Female Leadership and Gender Equity:

Evidence from Plant Closure*

Geoffrey Tate
UCLA

Liu Yang
UCLA

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Abstract

We use unique worker-plant matched data to track the employment outcomes of workers following plant closures. We find that plant closures result in significant and lasting declines in worker wages, particularly among women. We correct for endogenous selection of both the original and new employer using a unit-pair fixed effects model to compare the wages of men and women who move from the same closing plant to the same new firm. We observe lower post-closure wages among women immediately upon re-entering the workforce and continuing for the following three years. These differences persist throughout the wage and age distributions. However, we find a significantly smaller gap between men and women who move to a new firm with a higher fraction of female managers: The magnitude of the extra losses to women is cut in half. Our results suggest that adverse labor market shocks exacerbate gender differences in worker wages in ways which cannot be explained by differences in job selection. But, women in leadership positions foster cultures which are beneficial to female workers throughout their organizations.

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I. Introduction

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

We use unique worker-plant matched data from the Census Bureau to study gender equity in U.S. firms. We track the employment outcomes of workers following plant closures and compare the outcomes of men and women. We find that plant closures result in significant and lasting declines in worker wages. To account for endogenous selection of both the original and new employer, we use a plant-pair fixed effects model to compare changes in wages for men and women who move from the same closing plant to the same new firm. Consistent with a demand-side mechanism, we find strong evidence that women at all age and wage levels fare worse than men, both immediately upon receiving a new job and over the following three years. However, the gap is significantly smaller for workers who move to a firm with a higher fraction of women on the top executive team, particularly when women form a majority. Our results identify an important difference between firms with men and women in leadership roles: female-led firms demonstrate greater gender equity in their hiring practices.

We identify closures of U.S. plants between 1993 and 2001 using the Census Bureau's Longitudinal Business Database (LBD). For a subset of these plants, we obtain detailed worker-level information on wages and demographics from newly available data provided by the Longitudinal Employer Household Dynamics (LEHD) program. The result is a panel dataset of 461,449 workers in 9,244 closing plants covering 23 states.

Because the LEHD wage data extends to the first quarter of 2004, we are able to track each affected worker for at least two full years following plant closure.

Our primary goal is to identify differences in the labor market treatment of men and women. To do so, we measure worker wage changes following job loss due to plant closure. This strategy allows us to circumvent several challenges inherent in comparing the level of wages across individuals. First, it is possible that there are differences across men and women in unobserved, time-invariant worker skill which might lead to differences in wage levels. We remove these differences by estimating the change in wage around a closure event. Though such differences could also affect wage changes, we control for the pre-closure wage level to capture these effects. Focusing on job loss due to closure also allows us to distinguish voluntary from forced job changes. If men and women voluntarily change jobs at different rates, then wage changes around the full set of job changes will be difficult to interpret.

To begin, we estimate the impact of plant closure on worker wages using a nearest-neighbor matching estimator with bias adjustment (Abadie and Imbens (2007)). We choose a random sample of closing workers and then choose a control worker from a non-closing plant in the same state, quarter, and 2-digit SIC for each treated worker, matching on gender, race, age, unit employment, pre-closure wage, and an indicator for multi-unit firms. We estimate a statistically significant 17% decline in annual wages in the first year following closure for closing workers relative to their matches. Wages remain 7% lower among treated workers three years following the event. We also condition our estimates on the treated worker re-entering the workforce to remove the impact of unemployment. We find that the relative decline in wages is 6.3% in the first year and 3.4% after three years. Thus, workers in closing plants suffer not only due to unemployment, but also from worse matches with their new employers upon finding a new job.

Next, we ask whether gender affects the magnitude of the shock to worker wages. We estimate a pair fixed effects model which compares women and men from the same closing unit who move to the same new unit following closure of the original plant. Men and women may work in firms which are systematically different. For example, women may prefer to work in firms which are more family-friendly or which minimize commute times. Since women on average have shorter expected work lives and higher job turnover rates (Gronau (1988)), they may also invest less in firm-specific human capital than male colleagues. As a result, they may choose to work in firms in which such capital carries less of a premium. Since displacement due to closure implies the forfeiture of such capital, these differences in choices may generate gender differences in wage changes across men and women following plant closure. Finally, the same preferences which lead mean and women to choose different firms ex ante might lead them to make different choices ex post when re-entering the work force. Including fixed effects for the closing plant – new firm pair allows us to identify the impact of gender distinctly from these selection effects.

We find that women experience an additional 4% one-year loss in real wages, controlling for age, race, tenure, and the wage prior to closure. Because women do worse than men from the same closing plant (controlling for wage and tenure), the wage gap is unlikely to be the end result of less investment in firm-specific capital over time in the closing plant. A related alternative, however, is that the new employer anticipates that the displaced women will make fewer such investments going forward, reducing their expected marginal productivity. We perform several additional tests to try to gauge the importance of this mechanism. First, we partition the age distribution, considering separately workers under 25, between 25 and 35, between 35 and 45, between 45 and 55, and over 55. Though the wage gap is largest in the youngest two categories (consistent with lower expected future investments in firm-specific capital), we find that women underperform men from their closing plants in every single grouping. Among workers over 55, for whom the constraints of family are less likely to differ by gender, women underperform men by 2.5% over the first year and 4.5% after three years.

