Female leadership and gender equity:

Evidence from plant closure*

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Abstract

We use unique worker-plant matched panel data to measure differences in wage changes experienced by workers displaced from closing plants. We observe larger losses among women than men, comparing workers who move from the same closing plant to the same new firm. However, we find a significantly smaller gap in hiring firms with female leadership. The results are strongest among women who are displaced from male-led plants and from less competitive industries. Our results suggest an important externality to having women in leadership positions: They cultivate more female-friendly cultures inside their firms.

JEL classification: G02, J16, J31, J71, M54.
Key words: Corporate culture, Managerial style, Female leadership, Gender wage gap

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1. Introduction

Different firms, even those operating in the same lines of business, can have different sets of shared values and guiding principles. Just as individual managers play an important role in shaping the financial policies of their firms (Bertrand and Schoar, 2003), they can exert influence over the workplace culture, including wage-setting practices. Because wages, in turn, determine employee incentives, compensation policy could be an important mechanism through which managers affect firm value. A large literature in labor economics establishes the existence of gender disparity in wages. 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 US corporate chief executive officer (CEO) and top executive positions (Matsa and Miller, 2011b). We ask whether women in managerial positions create more female-friendly cultures, improving the outcomes of other women in their firms.

We use newly available worker-firm matched panel data from the US Bureau of the Census's Longitudinal Employer-Household Dynamics (LEHD) program to link gender pay disparity inside the firm with managerial style. 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.

A challenge for our analysis is the endogeneity of the allocation of jobs across gender. Because women on average have shorter expected work lives and higher job turnover rates (Gronau, 1988), they could invest less in training and other forms of firm-specific human capital than men. As a result, they could choose to work in firms in which such capital carries less of a premium. Women could also bear a disproportionate share of family responsibilities, choosing to work in firms with more flexible hours or which minimize commute times. They could be more likely to reject outside opportunities with higher wages if they are more often the secondary earner in their families and cannot move to accept a new position. In addition, men and women could differ in risk aversion (Sapienza, Zingales, and Maestripieri, 2009) or in their attitudes toward competition or negotiation (Bowles, Babcock, and Lai, 2007; Niederle and Vesterlund, 2007), causing women to shy away from risky or highly competitive industries such as investment banking. Finally, women could 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 take several steps to address these identification concerns. First, we use involuntary displacement due to plant closure as a way to address the endogeneity of job changes. If men and women voluntarily change jobs at different rates or time their job changes differently, then wage changes around the full set of job changes (or new hires) would be difficult to interpret. By measuring wage changes following job loss due to plant closure, we isolate a set of forced job changes. We use the Census Bureau's Longitudinal Business Database (LBD) to identify closures of US plants between 1993 and 2001. We 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 data set of 461,449 workers in 9,244 closing plants covering 23 states. Because LEHD wage data extend 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. Our approach also removes differences between men and women in unobserved, time-invariant skills (or preferences), which could lead to differences in wage levels. Because such differences could also affect wage changes, we control for the pre-closure wage to capture these effects.

Next, we correct for differences in the job choices of men and women by estimating a pair fixed effects model that 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 same involuntary job change). However, differences between men and women could remain even within each job change group. To address this concern, we compare the group means by gender across a host of observable characteristics such as age, race, education, and tenure. We find few economically meaningful differences. Unsurprisingly, we observe a significant within-group difference in ex ante wage levels between men and women. This difference is driven by the general gap in wages across genders. To show this, we compute the within-gender wage percentile for each worker by subtracting the mean wage for his or her gender and then normalizing by that mean wage. We do not find a significant difference between men and women in these percentiles within closing plant, hiring firm groups. Nevertheless, to correct for the effect of differences in ex ante wages, we perform a robustness check interacting fixed effects for each closing plant, new employer pair with fixed effects for categories of the ex ante wage. This specification identifies the effect of gender using only men and women who are sufficiently close together in the wage distribution, providing a less parametric correction for ex ante wage differences and ensuring, for example, that our identification does not rest on comparisons between bosses who make the same job change as their secretaries.

To conduct our main test, 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, both in the hiring unit and the overall firm. We estimate the difference in wage changes for men and women displaced from the same closing plant who move to the same new firm and then compute the difference-in-differences across workers who move to new firms that are led by female managers and those who move to firms led by male managers. We again consider a robustness check that estimates the impact of female leadership on gender wage differences comparing only workers from the same closing plant moving to the same hiring firm who are also part of the same ex ante wage category. Because our goal is to identify the impact of management on the gender wage gap, a concern is that managers are not randomly assigned to firms. Women could hold top positions in female-friendly firms or industries. Moreover, trends toward greater gender equality and changes in firm cultures over time could lead to spurious correlation between declining gender wage gaps and female leadership. We take several steps to address these concerns, including estimating models that relay on within-firm identification and that adjust explicitly for time, industry, and regional trends.

We find that displaced women experience significant wage losses of roughly 5% relative to men, but the wage gap is significantly smaller (by roughly 50%) if they are hired by female-led firms. The results are strongest for women in the middle of the age distribution and extend to women at the lower reaches of the wage distribution. The result is particularly strong if women comprise the majority of the new firm's management team. Interestingly, we find that the gender of the CEO matters in multi-division firms and not the gender of the division manager who hired the displaced worker. Moreover, we do not find significant differences if the workers are hired into the home division of the CEO or one of the firm's other divisions. These results suggest changes in culture as a mechanism instead of differences in the local interactions between female employees and female leaders, including initial wage negotiations.

We also perform a number of additional tests and robustness checks. First, we measure the gender composition of the leadership of the closing plants from which sample workers are displaced. If women in leadership roles improve the labor market prospects of female employees, then women displaced from plants with female leadership should already enjoy greater equality with their male colleagues. Then, we should expect a smaller relative wage change when they move to a new firm with female leadership. We confirm this effect in the data. The impact of female leadership on the relative wages of newly hired men and women comes entirely from workers displaced from plants or firms without female leadership.

We also provide some evidence on the mechanism by which women in power improve the outcomes of other women in their firms. One possibility is that women in leadership roles improve the productivity of women in their firms relative to men (e.g., by instituting family-friendly policies such as onsite daycare or by shifting women's beliefs about the likelihood of internal advancement) and that hiring firms with female leadership anticipate this effect in setting the wages of newly hired women. This hypothesis is difficult to test directly because we do not observe individual productivity. Moreover, it is unclear why an expected baseline productivity gap exists between men and women displaced from the same plant moving to the same firm controlling for ex ante wages and observable characteristics. Nevertheless, we find that the helping hand is most evident toward women over the age of 45, for whom family pressures are likely to be smaller, casting some doubt on this interpretation. An alternative is that women in power reduce wage discrimination. Consistent with this view, we find that the effect of female leaders in reducing the gender wage gap among displaced workers is strongest in less competitive industries, which face less market pressure to curtail suboptimal compensation practices. We also find evidence of a similar effect of black leadership on the wage deficit of newly hired black employees, suggesting commonality between the factors that drive pay differentials among women and racial minorities. Ultimately, our main message is independent of separating these potential mechanisms. Women in power exert a positive externality on the labor market outcomes of other women in their firms.

Our findings contribute to a number of literatures. Building on Kreps (1990), several recent studies confirm an empirical link between culture–measured, for example, using employee satisfaction surveys–and firm value (Guiso, Sapienza, and Zingales, 2009); Edmans, 2011; Bargeron, Lehn, and Smith, 2011). An important question, then, is how cultures are determined and how they evolve over time. We provide evidence that managers can and do redirect the corporate cultures of their firms. We also propose a novel approach to measuring firm culture, focusing on a specific policy dimension that is plausibly related to notions of organizational fairness and trust.[1] We complement existing studies by side-stepping two weaknesses of employee survey responses: (1) there are no economic stakes for responders and (2) responders are a self-selected sample.

We also contribute 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 mainly on women serving on boards of directors (Adams and Ferriera, 2009; Ahern and Dittmar, 2012; Dezso and Ross, 2011; Matsa and Miller, 2011a). Most related to our results, Matsa and Miller (2011b) 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

[1] In addition to the studies cited above, practitioners emphasize these qualities as defining characteristics of workplace cultures. In describing its criteria for identifying the *Best Companies to Work For in America*, for example, the Great Place to Work Institute emphasizes "the respect with which employees feel they are treated and the extent to which employees expect to be treated fairly" (http://www.greatplacetowork.com).

leadership on the hiring of women throughout the organization and controlling carefully for endogenous differences in job choices by gender.

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; Becker, 1985)–or due to differences in labor market treatment–the discrimination hypothesis (Becker, 1971; Aigner and Cain, 1977; 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 2009; 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. Unlike much of this literature, our emphasis is on factors that mitigate gender gaps, instead of explaining the gap itself.

The remainder of the paper is organized as follows. In Section 2, we describe the data we use in our analysis. In Section 3, 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 4, we implement the strategy outlined above to address endogeneity concerns. Finally, Section 5 concludes.

