)
$(\text{Female})*(25 \leq \text{Age} < 35)*(\% \text{ Female Leaders} > 0.5)$	0.029 ***	0.013	0.015
	(0.008)	(0.011)	(0.010)
$(\text{Female})*(35 \leq \text{Age} < 45)*(\% \text{ Female Leaders} > 0.5)$	0.039 ***	0.017 *	0.024 ***
	(0.007)	(0.009)	(0.008)
$(\text{Female})*(45 \leq \text{Age} < 55)*(\% \text{ Female Leaders} > 0.5)$	0.044 ***	0.025 ***	0.033 ***
	(0.008)	(0.009)	(0.009)
$(\text{Female})*(\text{Age} \geq 55)*(\% \text{ Female Leaders} > 0.5)$	0.032 ***	0.025 ***	0.031 ***
	(0.011)	(0.012)	(0.011)
Worker and Firm Characteristics	yes	yes	yes
Plant Fixed Effects	yes	no	yes
Plant - New SEIN Fixed Effects	no	yes	no
New Firm Fixed Effects	no	no	yes
R^2	0.194	0.702	0.617
N	256,881	256,881	256,881

Table IX
Female Leadership and Wage Changes for Newly-hired Displaced Workers by Wage Group

The dependent variable is the difference in the natural logarithms of the pre- and post- plant closure wage. The pre-closure wage is the annual wage (defined on table II) two quarters prior to plant closure and the post-closure wage is the annual wage four quarters following the plant closure. Worker and Firm Characteristics are the control variables from Table VII: race indicators (Black, Asian, Hispanic, and Other Minorities), ln(Age), Manager, and ln(Tenure). % Female Leaders is the percentage of females among the top-five earners in the worker's post-closure employer. All standard errors are clustered by closing plant, and are reported in parentheses. *, ** and *** represent significance at 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)
(20K ≤ Wage Age < 30K)	-0.084 ***	-0.072 ***	-0.080 ***
	(0.003)	(0.004)	(0.004)
(30K ≤ Wage Age < 40K)	-0.121 ***	-0.105 ***	-0.116 ***
	(0.004)	(0.006)	(0.005)
(40K ≤ Wage Age < 50K)	-0.144 ***	-0.131 ***	-0.143 ***
	(0.008)	(0.013)	(0.011)
(50K ≤ Wage Age < 75K)	-0.161 ***	-0.143 ***	-0.156 ***
	(0.008)	(0.013)	(0.011)
(75K ≤ Wage Age < 100K)	-0.190 ***	-0.160 ***	-0.181 ***
	(0.009)	(0.013)	(0.012)
(Wage ≥ 100K)	-0.347 ***	-0.318 ***	-0.336 ***
	(0.017)	(0.023)	(0.021)
(Female)*(Wage < 20K)	-0.060 ***	-0.044 ***	-0.047 ***
	(0.003)	(0.004)	(0.004)
(Female)*(20K ≤ Wage Age < 30K)	-0.044 ***	-0.030 ***	-0.034 ***
	(0.004)	(0.005)	(0.004)
(Female)*(30K ≤ Wage Age < 40K)	-0.038 ***	-0.023 ***	-0.029 ***
	(0.005)	(0.007)	(0.006)
(Female)*(40K ≤ Wage Age < 50K)	-0.033 ***	-0.016 **	-0.020 ***
	(0.007)	(0.008)	(0.007)
(Female)*(50K ≤ Wage Age < 75K)	-0.037 ***	-0.020 **	-0.027 ***
	(0.007)	(0.009)	(0.008)
(Female)*(75K ≤ Wage Age < 100K)	-0.047 ***	-0.042 **	-0.046 ***
	(0.015)	(0.019)	(0.018)
(Female)*(Wage ≥ 100K)	-0.043 *	-0.012	-0.028
	(0.025)	(0.032)	(0.030)
(Female)*(Wage < 20K)*(% Female Leaders > 0.5)	0.030 ***	0.008	0.009
	(0.007)	(0.010)	(0.009)
(Female)*(20K ≤ Wage Age < 30K)*(% Female Leaders > 0.5)	0.034 ***	0.024 **	0.031 ***
	(0.008)	(0.010)	(0.009)
(Female)*(30K ≤ Wage Age < 40K)*(% Female Leaders > 0.5)	0.049 ***	0.034 **	0.044 ***
	(0.011)	(0.014)	(0.013)
(Female)*(40K ≤ Wage Age < 50K)*(% Female Leaders > 0.5)	0.037 **	0.037 *	0.040 **
	(0.016)	(0.019)	(0.017)
(Female)*(50K ≤ Wage Age < 75K)*(% Female Leaders > 0.5)	0.001	0.012	0.013
	(0.020)	(0.022)	(0.020)
(Female)*(75K ≤ Wage Age < 100K)*(% Female Leaders > 0.5)	-0.034	0.025	0.014
	(0.045)	(0.053)	(0.049)
(Female)*(Wage ≥ 100K)*(% Female Leaders > 0.5)	-0.039	-0.026	0.027
	(0.075)	(0.104)	(0.098)
Worker and Firm Characteristics	yes	yes	yes
Plant Fixed Effects	yes	no	yes
Plant - New SEIN Fixed Effects	no	yes	no
New Firm Fixed Effects	no	no	yes
R^2	0.185	0.698	0.612
N	256,881	256,881	256,881