Next, we partition the sample into seven bins based on pre-closure wages (less than $20K, $20-30K, $30-40K, $40-50K, $50-75K, $75-100K, and greater than $100K). We again find that women underperform men from the same closing plant in every wage bin. Women in the highest wage bins appear to do the worst, which is surprising if their high ex ante wages reflect a lower tendency to sacrifice career advancement in favor of family.

Finally, we consider the subsample of "stayers," or workers who worked in their closing plant for at least 5 years prior to closure. Lower expected investments in the new job may entail lower expected commitment to the job. However, women in the "stayer" group demonstrated their commitment to long-term employment in their prior jobs. Nevertheless, we find that women significantly underperform displaced men, even in this subsample. The magnitude of the estimate is smaller. However, a portion of the effect seems to come from sample composition: there are more workers from the less penalized older age deciles in the "stayer" subsample.

Our evidence suggests a demand-based mechanism for the higher displacement costs among women. As a final step, we provide direct support for this interpretation. We identify the top management of each firm which hires displaced workers. We then classify firms based on the percentage of the leadership who are women. We find that the gap between the losses of displaced men and women is smaller if they are hired by female-led firms. The results are strongest for women in the middle of the age distribution (beyond the main years in which career interruptions for child-birth are a prime concern) and extend to women at the lower reaches of the wage distribution. Moreover, the result is particularly strong if women comprise the majority of the new firm's management team. Though it is difficult to separate the impact of female-friendly corporate cultures from the direct influence of female management, the latter result suggests that women in power can improve the prospects of other women in their organizations.

Overall, we find robust evidence that plant closure decisions harm workers, not just via unemployment but also through "worse" future employment. The impacts of these adverse shocks appear to accrue disproportionally to women, even correcting for selection into different firms ex ante and ex post. The effects persist for (at least) three years and apply to a wide cross-section of events across geographic, industry, and firm boundaries. Our results suggest that the impact of taste-based discrimination is particularly salient when workers have limited opportunity sets: Women appear to fall further behind men as a result of adverse labor market shocks, an effect which does not appear to diminish over time.

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

We also add to the literature measuring the cost of displacement. Existing evidence using data from unemployment administrative records typically focuses on smaller samples consisting of a single state (Jacobson, Lalaonde, and Sullivan (1993) and Couch and Placzek (2010)). To our knowledge, ours is the first study to link administrative data across a wide panel of states to plant-level data. This match is important since "mass worker exits" observed only in administrative records may be partially worker initiated, may not involve all of the firm's workers, and may represent pure administrative changes with no impact on workers at all. Our approach yields several new insights relative to such studies. For example, Jacobson et al do not find evidence of gender differences in displacement costs using a sample of Pennsylvania workers. Using our broader data, we overturn this result, showing that women suffer more from displacement in every region of the country except the Northeast. A small number of papers do find evidence of gender effects on the cost of displacement using survey data from the National Longitudinal Survey (NLS) or the Displaced Worker portion of the Current Population Survey (CPS) (Koeber and Wright (2006), Madden (1987), Maxwell and D'Amico (1986)). The samples of workers in these studies are typically small and the data do not allow the researchers to match workers to specific closing firms or to new post-displacement firms. Thus, they cannot separate selection effects from the unexplained impact of gender.[1]

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

Our evidence also contributes to a growing literature in corporate finance which measures long-lasting impacts of early-life or career outcomes on workers (Oyer (2008), Schoar (2007), and Malmendier, Tate, and Yan (forthcoming)). For data reasons, many of these papers focus on the subset of workers who ultimately attain a top corporate job or on survey evidence from employees very early in their careers. We extend the literature by examining a major corporate event (plant closure) and measuring its implications for an (unselected) broad cross-section of workers. Plant closure is particularly important since, to the degree that workers are aware of the financial positions of their firms and cannot diversify away their human capital risk, high expected bankruptcy costs at the worker level may influence bargaining and, ultimately, firms' capital structure choices (Berk, Stanton, and Zechner (2009)).

Our paper also contributes to the growing literature that documents differences in the decision-making of women and men in top corporate positions. A number of recent papers examine the impact of women on corporate boards of directors (Adams and Ferriera (2009); Ahern and Dittmar (2010)). Bertrand and Hallock (2001) study the gender wage gap among top corporate executives. Most related to our results, Matsa and Miller (forthcoming) and Bell (2005) show that women top executives earn more in female-led firms. We extend their analyses of "women helping women" to the entire firm, looking at the impact of female leadership on the hiring of women throughout the organization.

The remainder of the paper is organized as follows. In Section II, we describe the data we use in our analysis. In Section III, we estimate the impact of plant closure on displaced employees. In Section IV, we test for differences in the impact of job loss across men and women. In Section V, we test whether the costs of plant closure differ among workers who move to female-led firms. Finally, Section VI concludes.