2. Data

We use worker-, firm-, and plant-level data from the US 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 [four-digit Standard Industrial Classification (SIC)] using the Longitudinal Business Database. The LBD covers all non-farm establishments with paid employees in the US since 1976. It provides information 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 data. The LEHD data are 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 US.[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 states, and, in some states, employer contributions to certain deferred compensation plans such as 401(k) plans.[3] The US Census Bureau negotiates agreements state-by-state to provide research access to UI data. 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 because the LEHD data report tax units and the LBD reports physical business establishments. The internal bridge file at the Census Bureau, 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 unique plant (from the LBD), we require that the LBD plant is unique within this partition.

[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.

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 state employer identification numbers (SEINs) 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. 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 could begin to exit a dying plant in the months preceding closure. To the extent that such exit is not random, it could 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 are 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.

Because the Census Bureau currently provides access to employment records from only 23 states in the LEHD data, we generally overstate unemployment rates in our sample. A worker could 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. Because 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.[5] Moreover, the within-sample rate of migration to a new covered state, even following plant closure, is low (approximately 2%). 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 might not provide an accurate reflection of the worker's earnings and quarterly wage changes might not reflect

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

[5] Often the constraint on allowing research access to data is preexisting state laws, suggesting that this condition is likely to hold.

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 because 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 SEIN, the main reporting unit in the LEHD data, as the individuals who have the five highest 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 15 hundred largest publicly traded US companies.[6]

In Table 1, we provide plant-level summary statistics of the data. Included are 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. Fifty-eight percent of the plants belong to multi-unit firms, and 42% are part of firms that operate in at least two distinct two-digit SIC codes. Fifty-five percent of the plants come from the 23 states covered by the LEHD data.

We construct a 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 that are unique within their firm, county, and four-digit SIC is that our matched data significantly underrepresents plants from multi-unit firms (15% compared with 49% in the closing sample). However, conditional on being part of a multi-unit firm, the fraction of plants that are part of a diversified firm is 69%, which is similar to the overall LBD sample

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

(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.

Overall, our analysis reveals some nonrandom 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 nonrandom selection of closing plants into our sample.

3. Female leadership and worker wages in a random sample

To begin, we explore the determinants of wage levels using a random sample of worker-quarters drawn from the LEHD data. In Table 2, we provide summary statistics of our sample. The average worker is 41 years old with 3.5 years of tenure in the SEIN. Women make up 46% of the workforce. Ten percent of the workforce is black; 4%, Asian; 9%, Hispanic; and 5%, other nonwhite. The mean annual wage is $35,145.

On average, roughly one of the top five highest paid workers in each included firm is a woman (mean percentage female among top five = 0.15). Forty-three percent of the firms have at least one woman among the top five 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 such as Compustat's ExecuComp database, we observe a somewhat higher frequency of women in top positions than prior studies. However, racial minorities are rare in these positions. In particular, only 12% (5%) of firms have more than one black (Hispanic) worker among their top five earners.

We also provide summary statistics for the subsamples of male and female workers. We observe two notable cross-sample differences: (1) mean annual wages for women are $14,222 lower than for men and (2) women appear to sort more frequently into firms with female leadership. Twenty percent of women work in plants with female leadership, but only 11% of men do.

To establish the baseline effect of gender on wages in our sample, we regress the natural logarithm of annual real wages on gender, race (broken into indicators for black, Hispanic, Asian, and other nonwhite workers), the natural logarithm of tenure, the natural logarithm of age, education, and indicators for whether the worker is foreign and for whether the worker is native to the state in which his or her plant is located.[7] We also control for firm size (the natural logarithm of aggregate firm employment) and include an indicator for diversified firms (i.e., firms that operate in at least two distinct two-digit SIC codes). Finally, we include state, two-digit industry, and year fixed effects. We cluster standard errors at the SEIN level. We report the results in Column 1 of Table 3.

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 nonwhite workers separately from white workers, resulting in different comparison groups. We find that foreign workers earn significantly lower wages. 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 were born in the state in which they currently work earn significantly lower wages, suggesting a premium to mobility. Finally, we find that women earn roughly 29.7% less than men, which is somewhat larger than the 22% gap estimated by Altonji and Blank.

Having confirmed the similarity of our sample to standard data sources, we turn to the effect of interest. In Column 2, we reestimate the specification from Column 1, but include the percentage of women among the top five earners in the worker's SEIN and its interaction with the gender indicator as additional independent variables. We exclude workers who are themselves among the top five earners in the SEIN. We find a strong

[7] 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 it is not obvious why the problems with the variables would be correlated with gender).

and significant positive effect of female leadership on the relative wages of women. Adding a 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 4.5 percentage points). At the limit, a firm with 100% women among its top five earners would have a mean gender wage gap of roughly 8%.

In Column 3, we test whether having a woman as the top earner in the SEIN has a significant impact on the wages of other women in the SEIN. We reestimate the regression specification from Column 2, but replace the percentage of women among the top five 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 other women in the firm. Having a female leader reduces the gender gap in the firm by 5.6 percentage points (or by roughly 20%). In Column 4, we estimate the marginal effects of each additional female leader in the SEIN. We report the results of a specification that estimates separate marginal effects for each 20% increment in the percentage of females among the top five earners and also for a woman as the top earner. We find a positive and significant marginal effect for each woman added to the top five earners, starting from the first woman and continuing through the third woman. However, once women make up the majority of the top five earners, the marginal effect of adding a woman is small and statistically insignificant. 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. Our results are consistent with the idea that one woman in a leadership role is not sufficient to redirect the organizational culture, but that a critical mass of women can affect such changes.[8]

We also conduct a number of robustness checks on this evidence. First, we reestimate all regressions including SEIN fixed effects, using within-firm variation to identify the gender wage gap. The results are nearly identical.[9] We also include

[8] McKinsey and Co. provides survey evidence for European companies consistent with this idea. It finds that firms with three or more women on management committees score higher on its scale of "organizational excellence." The same report finds evidence of a positive correlation between female representation on management teams and firm value, suggesting a potential channel from culture changes to value (Desvaux, Devillard-Hoellinger, and Baumgarten, 2007).
[9] Tabulated estimates are available upon request.

additional controls for the overall percentage of female employees in the SEIN and its interaction with the female indicator. These specifications confirm that the effects we attribute to female leadership are not instead due to a high overall percentage of female employees.

4. Female leadership and wage changes among displaced workers

In Section 3, we establish a negative correlation between female leadership and the gender wage gap. However, it is difficult to assess causality due to the nonrandom allocation of workers to firms. Differences in wages across employees can reflect differences in workers' career choices or uncontrolled variation in worker productivity. In this section, we reestimate the impact of female leadership on pay differences between men and women, but following a more careful identification strategy.

4.1. Empirical specification and identification strategy

A key step in our strategy is to identify a set of involuntary job changes. By doing so, we sidestep differences between men and women in the timing of job changes or in the rates of voluntary versus involuntary moves. We follow Gibbons and Katz (1991), focusing on the subset of workers (exogenously) displaced as a result of plant closures. An added advantage of this approach is that wage changes implicitly remove a time-invariant individual effect on wages. This is important to the extent that differences exist in unobservable quality across workers that we cannot capture with our set of observable controls.[10] Moreover, focusing on job changes enables us to control for the pre-job change wage as a way to capture these differences in our main tests.[11]

[10] This concern is exacerbated in our sample because we do not directly observe worker education. Though we control for imputed education, a portion of the measured difference in wage levels across men and women could be explained by measurement error. Men could 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 because 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.

[11] 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.

In Table 4, we present summary statistics of the sample of workers displaced by plant closures in our LBD-LEHD matched data. Relative to the random set of workers summarized in Table 2, the mean worker is one year younger (39.7) and women make up a 5 percentage point smaller portion of the workforce (41%). Most noticeably, mean wages are smaller ($29,933), likely reflecting the smaller plant size in the matched sample (Table 1; see also Section 2). 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 that close plants. On average, one of the top five managers is a woman. Twenty-five percent of the firms have at least two female managers, and 12% of the firms have more than two female managers. We also break out the sample by gender. The patterns are similar to the random sample. Finally, we report two additional statistics related to the labor market reentry decisions of displaced workers. We find that roughly 2% of workers find a new job in a different state and 33% in a different two-digit SIC from their former job. In both cases, the frequencies are significantly smaller among women.

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 could have different preferences over career paths and working environments. For example, women could prefer flexible hours to accommodate family demands outside of the workplace. They could 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 could lead to differences in job choices ex ante or ex post. These differences, in turn, could be correlated with the gender composition at the top of the firm. An advantage of our data relative to alternative sources such as 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 difference-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 possible concern with this approach is that by focusing on relatively small groups of workers who make the same ex ante and ex post job choices, we base our results on comparisons of workers who are different at the individual level. For example, a boss and secretary could move together from a closing plant to a new employer. Though we always control for ex ante wages, our estimates in this case might rely heavily on extrapolation and be sensitive to functional form. As a first step to assess the empirical relevance of this concern, we report the means of within-group averages of observable characteristics by gender in Table 4. By construction, our approach removes the significant differences across men and women in the probability of geographic or industry migration and of working for a firm with female leadership. We also see that men and women within job change groups appear, if anything, more similar along other observable dimensions, including ex ante wages, than random men and women from closing plants or from the working population at large. Moreover, most of the differences across characteristics are economically small. An unsurprising exception is ex ante wage levels, which is of particular concern to the degree that this difference proxies for differences in unobservable quality. However, we do not see differences between men and women within job change groups in their positions in the gender-specific wage distribution. We compute the wage percentile, separately for each gender, by subtracting the mean wage for workers of that gender from each worker's wage and normalizing by the gender-specific mean wage. After this adjustment, the difference within groups between men and women completely disappears. Despite this evidence, we continue to control for observables that vary within groups in our regression analyses, including ex ante wages. We also return to this issue in the regression context, taking an alternative approach to address the difference in ex ante wages.

Another potential concern is that the type of women who accept jobs at firms with female leadership is different from the type of women who join male-led firms. For example, women who are hired by female executives could have higher quality than women hired by men. If so, a finding of better relative wage performance could reflect uncontrolled differences in quality instead of an effect of female leadership. To assess the degree of sorting in our sample, we construct ex ante worker-level summary statistics, separately for women (and men) hired by female- and male-led firms (Table 5). We use the presence of a female top earner in the hiring firm to measure female leadership. We find that the women hired in the two types of firm look similar along most observable dimensions. Only one cross-group difference is significant: annual wages. However, the women hired into female-led firms are on average lower paid ex ante than the women hired into male-led firms. Thus, the evidence does not suggest that the women who sort into female-led firms are of higher quality on average. Instead, worker sorting appears to work against our main hypothesis. We reach similar conclusions when we interact the observables with the gender indicator in a linear probability model using an indicator for a female top earner in the hiring firm as the dependent variable. In the online Appendix, we provide a table with these results. We also repeat the analysis using the presence of a majority of women among the hiring firm's top five earners to measure female leadership and find similar results.

4.2. Female leadership and worker wage changes

Next we implement our main identification strategy by measuring the effect of female leadership in the hiring firm on the relative wage changes of men and women displaced from their jobs by plant closures.

4.2.1. Baseline effect of gender on wage changes

We begin by establishing the baseline difference in wage changes for displaced men and women. Table 6 reports the estimates from an ordinary least squares (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 reenter the workforce by quarter t+3. As controls, we include the set of race indicators from Table 3, the natural logarithm of age, the natural logarithm of tenure in the closing plant, the natural logarithm of the pre-closure wage, and an indicator for the top earner in the closing plant (or manager).[12] We cluster standard errors

[12] We do not include the imputed education control in these regressions or our indicators for foreign workers or 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 (or, later, the effect of female leadership), consistent with the effectiveness of pre-closure wages as a sufficient statistic for unobserved differences across workers.

at the plant level. We report two comparisons: In Column 1, we include a plant fixed effect in the regression, isolating differences in the wage changes of men and women displaced from the same plant. In Column 2, we include fixed effects for the closing plant, hiring division pair, isolating differences among men and women who make the same job change. In both cases, it is no longer necessary to control for firm-level characteristics such as size or diversification because the data contain each closing plant only once and, therefore, these differences are captured by the fixed effects.[13]

We find that women experience a significant 4% to 5% decline in wages relative to men. 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 because 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.

We find that the baseline losses experienced by displaced women relative to men are robust and persistent. In untabulated analyses, we partition the age and wage distributions and allow for differences in the wage gap across groups.[14] 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 five years prior to closure. We also consider the outcomes of workers who reenter the labor force one or two years after plant closure. The difference in wage changes between men and women substantially increases as the length of the unemployment spell increases. Thus, our reported results provide a conservative measure of the impact of gender on wage changes.

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[13] We also run a specification with only industry, state, and year fixed effects finding largely similar results. We omit this specification from the tables for brevity.

[14] We use the same groupings as in Table 7. Tables are available upon request.

We also reestimate the regressions from Columns 1 and 2, but using the two- and three-year wage change as the dependent variable for workers who are reemployed by quarter t+3 and do not make any additional job changes after reentering the workforce. Thus, we isolate the continuing change in wages for the workers we study in Table 6. We find that the qualitative patterns from the first set of regressions continue to hold. The difference in wage changes between men and women modestly increases in the second year, but remains relatively flat over the third year.[15] Thus, the initial difference in the shock to wages among women does not appear to reverse over time.

An advantage of our setting relative to prior studies is that it is challenging to interpret the differences between men and women as differences in expected productivity. We find that men and women (exogenously) displaced from the same plant and hired within three quarters by the same new firm experience different changes in wages. Moreover, this is true correcting for the (small) differences in observables such as age and tenure in the closing plant (see Table 4) as well as the pre-closure wage.

A possible concern is that men with similar pre-closure wages and experience in the closing firm could differ from women in accumulated firm-specific capital. However, in this case, we would expect larger wage losses among men than among women who make the same job change, to the degree that the capital does not transfer to the new firm. A second possibility is that men and women differ in the terms at which they are willing to reenter the labor market following displacement. For example, women could be 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 reenter the workforce following plant closure. We estimate a slightly higher reentry rate in the first year among women. However, women are also less likely to change states following plant closure (Table 4). So, even this near-zero effect is confounded by the possibility that men more often move to states that 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 toward the (higher) wages of men over time. We instead find the

[15] 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.

opposite. The relative losses of women increase over the two years following the acceptance of a new job. Then, to explain our results, women must be different along some other unobservable dimension uncorrelated with observables and prior wages, but negatively correlated with expected future productivity. Even if this is the case, our focus is not on the baseline gap in the outcomes of men and women, but on the potential externality provided by female managers to (newly hired) women in their firms.

4.2.2. Female leadership in the hiring SEIN

In Table 6, we estimate the impact of female leadership on the relative wage changes of displaced men and women. To measure the prevalence of women in top positions of hiring firms, we adapt our strategy from Section 3. First, we compute the percentage of women among the top five earners in the hiring SEIN in the year prior to hiring workers from a closing plant. In Column 3 of Table 6, we reestimate the Column 2 regression, but including the percentage of women in the hiring firm's top five positions and its interaction with the gender dummy as additional independent variables. We continue to find that women fare worse than men following plant closure. The one-year wage change is 4.1 percentage points lower for women, a difference that is significant at the 1% 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 2.6 percentage points larger wage loss than her male colleagues.[16]

As in Section 3, 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 five positions greater than 50%. In Column 4 of Table 6, we reestimate the regression from Column 3 with this alternative explanatory variable. We find that the wage losses among women are cut by nearly 50% among firms with a majority of women in the top leadership roles. The statistical significance of our estimates increases, suggesting that the results using the raw

[16] Our results are stronger if we compare all workers from the same closing plant (i.e., including only closing plant fixed effects) due to greater power, but at the cost of potential uncontrolled heterogeneity of hiring divisions. For example, women leaders are more common in divisions that generally pay lower wages, an effect we remove with the pair fixed effect.

percentage of women on the management team are driven more by the higher end of the distribution than by comparisons of divisions with one woman in a leadership role with those with no women in power. This result again suggests the importance of a critical mass of women in leadership positions.

We also consider an alternative strategy to control for differences in pre-closure wages between workers within closing plant, hiring firm groups. Instead of controlling for pre-closure wage levels, we split workers into five groups depending on their pre-closure wages: less than $20,000, $20,000-$40,000, $40,000-$60,000, $60,000-$100,000, and above $100,000. We then interact dummies for each group with the pair fixed effects and re-estimate the regression specification from Column 4. Thus, we estimate the impact of female leadership on the relative wage change among women comparing only men and women within each wage group who move from the same closing plant to the same new employer. Despite the loss of power, our main result is largely unaffected. Our findings are robust to using alternative wage groupings (e.g., four wage groupings with cutoffs at $25,000, $50,000, and $100,000 or seven groupings with cutoffs at $20,000, $30,000, $40,000, $50,000, $75,000, and $100,000). Thus, our main results do not depend upon comparisons of men and women who are far apart in the wage distribution.

One challenge we face in interpreting our results is the endogenous sorting of managers to firms. Some firms could have existing cultures that are more friendly toward women and those firms could also be more likely to promote or attract female managers. In Column 5 of Table 6, we report the results from a regression including separate closing plant and hiring SEIN fixed effects. Thus, we estimate the impact of female leadership on the wage differential paid to newly hired displaced workers using only variation within the hiring firm. We again 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.