II. Data

We use worker-, firm-, and plant-level data from the U.S. Census Bureau to estimate the impact of plant closure (and gender) on wages. We identify individual plants and their ultimate owners (firm), geographic locations (state and county) and industries (4-digit

SIC) using the Longitudinal Business Database (LBD). The LBD is a longitudinal database covering all non-farm establishments with paid employees in the US since 1976. It also 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 (LEHD) data. The LEHD data is constructed using administrative data collected from the state unemployment insurance (UI) system and the associated ES-202 program. The coverage of the state UI system is broad and generally comparable from state to state: it contains about 96% of total wages and civilian jobs in the U.S.[2] Wages reported to the state UI system include bonuses, stock options, profit distributions, the cash value of meals and lodging, tips and other gratuities in most of the states, and, in some states, employer contributions to certain deferred compensation plans such as 401(k) plans.[3] The U.S. Census Bureau negotiates agreements state-by-state to provide research access to UI data through the Census Research Data Centers (RDC). Currently, 23 states allow such access to their data: Arkansas, California, Colorado, Florida, Iowa, Idaho, Illinois, Indiana, Maryland, Maine, Montana, North Carolina, New Jersey, New Mexico, Oklahoma, Oregon, South Carolina, Texas, Virginia, Vermont, Washington, Wisconsin, and West Virginia.

Our identification strategy requires us to link worker data from the LEHD 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. However, such a link is not generally possible for firms which have multiple establishments. Firms within the LEHD data are identified using state employer identification numbers (SEINs), which are then linked to EINs. Each EIN may have many associated SEINs, both within and across state lines. Within each SEIN, a firm may

[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.

have several reporting units (SEINUNITs). However, there is no clear relation between these tax reporting entities and the physical establishments identified in the LBD.[4]

If the firm only operates a single plant, it is trivial to link the firm's workers to the plant using the EIN. If the plant is part of a multi-unit firm, we first determine whether the plant is the firm's only one within its state, county, and four-digit SIC code. If so, we determine which of the firm's employees work at the plant using the Census Bureau's Business Register Bridge (BRB). The BRB links the LEHD data and the LBD at various levels of aggregation. The finest partition is at the EIN, state, county, and four-digit SIC code level. When the BRB allows data from the two sources to be merged at this level, it means that all workers from the LEHD within the partition match to all LBD plants within the partition. Thus, when we add the additional restriction that the LBD plant is unique within the partition, we achieve a match of individual workers to a unique plant.

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 SEINUNIT(s) to which we link the closing plant disappear from the LEHD data in the LBD-identified closing year or within the first three quarters of the following year to avoid "closures" due to changes in administrative records. Finally, we consider workers who are employed in the closing plant two quarters prior to the last quarter the SEINUNIT appears in the LEHD data. Workers may begin to exit a dying plant in the months preceding closure. To the extent that such exit is not random, it may bias our estimates of ex post wages and employment outcomes if we consider only the workers remaining at the closing date.

The BRB file linking the LEHD data to the LBD is available from 1992 to 2001. Because we must link closing plants to their workers in the year prior to closure, we restrict our samples to the period 1993 to 2001. The LEHD wage data is currently available through the first quarter of 2004. Thus, we can track the outcomes of all sample workers for (at least) 2 full years following a plant closure.

Since the Census Bureau currently only provides access to employment records from 23 states in the LEHD data, we generally overstate unemployment rates in our

[4] There is wide variation in the number of "units" reported for a particular firm across the two datasets, even within a particular state, county, and industry classification (4-digit SIC). Moreover, the number of units is sometimes larger in the LEHD data and is sometimes larger in the LBD.

sample: a worker may have a job in one quarter and not appear in the data the next due either to job loss or to migration to an uncovered state. Most of our analysis concerns changes in wages. As long as the factors affecting the decision of the state to opt into or out of the LEHD program are orthogonal to the determinants of (changes in) wages, our estimates should not suffer from selection bias. Moreover, the within-sample rate of migration to a new covered state – even following plant closure – is low (approximately 2.5%). Thus, the potential impact of unobserved migration on our analysis appears to be small.

We make several adjustments to the reported wages for our analysis. We use the quarterly consumer price index to compute real quarterly wages in beginning-of-1990 dollars. We also aggregate quarterly wages into annual real wages. Because of annual bonuses and other predictable seasonal variation, quarterly wages may not provide an accurate reflection of the worker's earnings and quarterly wage changes may not reflect real changes to the compensation contract. Thus, in any given quarter, we compute annual real wages for the preceding year as the mean real wage over the prior 4 quarters multiplied by 4. We also require at least three consecutive quarters of wage data to include the quarter in the sample and use only interior quarters in the computation. The latter restriction is necessary since the first or last quarter's wage reflects payment for an unobserved fraction of the quarter. Finally, we exclude workers younger than 16 or who earn less than $10,000 from our analysis. We identify the manager of the unit (firm) quarter-by-quarter as the worker with the highest wage in the unit (firm) and the management team as the top 5 highest paid workers in the unit (firm).