In untabulated regressions, we also allow for different gender pay gaps in each hiring firm by interacting the hiring SEIN fixed effects with the female worker indicator. Despite having limited data to identify such effects, we find qualitatively similar results. The point estimate for the effect of female leadership on the gender wage gap is slightly larger than what we observe in Column 5, though the statistical significance depends on how we cluster the standard errors. Overall, it does not appear that our results can be explained by women sorting into firms with female-friendly cultures. Instead, women in power make different decisions on how to compensate new hires than men running the same firms.

4.2.3. Robustness checks

In this subsection, we describe several robustness checks of our main finding. In the online Appendix, we provide additional details of the analyses as well as full tables containing the estimates described below.

Time trends. A potential confounding factor is the possibility of coinciding trends in female leadership and the wage gap between men and women. In particular, increasing incidence of female leadership over time coupled with a decline in the gender gap could generate a spurious result. We would be more likely to measure female leadership in later sample years in which the gender gap is also smaller, even if there is no causal link between the two phenomena. We take several steps to address this concern. First, we reestimate our main specification (Column 4, Table 6), but include an interaction of year fixed effects with the gender indicator. We find that the result is virtually unchanged (coefficient = 0.017; standard error = 0.006). We also consider the possibility that time trends in the wage gap might differ across industries or states. Our results are also largely unaffected if we include interactions of industry-year dummies or state-year dummies with the gender indicator (estimates are 0.018 and 0.015, significant at 1% and 5%, respectively). Finally, we include separate time, industry, and state dummies interacted with the gender indicator, again with little impact on our results (coefficient = 0.014; standard error = 0.007). Thus, the effect of female leadership on the pay gap cannot be explained by the concentration of female-led firms in particular times, regions, or industries in which men and women happen to be paid more equitably.

Common shocks to leadership and wage gaps. Even if female managers provide more generous relative wages to newly hired women than male predecessors or successors in their firms, it is possible that these differences reflect time-varying changes in firm culture rather than leadership-driven initiatives. For example, a discrimination lawsuit could cause a firm both to initiate a change to a more female-friendly culture and to replace male leaders with female leaders.

One way to assess the importance of this kind of change is to ask whether changes from male to female leadership (and not vice versa) drive our results. Although we do more often observe changes from majority male to majority female leadership than in the other direction, the magnitude of the difference is small (52.8% versus 47.2%). More important, we do not find any evidence that the effect of female leadership on the relative wage change of female workers is different when female leadership follows male leadership.

More generally, if the link between female leadership and the wages of newly hired men and women is not causal but is driven by exposure to a common shock, then we would expect the existing workers in the hiring firms to enjoy similar wage changes. To investigate this possibility, we construct a sample of workers from the hiring firms and examine the changes in their wages around the time when displaced workers are hired. To keep the sample size manageable (roughly 5.8 million workers), we restrict our attention to firms that either hire both male and female displaced workers or have male and female leadership spells (i.e., the firm must contribute to the identification of either the gender effect or the female leadership effect in our hiring firm fixed effect specification). We do not find evidence that female leadership is associated with relative wage gains by women in the hiring firms' existing workforces either before or contemporaneously with the hiring of the displaced workers. Thus, no evidence exists of widespread culture changes occurring within the hiring firms at the time of the shocks we use for identification. We identify worker wage changes around external plant closures, shocks that neither occur inside the hiring firms nor appear to have a direct impact on those firms.

We go a step further and look at the changes in the wages of workers inside the hiring firms around the changes in the gender composition of firm leadership in our sample. This is nearly always a different time from when the displaced workers were hired. We find that female workers do not enjoy wage gains relative to male colleagues in the year preceding the leadership change (there is no significant difference). Moreover, we do not see a contemporaneous increase in women's relative wages when the female leader is hired. Interestingly, we do find a general increase in wages of roughly 3 percentage points (significant at 1%). Thus, firms could hire female managers at times when they are generally moving to more labor-friendly regimes [consistent with recent work suggesting that female managers are associated with labor-friendly policies (Matsa and Miller (2011a)], but little evidence shows that they do so specifically to create female-friendly cultures.

As a final step, we test whether the effect of female leadership on the relative wage changes of displaced workers is concentrated in the initial years of the female leaders' tenures. We find instead that the effect of female leadership is, if anything, increasing with the leaders' tenure (the estimates are generally positive, though never statistically significant). This finding, again, suggests that the relative wage improvements of women moving to firms with female leadership reflect the preferences of the female leaders. Our identification comes from differences in how male and female leaders treat newly hired employees many years into their tenure, not differences in the trends of existing workers in the firm around the time of the leaders' promotions. Nevertheless, it is ultimately difficult to isolate fully the effect of leadership from (changes in) the underlying culture of the firm. At the very least, we provide novel insights into the evolution of firm culture and its relation to management changes.

Additional controls. We perform several additional untabulated robustness checks of our findings to separate the effect of female leadership from potential contaminating factors. We include the interaction of the overall fraction of women in the hiring SEIN and the gender dummy to distinguish the impact of leadership from the impact of female-dominated workforces. We include additional controls for whether the worker is foreign and whether the worker is native to the state in which his or her closing plant is located. In all cases, the results are nearly identical to those reported above.

4.2.4. Effects across the wage and age distributions

Next, we examine whether the impact of female leadership is uniform for women across the age and wage distribution. In Table 7, we report the results of reestimating the specifications in Columns 4 and 5 of Table 6, 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, between 25 and 35, between 35 and 45, between 45 and 55, and 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 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.[17] 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. Generally, productivity-based explanations for the wage gap between men and women rely on heightened family responsibilities among women. These can lead, for example, to different job choices, shorter work hours, and smaller investments in firm-specific capital. Though our identification strategy already corrects for many of these factors (see Subsection 4.1), the results of the age breakouts also suggest an additional mechanism at work. Though female leadership could increase women's relative wages by increasing expected productivity (e.g., by establishing flexible work hours or on-site day care), baseline productivity deficits are likely to be minimized for women above age 35 for whom family pressures, on average, should be the least. Thus, our results suggest that one mechanism through which female leaders improve the welfare of female workers is by removing the effects of discrimination on wages. We return to this issue in Subsection 4.4.

Also in Table 7, we consider instead the wage distribution, again dividing workers into five groups: less than \$20,000, \$20,000-\$40,000, \$40,000-\$60,000, \$60,000-\$100,000, and above \$100,000. We find that women in power 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 \$60,000 annually. In this portion of the distribution, we do not observe a statistically significant gender wage gap among hiring firms with a majority

[17] The estimates of the controls are not materially different from the estimates reported in Columns 3 and 4 of Table 6.

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.[18] Relatively fewer women earn these high salaries and relatively few firms have majority female leadership. Even our weakest identification strategy requires sufficient observations in the intersections of these sets.

4.2.5. Division- versus firm-level leadership in multi-division firms

So far, we see that SEINs with predominantly female leadership extend more equitable initial wage offers to displaced men and women. We have not distinguished between SEINs and firms even though many large firms have multiple SEINs, or divisions. In our data, roughly 60% of firms have multiple units (Table 1). An interesting question is whether the effect of female leadership on women's relative wages extends to the top management of multi-division firms. If the effect comes from a greater ability to discern information about female employees or more effective negotiations with them at the time of hiring, then we might expect the effects to exist primarily at the local level inside the firm. CEOs and upper-level management are unlikely to have any personal contact with lower-level employees in remote divisions. If the effects come from culture shifts inside the organization, then upper-level managers could be even more important than their division-level counterparts.

Mirroring our approach at the SEIN level, we construct two measures of female leadership: an indicator for whether more than 50% of the top five earners in the firm are female and an indicator for whether the top earner (or CEO) is a woman. To minimize measurement error, we restrict our analysis to firms for which we observe all divisions in the LEHD worker-level data. This means that all plants of the firm must be located in one of the 23 LEHD-covered states. The resulting sample consists of 160,642 of the 256,881 workers from the sample in Table 6.[19] Interestingly, we find that having a majority of female top earners at the firm level is highly correlated with having a majority of female top earners at the SEIN level, even in multi-division firms.[20] Thus,

[18] Note the very large standard errors on our estimates in these categories.
[19] Because each closing plant is in a covered state and workers rarely change states (even when displaced), this restriction mainly eliminates workers who move to large multi-unit firms that span uncovered states.
[20] The measures coincide for single-division firms, which make up roughly 40% of the sample.

when we reestimate the regressions from Panel B of Table 6, but using firm-level measures of female leadership, we find similar (and generally somewhat stronger) results.

In Table 8, we report estimates of the impact of female top earners (or CEOs) on the difference in the wages of newly hired men and women. We adapt the main specifications from Table 6. In Column 1, we regress wage changes among displaced workers on our usual set of controls, a gender indicator, and the interaction of the gender indicator with an indicator for a female CEO of the hiring firm. We include fixed effects for the closing plant, hiring SEIN pair. We find that the gap between the wages of newly hired men and women is roughly half as large when they move to a firm with a female CEO. In Column 2, we include closing plant and separate hiring SEIN fixed effects, finding similar results. We perform several robustness checks on the evidence. First, we interact the closing plant, hiring SEIN pair fixed effects with fixed effects for five wage categories (as defined in Subsection 4.2.2). We again find a statistically and economically strong effect. Female CEOs cut the gap in women's wages nearly in half (coefficient estimate = 0.16, significant at the 5% level). Second, we find that the effects of female CEOs cannot be explained by time trends (overall or within states or industries).[21] Moreover, these results are distinct from the impact of having a majority of women in the top five earners.[22] Thus, a woman in the top position within a firm is particularly important and supplements the impact of a critical mass of women on the management team.

Given the importance of female CEOs on the relative wage changes of displaced women, we ask whether female divisional CEOs also exert a significant influence on the wages of newly hired women in multi-division firms. To answer this question, we re-estimate the regression from Column 1 (i.e., including closing plant, hiring SEIN pair fixed effects) separately on the subsample of single-division firms and multi-division firms. For single-division firms, the definitions of divisional and overall CEOs coincide. We find on this subsample (Column 3) that female CEOs in the hiring firm decrease the gap between the wages of men and women. For multi-division firms (Column 4), we

[21] In the online Appendix, we report tables from this and other robustness checks from Subsection 4.2.3, but using a female CEO to measure female leadership.

[22] In unreported regressions, we estimate specifications including both measures of female leadership. We find that there are distinct effects. If anything, the impact of a female CEO appears to be the stronger factor.

include separate indicators for firm- and division-level female CEOs interacted with the gender dummy. We see that only firm-level CEOs matter, virtually erasing the gender wage gap. There is no impact of divisional female leadership on the gender wage gap in multi-division firms.[23] Thus, we uncover important differences between the roles of managers at different levels of the organizational hierarchy. Most crucially, the overall head of the firm appears to have the most important impact on gender differences in wages. These results suggest that the importance of female leaders for the outcomes of other women in the firm do not stem from personal relationships or information, but instead from their role in establishing firm-wide policies or culture, or both. As an additional test of this inference, we consider separately workers who are hired into the firm-level CEO's home division versus workers hired into peripheral divisions of the firm. We do not see a stronger effect of female leadership for women in the former set.

4.2.6. Female leadership in the closing plant

Thus far, we see significant impacts of female leadership in the hiring firm on the relative wages of newly hired men and women. Next, we ask whether the gender composition of the leadership team in the closing plant or its parent firm matters for the relative wage changes experienced by the displaced workers. We find strong evidence that women who originate in female-friendly firms face fewer obstacles in the job market than women in male-led firms.

We partition the set of displaced workers based on the gender composition of the top five earners in the closing plant. In Column 1 of Table 9, we estimate the impact of a female CEO in the hiring firm on the relative wage changes of men and women displaced from a plant with a majority of women in the top five earners. In Column 2, we consider the complementary set of workers displaced from plants with a minority of women in leadership roles. In both regressions, we exclude the top five earners themselves and include closing plant, hiring SEIN pair fixed effects.[24] We find a smaller gender wage gap and do not find an impact of female leadership in the hiring firm for workers displaced from a plant with female leadership. However, female leadership in the hiring

[23] We do not find an impact of female division-level CEOs even when we estimate the effect separately, without including an indicator for a female firm-level CEO.

[24] Though we tabulate only specifications with closing plant, hiring SEIN pair fixed effects, the results are robust to including separate closing plant and hiring firm fixed effects.

firm mitigates relative losses among women who are displaced from male-led plants. In Columns 3 and 4, we divide the sample based on the gender of the CEO of the ultimate owner of the closing plant. We find similar results. Women are more disadvantaged relative to men when they originate in male-led firms, but enjoy a helping hand from female-led hiring firms. Thus, female leadership at the original employer also appears to confer benefits on female employees.

4.3. Female leadership and the quantity of female workers hired

Given the apparent advantages enjoyed by women who obtain employment in female-led firms relative to peers who work in male-led firms, a natural question is whether women sort into such firms in larger quantities. To answer this question, we estimate a linear probability model using an indicator for whether a worker joins a hiring firm with a female CEO as the dependent variable.[25] We continue to examine the sample of workers displaced by plant closure. We include the same set of controls as in our wage models: indicators for race categories, the natural logarithm of age, the natural logarithm of tenure, an indicator for whether the worker is the manager of his or her prior employing plant, and the natural logarithm of the wage in the worker's prior job. The independent variable of interest is an indicator for female workers. In all cases, we cluster standard errors at the closing plant level. In Column 1 of Table 10, we report the estimates from a specification that includes closing plant fixed effects. Thus, we measure differences in the outcomes of men and women with the same ex ante employment, displaced by a common shock. We find that women are significantly more likely to move to a hiring firm with a female CEO. The effect is also economically meaningful. The baseline rate at which workers move to female-led firms is roughly 13% in the sample. We estimate that the rate among women is 3.4 percentage points higher, an increase of roughly 26.5%. In Column 2, we add fixed effects for the industries in which hiring firms operate, measured by two-digit SIC codes, finding similar results. Thus, the leadership effect is distinct from differences in how men and women sort into industries.

[25] The results are stronger using an indicator for a majority of women among the hiring SEIN's top five earners as the measure of female leadership. Estimates for the effect of female gender on the likelihood of joining a female-led firm are positive and significant, even for a specification including hiring firm fixed effects. See the online Appendix for a full table.

Together with our earlier results on relative wage changes, the larger quantities of female workers hired by female-led firms support a demand-side interpretation of the evidence. Suppose that the patterns we observe among displaced workers are driven by a shock to labor supply (i.e., displaced women apply to female-led firms at higher rates due to a preference to work in female-led firms). Given that labor demand is downward-sloping (firms demand fewer workers as wages rise), the (larger) increase in female workers available to female-led firms should drive the price down and we would expect to see that women hired into female-led firms would do relatively worse than women hired into male-led firms. Because we find that such women perform better, our results are more consistent with a higher demand for female workers among female-led firms. An outward shift in demand increases both quantity and price (assuming upward-sloping labor supply). This finding is consistent with our main hypothesis that women in leadership roles prefer to cultivate more inclusive corporate cultures.

4.4. Discrimination and the impact of female leadership

A remaining question is why women in leadership roles appear to have a stronger preference for hiring female workers. In the remainder of the paper, we conduct tests intended to shed additional light on the mechanism by which female leadership improves the outcomes of other women in the firm. A possibility is that women in power improve the expected productivity of women hired into the firm. Given our prior results, it is unclear why the men and women we compare would have different baseline expected productivities. Nevertheless, female CEOs, in particular, could matter by instituting family-friendly policies (such as on-site day care or flexible work hours), which increase women's productivity. Another possibility is that women in power reduce discriminatory hiring practices.[26] In this subsection, we attempt to provide some evidence of the latter mechanism. However, to the extent that we cannot separate the potential mechanisms, our finding that women in power improve outcomes among other women continues to have important policy implications.

[26] A gray area also exists in between these possibilities. For example, women could feel more comfortable in working environments with other women, taking more of an active role. However, our results are strongest for women in the CEO position. Moreover, they are entirely distinct from the impact of having a higher overall percentage of female employees. Thus, this story seems difficult to reconcile with our evidence.

We consider the competitiveness of the labor markets in which the closing plants operate. We measure competitiveness of the industry by constructing a Herfindahl index of employment across firms at the two-digit SIC level. Because industry changes are themselves costly for workers (Neal, 1995; Tate and Yang, 2011), firms operating in concentrated industries have greater discretion to set wages that do not optimize worker incentives (or, as a result, firm value). Conversely, firms in highly competitive labor markets face the prospect of worker exit if they do not offer optimal wages. We divide industries into quartiles based on their annual Herfindahl indices. We then reestimate the impact of female CEOs in hiring firms separately for workers displaced from closing plants in each industry quartile. Because we include closing plant, hiring SEIN fixed effects, we do not have to worry about male and female workers making industry changes at different rates following displacement (all workers in each closing plant, hiring SEIN pair make the same choice by construction). Consistent with the hypothesis that female CEOs mitigate discrimination in wage setting, we find the strongest impact of female CEOs on the relative wages of women displaced from closing plants in concentrated industries (Table 11).

We also 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 3 and 6) 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 five earners. Using our difference-in-differences framework, we find that black workers experience a significantly bigger wage loss compared with their white coworkers, 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.[27] 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

[27] These estimates are untabulated, but are available upon request. We do not find an impact of female leadership on racial wage gaps.

other candidate explanations for a wage discount among women–related, for example, to childbirth and family responsibilities–are unlikely to generate corresponding wage gaps for racial minorities.

5. 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 program and the Longitudinal Business Database, 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 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 difference-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 that 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 4-5% compared with men. The difference is persistent and exists throughout the age and wage distributions. However, the gap is significantly reduced when women hold positions of leadership in the hiring firm. The latter result survives a number of robustness checks designed to isolate the effect from cross-sectional differences in underlying firm cultures and secular trends in the labor market.