In Table 1, we provide plant-level summary statistics of the data. In Panel A, we provide summary statistics for a random sample of 655,929 plants from the LBD between 1993 and 2001. The average plant has 194 workers and a payroll of $6.83 million. 58% of plants are part of multi-unit firms and 42% are part of firms which operate in at least 2 distinct 2-digit SIC codes. In Panel B, we see that plants from multi-unit firms do not have significantly larger employment (mean = 202), but have larger payrolls (mean=$7.59 million). 55% of the plants come from the 23 states covered by the LEHD data.

We also consider the 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. One consequence of our restriction to plants which are unique within their firm, county, and 4-digit SIC is that our matched data significantly under-represents plants from multi-unit firms (15% as compared to 49% in the total closing sample). However, conditional on being part of a multi-unit firm, the fraction of plants which are part of a diversified firm is 69%, which is similar to the overall LBD sample (71%) and only slightly lower than the LBD closure sample (79%). Matched sample plants are also smaller than the typical LBD (closing) plant, both among single- and multi-unit firms. In the full matched sample, mean employment is 134 and average payroll is $2.333 million. The matched sample also significantly under-samples the Northeast, most likely due to the exclusion of New York from the LEHD universe. Surprisingly, we do not observe a large spike in closures in 2001, as in the random LBD sample. Our matched sample has a similar industry distribution to the closing and random samples from the LBD.

In Table 2, we provide summary statistics at the worker level. In Panel A, we present statistics for a random sample of worker-quarters from the LEHD data. The average worker is 41 years old with 3.4 years of tenure in the SEIN. Women make up 46% of the workforce. 10% of the workforce is Black, 4% Asian, 9% Hispanic, and 5% other non-white. The mean annual wage is $34,999. Workers in multi-unit firms earn higher mean wages, particularly in diversified firms (mean single-unit = $30,613; mean focused multi-unit = $33,527; mean diversified = $37,121).

In Panel B, we provide summary statistics for the workers in the LBD – LEHD matched sample of closing plants. The mean worker is one year younger and women make up only 41% of the workforce. Most noticeably, mean wages are smaller ($29,933), likely reflecting the smaller plant size in the matched sample (Table 1). The pattern in mean wages across firms with different organizational structures is also less pronounced in this sample.

Overall, our analysis reveals some non-random selection as a result of limitations in our ability to merge the LBD with LEHD data. However, it is unclear how or why these selection effects would interact with the impact of gender on wages. Our main tests use plant fixed effects as a way to correct for the non-random selection of closing plants into our sample.

III. The Costs of Plant Closure

In most of our analysis, we focus on relative differences in the impact of plant closure between men and women. Before turning to this analysis, we establish the baseline effect of plant closure on affected workers. We demonstrate that involuntary displacement due to plant closure has a significant and long-lasting negative impact on employees. This effect can take two forms: extended unemployment due to frictions in the labor market and wage declines due to less advantageous matches between the displaced workers' human capital and their new firms' needs. We estimate two sets of models to disentangle the effects.

First, we estimate an unconditional average treatment effect for the treated, where treatment is employment in a plant two quarters prior to its closure. The outcome variable of interest is the percentage change in the real annual wage. Using t to indicate the quarter of closure, we measure the wage change as the annualized real wage from t to $t+4$ divided by the annualized real wage from $t-5$ to $t-2$, minus 1. We also consider the change over two and three years, substituting the annualized real wage from $t+5$ to $t+8$ and $t+9$ to $t+12$ for the wage from t to $t+4$, respectively. We use the nearest neighbor matching technology from Abadie and Imbens (2007) to perform the match, with bias adjustment.[5] For computational tractability, we use a random sample of 2,004 workers from our LBD-

[5] The bias adjustment procedure estimates an OLS regression of the outcome variable on the match variables on the set of non-treated observations. These estimates are then used to correct the estimated treatment effect for bias due to remaining differences in covariates across the treated and control samples post-match. Because of the large pools of potential matches in our application, these remaining differences are quite small and the bias adjustment has only a minimal effect on the estimates.

LEHD matched sample.[6] We exact match treated workers to control workers in the same state, quarter, and 2-digit SIC. We include gender, race, age, firm size (measured as aggregate employment and by an indicator for multi-unit firms), and the annualized real wage from *t-5* to *t-2* as additional match variables. The idea is to compare each worker in a closing plant to a worker in the same state and industry at the same time whose plant is not closing and who is the same on all the other matching characteristics. In this way, we isolate the impact of closure on wage changes from the impact of differing characteristics across treated and untreated workers. The estimate is valid as long as unconfoundedness holds; or, essentially, as long as there are no unobservables correlated both with plant closure and wage changes, conditional on the included covariates. In Panel A of Table 3, we report the results using a single match for each treated worker, but the results are similar using the two nearest neighbors as controls.

We find a statistically significant 17% one-year decline in real wages among workers in closing plants, relative to their matches. When we extend the horizon to two or three years, we see some convergence in wages between the treated and control samples. But, even after three years, we continue to see a 7% gap.

To separate the impact of unemployment from worse subsequent job matches, we re-estimate the average treatment effect for the treated conditional on the worker being employed (i.e. having a non-missing wage).[7] In Panel B, we present the results.[8] We continue to find a negative and significant impact of plant closure on the workers. Over the one year horizon, the effect is roughly 6.3% even among the workers already re-employed. Over three years, the effect is a more modest 3.4%.

Overall, our results confirm that plant closure has negative consequences for workers, due both to unemployment and lost relative wages in their new jobs. Our estimates of the

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[6] This corresponds to 1% of the sample with all necessary variables non-missing. We also choose our control observations from a non-overlapping (much larger) random sample of the LEHD data. Our results are robust to using different seeds to choose the 1% sample and to choosing larger random samples (e.g. 5%). An advantage of this approach is that it prevents the large sample size from swamping the sampling variation in the standard error computation.

[7] Because we only observe 23 states, a missing wage can indicate either unemployment or employment in an uncovered state. Though cross-state migrations are relatively rare, this alternative test also allows us to remove the error from treating movers as unemployed.

[8] Here we use the difference in log wages rather than the percentage change as the dependent variable to mitigate the impact of outliers on the results. We use the percentage change in our first estimations since unemployment leaves a large mass of workers with 0 wages. In this case, the difference in the log wage massively overstates the wage loss relative to the percentage change.

effect are somewhat smaller than other estimates in the literature. These differences may reflect our broader data sample or cleaner separation of closures from (performance-induced) layoffs. Alternatively, they may reflect our sample period, which falls within a protracted economic boom.

IV. The Impact of Gender on Displacement Costs

Next, we ask whether there is a gender difference in the cost of plant closure to workers. There is a well-known gender gap in the cross-section of worker wages. In our sample, we estimate a gender wage gap in pre-closure wages of roughly 26%, including standard demographic controls as well as plant, state, year, and industry fixed effects.[9] This estimate is in line with Altonji and Black (1999) who report a 22% gap using data from the March 1996 Current Population Survey. Given the gender gap in wage levels, it is natural to ask whether women are also more adversely affected by labor market shocks. An advantage of looking at changes around such shocks is that we remove the impact of unobserved time-invariant differences in worker quality, which may contaminate estimates of differences in wage levels if they are correlated with gender. Of course, such differences in quality may also affect the change of wages in response to shocks; however, we can use the pre-shock wage level itself as a sufficient statistic for these unmeasured differences in human capital.

IV.A. Job Choices

A key challenge in interpreting differences across men and women in the impact of displacement is separating the effects of supply and demand. Suppose we can fully remove the impact of worker quality on outcomes. Then, to what degree do men and women experience different outcomes because they have different underlying preferences which cause them to choose different jobs either ex ante or ex post? Or, to what degree

[9] The demographic controls are race (with separate dummies included for black, Hispanic, Asian, and other non-white workers), a dummy for foreigners, age, tenure, and education. In the LEHD data, tenure is left-truncated. That is, we do not know how long workers have been with their firms prior to the beginning of our data sample. Moreover, education is an imputed variable. Thus, the coefficient is, at best, estimated with error. We include these variables simply to soak up variation potentially explained by factors other than gender (and note that it is not obvious why the problems with the variables would be correlated with gender).

do their outcomes differ due to differences in the opportunities they face in the marketplace?

To illustrate the challenge, we briefly consider the rates at which men and women change states or industries following plant closure. In Columns 1 and 2 of Table 4, we report estimates from logit regressions on the sample of workers from closing plants, in which the dependent variable indicates that the worker moved to a different (covered) state. We include controls for the pre-closure wage, race (broken into dummies for Black, Hispanic, Asian, and other non-white workers), tenure, age, firm size, and the number of plants in the same 2-digit industry and county as the worker's closing plant. We also include indicators for whether the worker is native to the state in which the closing plant is located, whether s/he is the firm's manager (defined as the top earner in the SEIN), and whether the firm is diversified. Finally, we include state, 2-digit industry, and year fixed effects. We cluster standard errors at the plant level.[10] We report the coefficient estimates as log odds ratios.

The results reveal a number of interesting patterns. We see that minority workers are significantly less likely to change states, as are younger workers, low-wage workers and "local" workers. Workers are also significantly less likely to change states if there are more local plants operating in their current 2-digit industry, suggesting that location and industry are substitutes. This effect is consistent with higher displacement costs among workers who move to new industries (Neal (1995)). Turning to the coefficient of interest, we find that odds of women moving to a new state following plant closure are 15% less than the odds among men. In Column 2, we partition the gender dummy by categories of worker age. We find that the effect of gender is stronger among older workers than among younger workers.