We also find important differences in the impact of leadership at the divisional and firm levels. Though a critical mass of women in leadership roles appears to matter at the division level, we find a high correlation of the prevalence of women in these roles and the prevalence of women in the top five leadership roles of the overall firm. Though a female division leader does not appear to affect the wage gap between men and women, female CEOs have a strong and robust impact. Thus, our results suggest that leadership-driven changes in overall firm culture could be a more important mechanism for reducing gender wage differences than the personal relationships between workers and their bosses.

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 could be a mechanism to change the culture of the firm in a direction that is friendlier to female workers (or other workers impacted by labor market discrimination). And, recent gains by women in representation on corporate boards could have important spillovers to other women in those firms. Moreover, if differences in the treatment of men and women in the labor market reflect (implicit) employer tastes instead of expected differences in productivity, these changes could improve firm value by removing distortions in worker incentives. More generally, our results identify a novel channel through which managerial style can affect performance. Prior research identifies style effects on performance, but focuses on differences in corporate policies such as investment or leverage as potential mechanisms. We propose a broader and potentially more pervasive mechanism: Managers can redirect the cultures of their organizations, affecting the incentives and productivity of existing employees as well as the attractiveness of the firm in the labor market.

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Table 1

Summary statistics: plant level

The table reports summary statistics of a random sample of closing plants from the US Bureau of the Census's Longitudinal Business Database (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 Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. The table also reports the corresponding statistics for the subsamples of plants from multi-unit firms. We define multi-unit firms as firms that operate at least two distinct plants. Standard deviations are reported in parantheses for continuous variables. * denotes subsamples on which we cannot report distributional statistics due to disclosure risk (some partitions contain too few firms). SIC = standard industrial classification.

	All firms			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 thousands of dollars)	\$6,830	\$5,299	\$2,333	\$7,590	\$6,676	\$3,703
	(\$383,230)	(\$66,606)	(\$6,709)	(\$178,102)	(\$92,809)	(\$9,611)
Percent of multi-unit firms	0.58	0.49	0.15	—	—	—
Percent 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	*
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	—
Northeast	0.22	0.22	0.08	0.21	0.22	0.09
Midwest	0.25	0.21	0.16	0.25	0.22	0.18
South	0.23	0.24	0.23	0.24	0.24	0.26
Southwest	0.12	0.13	0.19	0.12	0.12	0.19
West	0.14	0.16	0.29	0.14	0.15	0.22
Rocky Mountains	0.04	0.03	0.05	0.04	0.03	0.06
Yearly distribution						
1994	0.10	0.08	0.08	0.10	0.07	0.05
1995	0.11	0.08	0.10	0.10	0.08	0.07
1996	0.11	0.11	0.12	0.11	0.11	0.13
1997	0.11	0.10	0.09	0.11	0.10	0.07
1998	0.11	0.11	0.13	0.12	0.11	0.12
1999	0.12	0.12	0.12	0.12	0.14	0.10
2000	0.12	0.12	0.14	0.12	0.13	0.22
2001	0.12	0.21	0.14	0.12	0.17	0.17

Table 2

Summary statistics: random sample of workers

This table reports summary statistics for a random sample of workers from the US Bureau of the Census's Longitudinal Employer-Household Dynamics (LEHD) data. Annual wage is the mean real wage over the preceding four quarters multiplied by four. To be included in the annual wage computation, a quarter cannot be the worker's first or last quarter in his or 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 five earners in the worker's state employer identification number (SEIN).

	Overall			Men			Women		
	N	Mean	Standard deviation	*N*	Mean	Standard deviation	*N*	Mean	Standard deviation
Worker characteristics									
Annual wage	235,822	35,145	92,402	127,405	41,683	102,060	108,417	27,461	76,086
Age	235,822	41.34	11.10	127,405	41.37	11.14	108,417	41.30	11.07
Tenure (years)	235,822	3.51	2.61	127,405	3.49	2.60	108,417	3.70	2.60
Education (years)	235,822	13.78	2.60	127,405	13.63	2.74	108,417	13.97	2.41
Female	235,822	0.46							
Race									
White	235,822	0.72		127,405	0.73		108,417	0.72	
Black	235,822	0.10		127,405	0.09		108,417	0.11	
Asian	235,822	0.04		127,405	0.04		108,417	0.04	
Hispanic	235,822	0.09		127,405	0.09		108,417	0.08	
Other	235,822	0.05		127,405	0.06		108,417	0.04	
Foreign	235,822	0.14		127,405	0.16		108,417	0.13	
Native to state	235,822	0.44		127,405	0.43		108,417	0.46	
Firm-unit characteristics									
% Female Leaders	235,822	0.15	0.21	127,405	0.11	0.17	108,417	0.20	0.24
% Black Leaders	235,822	0.03	0.10	127,405	0.03	0.09	108,417	0.04	0.11
% Hispanic Leaders	235,822	0.01	0.06	127,405	0.01	0.07	108,417	0.01	0.06

Table 3

Female leadership and wage levels

The dependent variable is the natural logarithm of the annual wage (defined in Table 2). The sample in Column 1 is a random sample of worker-quarters from US firms in 23 states covered by the Longitudinal Employer-Household Dynamics (LEHD) Program of the US Bureau of the Census. Columns 2-4 exclude workers who are among the top five earners in their state employer identification number (SEIN). The omitted race category is white. *Age* is worker age. *Tenure* is measured as the number of quarters that a worker has spent in the firm. *Foreign* is an indicator for workers born outside the United States. *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 standard industrial classification codes. *Firm Employment* is measured as the total number of workers for the entire firm (across all its plants). *Female Top Leader* is an indicator equal to one if the top earner in the worker's SEIN is female. *% Female Leaders* measures the percentage of women among the top five 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.

Independent variable	(1)	(2)	(3)	(4)
Race = black	-0.209 ***	-0.194 ***	-0.195 ***	-0.194 ***
	(0.004)	(0.004)	(0.004)	(0.004)
Race = Asian	-0.034 ***	-0.029 ***	-0.029 ***	-0.029 ***
	(0.009)	(0.008)	(0.008)	(0.008)
Race = Hispanic	-0.282 ***	-0.266 ***	-0.267 ***	-0.266 ***
	(0.005)	(0.005)	(0.005)	(0.005)
Race = other minorities	-0.036 ***	-0.033 ***	-0.033 ***	-0.033 ***
	(0.006)	(0.006)	(0.006)	(0.006)
Foreign	-0.102 ***	-0.094 ***	-0.094 ***	-0.094 ***
	(0.006)	(0.005)	(0.005)	(0.005)
Native	-0.123 ***	-0.115 ***	-0.115 ***	-0.115 ***
	(0.003)	(0.003)	(0.003)	(0.003)
Education	0.037 ***	0.035 ***	0.035 ***	0.035 ***
	(0.001)	(0.001)	(0.001)	(0.001)
ln(*Age*)	0.296 ***	0.261 ***	0.262 ***	0.262 ***
	(0.006)	(0.006)	(0.006)	(0.006)
ln(*Tenure*)	0.099 ***	0.096 ***	0.097 ***	0.096 ***
	(0.002)	(0.002)	(0.002)	(0.002)
Diversified	0.016 ***	0.013 **	0.014 **	0.013 **
	(0.006)	(0.006)	(0.006)	(0.006)
ln(*Firm Employment*)	0.029 ***	0.036 ***	0.037 ***	0.036 ***
	(0.001)	(0.001)	(0.001)	(0.001)
Female	-0.297 ***	-0.303 ***	-0.280 ***	-0.308 ***
	(0.004)	(0.004)	(0.004)	(0.004)
% Female Leaders		-0.276 ***		
		(0.012)		
(*% Female Leaders*) * (*Female*)		0.226 ***		
		(0.012)		
Female Top Leader			-0.074 ***	0.014 **
			(0.007)	(0.007)
(*Female Top Leader*) * (*Female*)			0.056 ***	-0.018 **
			(0.008)	(0.009)
% Female Leaders > 0				-0.047 ***
				(0.005)
% Female Leaders > 20				-0.058 ***
				(0.007)
% Female Leaders > 40				-0.068 ***
				(0.007)
% Female Leaders > 60				-0.07 ***
				(0.011)
(*% Female Leaders* > 0) * (*Female*)				0.056 ***
				(0.006)
(*% Female Leaders* > 20) * (*Female*)				0.071 ***
				(0.008)
(*% Female Leaders* > 40) * (*Female*)				0.032 ***
				(0.012)
(*% Female Leaders* > 60) * (*Female*)				0.021
				(0.017)
Year fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.340	0.355	0.352	0.355
N	235,822	230,729	230,729	230,729

Table 4

Summary statistics: displaced workers

This table reports summary statistics for a sample of workers from the US Bureau of the Census's Longitudinal Employer-Household Dynamics (LEHD) data matched to closing plants in the Census Bureau's Longitudinal Business Database (LBD). Annual wage is the mean real wage over the preceding four quarters multiplied by four. To be included in the annual wage computation, a quarter cannot be the worker's first or last quarter in his or 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 five earners in the worker's state employer identification number (SEIN). *Δ State* (*Industry*) is an indicator equal to one if the worker's new job four quarters after plant closure is in a new state (two-digit standard industrial classification).