In Columns 3 and 4, we re-estimate the logit regressions, but using an indicator for changing 2-digit SICs as the dependent variable. We again see some evidence of less mobility among minority workers and younger workers, though the effects are substantially weaker than we observe for state changes. We see that low-wage workers are more likely to change industries (as are managers, interestingly). Workers in diversified firms are also significantly more likely to change industries, suggesting a role

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

for organizational structure in facilitating the redeployment of human capital (Tate and Yang (2011)). Finally, we see that women are marginally more likely than men to move to a new industry following plant closure. Here the effect is mainly driven by younger workers.

Overall, we find that the new jobs of men and women following displacement are systematically different. This result suggests that there are serious selection biases in comparing displacement costs across men and women. Our goal is to isolate the portion of the effect that is not explained by different job choices among men and women before or after plant closure. This portion most clearly indicates the preferences of the new employer regarding men and women. Of course, these preferences can affect the employee's job choice as well. In this sense, our results may understate the true impact of employer tastes on gender equity.

IV.B. Wage Changes

Next, we measure the impact of gender on displacement costs due to plant closure. In Column 1 of Table 5, we estimate an OLS regression of the change in wage around plant closure on a gender dummy. We measure wage changes using the annualized real wage from quarters t to $t+4$ and $t-5$ to $t-2$, where quarter t is the quarter of closure. We also restrict the sample to workers who re-enter the workforce by quarter $t+3$. As controls, we include the set of race dummies from Table 4, age, tenure in the closing plant, pre-closure wage, firm size, an indicator for managers, and an indicator for diversified firms. We also include state, 2-digit industry, and year fixed effects. Again, we cluster standard errors at the plant level. We find that women experience a significant 4% decline in wages relative to men. Our results are similar if we include additional controls indicating whether the new job is out of state or in a new 2-digit SIC industry.

In Column 2, we address the first selection concern: women may work in different firms from men ex ante. For example, women may anticipate investing less in on-going training due to family considerations and, as a result, choose firms which place a lower premium on such investments. Then, wage changes following displacement are difficult to compare across groups since pre-event wages are set in systematically different firms. We correct for this source of selection by including a plant fixed effect in the regression.

That is, we identify the gender gap by comparing only men and women originating in the same closing plant, and controlling for pre-closure wages and demographics. We find that removing this source of bias actually increases the magnitude of our estimate of the gender effect.

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

We also see some interesting patterns in the control variables. After controlling for selection effects, we see that minorities perform worse than white workers, though the magnitude of the effects is less than the gender effect in all cases. We also see that older workers and higher wage workers suffer more. The latter effect is interesting since men are higher paid than women in the cross-section. Thus, despite being higher wage workers, on average, men still outperform women following closure. We also see that workers with longer tenure in the closing plant suffer more, which is not surprising if longer tenure allows 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 also repeat the exercise for workers who re-enter the labor force between quarters *t+5* and *t+8* and between quarters *t+9* and *t+12*. The gender gap substantially increases as the length of unemployment prior to finding a new job increases. Thus, our reported results provide a conservative measure of the impact of gender on wage changes. Overall, we conclude that women suffer more as a result of plant closure than men, even controlling for selection of the closing plant and the new firm.

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

Despite these controls, it remains possible that men and women differ in the terms at which they are willing to re-enter the labor market following displacement. An immediate concern is that women are more risk averse than men and therefore accept lower offers than men to avoid the possibility of prolonged unemployment. In our data, we do not see large differences between men and women in the rate at which they re-enter the workforce following plant closure. We estimate a slightly higher re-entry rate in the first year among women; however, women are also less likely to change states following plant closure (Table 4). So, even this near-zero effect is confounded by the possibility that men more often move to states which we do not observe in our data sample. Moreover, this story would imply that women hired at a given wage following plant closure are higher in quality than men. If this is the case, we would expect to observe convergence of the wages of women towards the (higher) wages of men over time. We instead find the opposite: the relative losses of women increase over the two years following the acceptance of a new job.

IV.C. Wage Changes by Worker Characteristics

Our results suggest that firms display a preference for displaced men over displaced women. Next, we examine possible explanations for this revealed preference. One possibility is that new employers anticipate lower productivity from women due, for example, to less expected investments in firm-specific capital or less commitment to the new job. If this is the case, we should see differential impacts among women depending on the intensity of the outside pressures they face. We identify three proxies for differences in these pressures and use them to assess the merits of this explanation for the gap between the wages of newly hired men and women coming from the same closing plant.

First, we exploit differing incentives across the age distribution. If women do worse due to lower expected commitment to the new job, we might expect to see the largest relative wage gap between men and women if we restrict attention to women's child-bearing years. In Table 6, we re-estimate the regression from Column 2 of Table 5, but including indicator variables for five breakouts of the age distribution in lieu of the continuous age control.[12] In particular, we consider separately workers under 25, workers between 25 and 35, workers between 35 and 45, workers between 45 and 55, and workers over 55. The distribution of our sample over these 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. We also interact each indicator with the gender dummy. In Column 1, we consider the one-year wage change as the dependent variable, as in Table 5. We find some evidence consistent with the human capital story. The magnitude of the gender wage gap is the largest among workers under the age of 35. For example, one year following the plant closure, women under 35 suffer a 7 percentage point larger wage loss than their male colleagues in the same age group. Although the wage loss decreases as we move up the age distribution, we still observe a significant gender wage gap of 4.5% for workers between 45 and 55 and a gap of 3.2% for workers over 55.

In Columns 2 and 3, we measure the extent to which the wages of displaced men and women converge over time. In Column 2, we measure the wage change as the difference between the annualized real wage from quarters *t+5* to *t+8* and the annualized real wage from quarters *t-5* to *t-2*. We restrict the sample only to workers who were also in the Column 1 sample. Thus, we do not include new re-entrants to the labor market which occur after quarter *t+4*. In Column 3, we repeat the same exercise, but using the wage change from quarters *t+9* to *t+12* and quarters *t-5* to *t-2*. We find that the wage gap widens, rather than converges over time in the new firm. For example, three years following the closure, women under 35 suffer a wage loss more than 10 percentage points higher than men from the same closing plants. The lowest estimated gender gap in any portion of the age distribution after three years is 5.5%. The difference is even bigger when we compare workers who re-entered the labor force after the first year. On average, among workers who found jobs between quarters *t+5* and *t+8*, the difference in wage losses among men and women is 6.2 percentage points; and, the difference increases to 14.5 percentage points for workers who found jobs between quarters *t+9* and *t+12*.

[12] This specification only has a fixed effect for the closing plant and not a fixed effect for the closing plant, new unit pair. However, all results in the remainder of the paper are robust to the inclusion of pair fixed effects. We tabulate only the plant fixed effects specification mainly for brevity. However, it is important to note that the pair fixed effect model identifies the coefficients of interest using only men and women who move from the same closing plant to the same new firm. In many cases, this set of workers may be small. Thus, the tradeoff between the two sets of results is between stronger identification and the potential for a lack of representativeness. Reassuringly, both sets of results lead us to the same conclusions.

A second way to split the sample based on likely outside pressures faced by women relative to men is to consider the ex ante wage distribution. Women at high levels of the ex ante distribution have signaled commitment to their careers in their prior jobs. Moreover, investments in firm-specific capital may be more important lower in the wage distribution. Management skills, for example, may be relatively more portable across firms than knowledge about a specific aspect of a firm's manufacturing process. In either case, we would expect the gender wage gap to be higher among workers lower in the wage distribution, if it is primarily due to an expectation of lower such investments among female workers.

We test this prediction in Table 7. We divide workers into seven wage groups – less than \$20,000, \$20,000 - \$30,000, \$30,000 - \$40,000, \$40,000 - \$50,000, \$50,000 - \$75,000, \$75,000 - \$100,000, and greater than \$100,000. As in Table 6, we include both the group dummy variables and their interactions with gender. We also estimate three specifications, using one-year, two-year, and three-year wage changes as the dependent variable. We again include only workers who are re-employed by quarter *t+4*. We include plant fixed effects in all of our specifications.

Consistent with our earlier findings, high-wage workers suffer bigger wage cuts. The coefficients on the wage group dummies decrease monotonically as we move up the wage distribution. Across all wage groups, women experience more severe wage cuts then men, with the coefficients on all the interaction terms significant at the 1% level. The coefficients are slightly lower for wage groups in the middle of the distribution (\$30,000 - \$40,000 and \$40,000 - \$50,000) and are slightly higher for both low- and high wage workers. Our findings suggest that the gender effect in wage changes is not specific to a certain wage group, but rather a common phenomenon affecting women across various income levels. As we found using worker age as a proxy for incentives to invest in firm-specific capital, such investments do not appear to provide a sufficient explanation for the entire observed gap.

As a third approach, we isolate the set of workers who demonstrated high commitment to their prior jobs. Specifically, we identify "stayers," who worked in their prior firm for at least five years before the plant closure. Women among this group should suffer less relative to men, having resolved uncertainty regarding their willingness to invest in the firm. A challenge in testing this prediction is that we only imperfectly observe tenure in our dataset. We have no information on workers' histories inside their firms or plants prior to the beginning of the dataset. We compute tenure as a within-sample measure of how long each worker has been with his or her firm. Thus, our measure of tenure is downward-biased. We will undercount "stayers" in the sample, particularly in the early years of the dataset. However, this error affects mainly the power of our estimates. It is the set of "leavers" which is measured with error.

In Table 8, we re-estimate our basic regression specification for the subsample of "stayers." Among our full sample of workers, only 14% have stayed for at least five years with the firm. We again consider one-, two-, and three- year wage changes and restrict the sample to workers who re-enter the workforce prior to quarter *t+4*. On average, the difference in wage changes between women and men is about 3.3 percentage points in one year and it grows to 4.2 percentage points in three years (in our main specification, it is 5.2 and 7 percentage points, respectively). Thus, again, we find some evidence consistent with lower expected commitment to the new firm, but a large portion of the gender gap remains unexplained.

Overall, our analysis suggests that plant closures have differential effects on men and women. The greater wage losses by women cannot be explained by selection effects and they are also difficult to reconcile fully with stories based on on-going investments in human capital. A remaining possibility is that the adverse outcomes of women are driven by labor demand.