	Workers displaced from closing plants						Closing plant, new SEIN groups		
	Overall		Men		Women			Men	Women
	N	Mean	*N*	Mean	*N*	Mean	*N*	Mean	Mean
Worker characteristics									
Annual wage	461,449	29,933	272,757	34,303	188,692	23,615	15,830	35,463	23,855
Age	461,449	39.68	272,757	39.55	188,692	39.87	15,830	40.20	39.87
Tenure (years)	461,449	2.57	272,757	2.56	188,692	2.65	15,830	2.55	2.51
Education (years)	461,449	13.66	272,757	13.47	188,692	13.95	15,830	13.54	13.79
Female	461,449	0.41							
Race									
White	461,449	0.68	272,757	0.67	188,692	0.69	15,830	0.67	0.68
Black	461,449	0.10	272,757	0.09	188,692	0.12	15,830	0.09	0.09
Asian	461,449	0.04	272,757	0.04	188,692	0.05	15,830	0.04	0.05
Hispanic	461,449	0.12	272,757	0.14	188,692	0.10	15,830	0.12	0.11
Other	461,449	0.06	272,757	0.06	188,692	0.05	15,830	0.08	0.08
Foreign	461,449	0.14	272,757	0.21	188,692	0.16	15,830	0.21	0.17
Native to state	461,449	0.42	272,757	0.41	188,692	0.44	15,830	0.38	0.41
Δ State	461,449	0.02	272,757	0.03	188,692	0.02	n.a.	n.a.	n.a.
Δ Industry	461,449	0.33	272,757	0.34	188,692	0.31	n.a.	n.a.	n.a.
Firm-unit characteristics									
% Female Leaders	438,298	0.20	259,078	0.14	179,220	0.29	n.a.	n.a.	n.a.
% Black Leaders	438,298	0.03	259,078	0.03	179,220	0.04	n.a.	n.a.	n.a.
% Hispanic Leaders	438,298	0.04	259,078	0.05	179,220	0.04	n.a.	n.a.	n.a.

Table 5

Summary statistics: displaced worker traits by gender of the top earner in the hiring firm

The table reports sample means for workers from the US Bureau of the Census's Longitudinal Employer-Household Dynamids (LEHD) data matched to closing plants in the Census Bureau's Longitudinal Business Database (LBD). The sample in the first (last) two columns is workers who find new jobs in firms with a female (male) top earner. Annual wage is the mean real wage over the preceding four quarters multiplied by four. To be included in the annual wage computation, a quarter cannot be the worker's first or last quarter in his or 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. ***, **, and * in Column 3 indicate significance of the difference in means between men in male- and female-led firms at the 1%, 5%, or 10% level, respectively. *** in Column 4 indicates significance of the difference in means between women in male- and female-led firms at the 1% level.

	Female top earner		Male top earner	
	Men (N = 8,165)	Women (N = 12,506)	Men (N = 85,224)	Women (N = 54,747)
	(1)	(2)	(3)	(4)
Annual wage	28,638	21,949	32,794 ***	23,486 ***
Age	39.18	40.52	39.68	40.07
Tenure (years)	2.36	2.51	2.57	2.65
Education (years)	13.37	14.01	13.37 *	14.03
Race				
White	0.62	0.70	0.66 **	0.70
Black	0.10	0.12	0.09	0.11
Asian	0.04	0.05	0.03	0.04
Hispanic	0.17	0.09	0.15	0.09
Other	0.07	0.04	0.06	0.05
Foreign	0.24	0.15	0.21 *	0.15
Native to state	0.40	0.46	0.43 **	0.48

Table 6

Female leadership and 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 in Table 2) two quarters prior to plant closure, and the post-closure wage is the annual wage four quarters following the plant closure. The omitted race category 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 state employer identification number (SEIN). Standard errors are clustered by closing plant and are reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively.

	Displacement costs		Female leadership		
Independent variable	(1)	(2)	(3)	(4)	(5)
Race = black	-0.042 ***	-0.034 ***	-0.032 ***	-0.032 ***	-0.037 ***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
Race = Asian	-0.003	-0.014 ***	-0.016 ***	-0.016 ***	-0.015 ***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Race = Hispanic	-0.032 ***	-0.031 ***	-0.029 ***	-0.029 ***	-0.031 ***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Race = other minorities	-0.015 ***	-0.014 ***	-0.013 ***	-0.013 ***	-0.013 ***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
ln(*Age*)	-0.071 ***	-0.072 ***	-0.069 ***	-0.069 ***	-0.068 ***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
ln(*Wage*)	-0.130 ***	-0.116 ***	-0.128 ***	-0.128 ***	-0.139 ***
	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)
Manager	0.020 ***	0.026 ***	0.036 ***	0.036 ***	0.042 ***
	(0.007)	(0.008)	(0.010)	(0.010)	(0.011)
ln(*Tenure*)	-0.005 ***	-0.015 ***	-0.014 ***	-0.014 ***	-0.014 ***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Female	-0.052 ***	-0.037 ***	-0.041 ***	-0.040 ***	-0.045 ***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
(*% Female Leaders*) * (*Female*)			0.015 *		
			(0.009)		
% Female Leaders > 50					-0.021
					(0.015)
(*% Female Leaders > 50*) * (*Female*)				0.017 ***	0.021 ***
				(0.006)	(0.007)
Plant fixed effects	Yes	No	No	No	Yes
Plant-new SEIN pair fixed effects	No	Yes	Yes	Yes	No
New SEIN fixed effects	No	No	No	No	Yes
Adjusted R^2	0.149	0.476	0.532	0.532	0.456
N	359,537	359,537	256,881	256,881	256,881

Table 7

Female leadership and wage changes by age and wage groups

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 in Table 2) 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 5: 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 state employer identification number (SEIN). All standard errors are clustered by closing plant and are reported in parentheses. *, **, and *** represent significance at 10%, 5%, and 1% level, respectively.

	Age breakouts		Wage breakouts	
Independent variable	(1)	(2)	(3)	(4)
(*Female*) * (*Age* < 25)	-0.065 ***	-0.057 ***		
	(0.006)	(0.007)		
(*Female*) * (25 ≤ *Age* < 35)	-0.056 ***	-0.060 ***		
	(0.004)	(0.004)		
(*Female*) * (35 ≤ *Age* < 45)	-0.033 ***	-0.038 ***		
	(0.003)	(0.003)		
(*Female*) * (45 ≤ *Age* < 55)	-0.033 ***	-0.041 ***		
	(0.003)	(0.004)		
(*Female*) * (*Age* ≥ 55)	-0.028 ***	-0.033 ***		
	(0.005)	(0.006)		
(*Female*) * (*Age* < 25) * (*% Female Leaders* > 50)	-0.008	-0.014		
	(0.014)	(0.016)		
(*Female*) * (25 ≤ *Age* < 35) * (*% Female Leaders* > 50)	0.013	0.015		
	(0.009)	(0.010)		
(*Female*) * (35 ≤ *Age* < 45) * (*% Female Leaders* > 50)	0.017 **	0.024 ***		
	(0.007)	(0.008)		
(*Female*) * (45 ≤ *Age* < 55) * (*% Female Leaders* > 50)	0.025 ***	0.033 ***		
	(0.007)	(0.009)		
(*Female*) * (*Age* ≥ 55) * (*% Female Leaders* > 50)	0.025 ***	0.031 ***		
	(0.009)	(0.011)		
(*Female*) * (*Wage* < 20K)			-0.043 ***	-0.046 ***
			(0.003)	(0.004)
(*Female*) * (20K ≤ *Wage* < 40K)			-0.024 ***	-0.029 ***
			(0.004)	(0.004)
(*Female*) * (40K ≤ *Wage* < 60K)			-0.017 ***	-0.022 ***
			(0.005)	(0.006)
(*Female*) * (60K ≤ *Wage* < 100K)			-0.030 ***	-0.037 ***
			(0.009)	(0.011)
(*Female*) * (*Wage* ≥ 100K)			-0.012	-0.028
			(0.026)	(0.030)
(*Female*) * (*Wage* < 20K) * (*% Female Leaders* > 50)			0.007	0.008
			(0.008)	(0.009)
(*Female*) * (20K ≤ *Wage* < 40K) * (*% Female Leaders* > 50)			0.026 ***	0.034 ***
			(0.008)	(0.009)
(*Female*) * (40K ≤ *Wage* < 60K) * (*% Female Leaders* > 50)			0.028 **	0.031 **
			(0.013)	(0.015)
(*Female*) * (60K ≤ *Wage* < 100K) * (*% Female Leaders* > 50)			0.018	0.010
			(0.025)	(0.029)
(*Female*) * (*Wage* ≥ 100K) * (*% Female Leaders* > 50)			-0.027	0.026
			(0.083)	(0.098)
Worker and firm characteristics	Yes	Yes	Yes	Yes
Plant fixed effects	No	Yes	No	Yes
Plant-new SEIN pair fixed effects	Yes	No	Yes	No
New SEIN fixed effects	No	Yes	No	Yes
Adjusted R^2	0.533	0.456	0.526	0.449
N	256,881	256,881	256,881	256,881

Table 8

Female chief executive officer (CEO) and wage changes for 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 in Table 2) two quarters prior to plant closure, and the post-closure wage is the annual wage four quarters following the plant closure. The omitted race category 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 CEO* indicates that the top earner in the hiring firm (FIRMID) is female. *Female Divisional CEO* indicates that the top earner in the hiring state employer identification number (SEIN) is female. SD is the subsample of single-division (SEIN) firms. MD is the subsample of multi-division (SEIN) firms. All standard errors are clustered by closing plant and are reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively.

	Female CEO		Firm versus divisional CEO	
Independent variable	(1)	(2)	(3)	(4)
Race = black	-0.029 ***	-0.032 ***	-0.033 ***	-0.022 ***
	(0.006)	(0.007)	(0.007)	(0.008)
Race = Asian	-0.011 *	-0.011	-0.003	-0.025 **
	(0.007)	(0.008)	(0.008)	(0.013)
Race = Hispanic	-0.026 ***	-0.026 ***	-0.035 ***	-0.015
	(0.005)	(0.006)	(0.004)	(0.010)
Race = other minorities	-0.011 ***	-0.011 ***	-0.012 ***	-0.009 *
	(0.003)	(0.004)	(0.005)	(0.005)
ln(*Age*)	-0.072 ***	-0.073 ***	-0.079 ***	-0.062 ***
	(0.005)	(0.007)	(0.007)	(0.009)
ln(*Wage*)	-0.122 ***	-0.127 ***	-0.127 ***	-0.123 ***
	(0.008)	(0.009)	(0.007)	(0.018)
Manager	0.023 *	0.029 **	0.029 **	0.030
	(0.012)	(0.014)	(0.013)	(0.027)
ln(*Tenure*)	-0.014 ***	-0.014 ***	-0.014 ***	-0.013 ***
	(0.002)	(0.002)	(0.003)	(0.004)
Female	-0.040 ***	-0.042 ***	-0.040 ***	-0.042 ***
	(0.003)	(0.004)	(0.004)	(0.006)
Female CEO		-0.028		
		(0.022)		
(*Female CEO*) * (*Female*)	0.021 ***	0.022 **	0.016 *	0.040 **
	(0.008)	(0.009)	(0.009)	(0.019)
(*Female Divisional CEO*) * (*Female*)				-0.009
				(0.015)
Sample	All	All	SD	MD
Plant fixed effects	No	Yes	No	No
Plant-new SEIN pair fixed effects	Yes	No	Yes	Yes
New SEIN fixed effects	No	Yes	No	No
Adjusted R^2	0.534	0.497	0.573	0.427
N	160,642	160,642	99,583	53,767

Table 9

Female leadership in closing plant and wage changes for 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 in Table 2) two quarters prior to plant closure and the post-closure wage is the annual wage four quarters following the plant closure. The sample excludes workers who are among the top five earners in their closing plants and includes only workers from closing and hiring firms for which we observe all state employer identification numbers (SEINs) in the US Bureau of the Census's Longitudinal Employer-Household Dynamics (LEHD) data. The omitted race category 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 CEO* indicates that the top earner in the hiring firm (FIRMID) is female. All standard errors are clustered by closing plant and are reported in parentheses. *, **, and *** represent significance at 10%, 5%, and 1% level, respectively.

	>50% female top 5 closing plant	≤50% female top 5 closing plant	Female CEO closing firm	Male CEO closing firm
Independent variable	(1)	(2)	(3)	(4)
Race = black	-0.029 ***	-0.022 ***	-0.037 ***	-0.021 ***
	(0.010)	(0.006)	(0.011)	(0.005)
Race = Asian	0.017	-0.012	0.026	-0.014 *
	(0.015)	(0.008)	(0.016)	(0.008)
Race = Hispanic	-0.018 *	-0.022 ***	-0.011	-0.023 ***
	(0.011)	(0.006)	(0.010)	(0.006)
Race = other minorities	0.008	-0.011 ***	-0.001	-0.010 **
	(0.010)	(0.004)	(0.010)	(0.004)
ln(*Age*)	-0.068 ***	-0.074 ***	-0.077 ***	-0.073 ***
	(0.010)	(0.007)	(0.011)	(0.007)
ln(*Wage*)	-0.091 ***	-0.133 ***	-0.097 ***	-0.131 ***
	(0.008)	(0.013)	(0.009)	(0.013)
ln(*Tenure*)	-0.020 ***	-0.014 ***	-0.014 ***	-0.015 ***
	(0.006)	(0.003)	(0.005)	(0.003)
Female	-0.018 **	-0.040 ***	-0.019 **	-0.039 ***
	(0.008)	(0.004)	(0.008)	(0.004)
(*Female CEO*) * (*Female*)	-0.011	0.028 ***	0.001	0.023 **
	(0.015)	(0.010)	(0.016)	(0.010)
Plant-new SEIN pair fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.510	0.532	0.538	0.525
N	17,585	106,375	18,122	105,838

Table 10
Quantity effects

Ordinary least squares (OLS) regressions using an indicator for a female chief executive officer (CEO) as the dependent variable. *Female CEO* indicates that the top earner in the hiring firm (FIRMID) is female. The omitted race category is white. *Age* is worker age. *Wage* is the pre-closure annual wage. The pre-closure wage is the annual wage (defined in Table 2) two quarters prior to plant closure. *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. Standard errors are clustered by closing plant and are reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively.

Independent variable	OLS (1)		OLS (2)	
Race = black	0.002		0.000	
	(0.003)		(0.003)	
Race = Asian	0.003		0.003	
	(0.004)		(0.004)	
Race = Hispanic	0.000		0.001	
	(0.003)		(0.003)	
Race = other minorities	0.002		0.002	
	(0.003)		(0.003)	
ln(*Age*)	0.014	***	0.012	***
	(0.003)		(0.003)	
ln(*Wage*)	-0.008	***	-0.008	***
	(0.002)		(0.002)	
Manager	-0.009		-0.013	**
	(0.006)		(0.006)	
ln(*Tenure*)	-0.002	*	-0.002	
	(0.001)		(0.001)	
Female	0.034	***	0.024	***
	(0.003)		(0.002)	
Plant fixed effects	Yes		Yes	
New industry fixed effects	No		Yes	
Adjusted R^2	0.004		0.034	
N	160,642		160,642	

Table 11

Industry competitiveness of closing plant and wage changes for 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 in Table 2) two quarters prior to plant closure and the post-closure wage is the annual wage four quarters following the plant closure. The omitted race category 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 CEO* indicates that the top earner in the hiring firm (FIRMID) is female. Industry competitiveness is measured using a Herfindahl index of employment at the two-digit standard industrial classification level. We split industry-years into four equal-size groups. Standard errors are clustered by state employer identification number (SEIN) and are reported in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively.

	Competitive industry	---------->		Concentrated industry
Independent variable	(1)	(2)	(3)	(4)
Race = black	-0.039 ***	-0.021 *	-0.015 **	-0.043 ***
	(0.005)	(0.012)	(0.006)	(0.011)
Race = Asian	0.004	-0.041 **	-0.019 *	0.030
	(0.009)	(0.019)	(0.010)	(0.024)
Race = Hispanic	-0.032 ***	-0.040 ***	-0.015	-0.021 *
	(0.005)	(0.007)	(0.010)	(0.011)
Race = other minorities	-0.005	-0.019 **	-0.013 **	-0.025 **
	(0.006)	(0.008)	(0.006)	(0.012)
ln(*Age*)	-0.077 ***	-0.059 ***	-0.068 ***	-0.080 ***
	(0.009)	(0.013)	(0.009)	(0.014)
ln(*Wage*)	-0.102 ***	-0.145 ***	-0.167 ***	-0.126 ***
	(0.006)	(0.011)	(0.032)	(0.017)
Manager	0.015	0.016	0.091 **	0.019
	(0.015)	(0.017)	(0.043)	(0.030)
ln(*Tenure*)	-0.018 ***	-0.003	-0.007	-0.030 ***
	(0.003)	(0.005)	(0.004)	(0.007)
Female	-0.043 ***	-0.037 ***	-0.046 ***	-0.039 ***
	(0.006)	(0.005)	(0.007)	(0.008)
(*Female CEO*) * (*Female*)	0.018 *	0.018	0.019	0.057 ***
	(0.011)	(0.014)	(0.019)	(0.022)
Plant-new SEIN pair fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.484	0.582	0.576	0.559
N	79,634	36,701	31,425	12,882