Before turning to a more direct test of this possibility, we briefly explore whether there are regional differences in the gender effect depending on the location of the closing plant. We partition our sample of closing plants into six regions: Northeast, South, Midwest, Rocky Mountains, Southwest, and West.[13] We then re-estimate the wage regressions from Table 5, including indicator variables for each region and interactions of those indicators with the gender effect. We uncover some interesting regional differences. Workers in closing plants in the South and Midwest fare significantly worse outcomes

[13] Northeast includes Maryland, Maine, New Jersey, and Vermont; Midwest includes Iowa, Illinois, Indiana and Wisconsin; South includes Florida, North Carolina, South Carolina, Virginia and West Virginia; Southwest includes Arizona, New Mexico, Oklahoma and Texas; Rocky Mountain includes Colorado, Idaho and Montana; and West includes California, Oregon and Washington.

than workers from closing plants in other regions. Moreover, in all cases, women fare worse than men, on average. The effect is strong and statistically significant at the 1% level over the one-, two-, and three-year horizon in every region except the Northeast. In the Northeast, we do not estimate a significant gender effect.

V. Wage Changes and Female Leadership

So far, we have shown that the impact of plant closure accrues disproportionally to women, even correcting for selection into different firms ex ante and ex post. The evidence is consistent with the argument that women face adverse labor market treatment. To provide more direct evidence on the role of firm demand, we test whether female leadership in the hiring firm mitigates the gap between the outcomes of displaced men and women.

There is substantial support for the notion that women may fare better in female-led firms. First, women may pull other women into firms because they prefer to work with similar individuals, consistent with the discrimination model of Becker (1957). Second, as women advance through ranks and signal information about their individual quality, they should be less subject to "statistical discrimination," as in Aigner and Cane (1977). Finally, women in senior management positions may help other women advance more quickly to the top of organizational hierarchies by improving their access to job-specific human capital and social networks (Catalyst (1996) and Ely (1994)).

To test this hypothesis, we first identify the percentage of women among the top five managers of each firm which hires workers from our sample of closing plants. We identify the top five mangers as the individuals who have the five highest annualized wages in the year prior to hiring workers from a closing plant. On average, one of the top five managers is a woman in this sample of firms. 25% of the firms have at least two female managers and 12% of the firms have more than two female managers. Note, in the latter group of firms, women make up the majority of the management team.

In Table 9, we measure the relation between wage changes following plant closure and female leadership of the new firm. We estimate the specification from Column 2 of Table 5. In Column 1, we add a categorical variable measuring the percentage of female managers in the new firm and its interaction with the gender indicator. We find a significant positive coefficient on the interaction term, suggesting that more women on the leadership team reduces the gap between newly hired (displaced) men and women. We also find a significant negative level effect of the percentage of female managers. That is, workers of both genders experience larger wage losses when they move to female-led firms. This result is not surprising, given the scarcity of female executives among the largest U.S. firms and the strong positive correlation between wages and firm size.

In Column 2, we include an indicator variable which equals one if the new firm has at least two women among the top five managers (the highest sample quartile). Again, we include the interaction between the female indicator and our measure for female leadership in the new firm. We find that women who join a new company with significant female leadership fare better: the wage loss relative to men is cut in half from 5.7% to 2.8%.

We also perform a number of other tests, mirroring our approach in prior sections. We find that the impact of female management on the wages of newly hired displaced women is the strongest among women in the middle of the age distribution. These women are less likely to experience future career interruptions due to child-birth. Thus, the result is again suggestive of taste-based discrimination as an explanation for the gap between men and women and female leadership as a mitigating factor. We also find that this helping hand is not extended only to women at the upper reaches of the hierarchy (e.g. female CEOs hiring female division heads), but is particularly strong among workers in the bottom two thirds of the wage distribution.[14] Moreover, it exists even among workers in the lowest quintile of the wage distribution.

A challenge is separating the effect of female leadership from the effect of more female-friendly corporate cultures in these firms. As a first step toward distinguishing these possibilities, we find that the reduction in wage losses among women is particularly strong when women make up the majority of the top management team in the hiring firm. Thus, women in leadership positions appear to play an integral part in establishing the female-friendly cultures of their firms.

[14] Recall the mean sample wage is among closing workers is roughly $30,000 (Table 2).

VI. Conclusion

We use a unique employer-worker matched data set – drawing on data from the Longitudinal Employer-Household Dynamics (LEHD) Program and the Longitudinal Business Database (LBD) – to examine the impact of plant closures on workers. We find that plant closures lead to a significant 17% immediate drop in real wages among affected workers. The decline comes not only from workers who are unemployed, but also from lower relative wages in the new jobs of workers who re-enter the workforce. We find that losses persist for (at least) three years following plant closure. To our knowledge, our results, which cover workers in 23 of the U.S. states, provide the most comprehensive available evidence on the impact of plant closure on workers.

We also uncover significant differences in the impact of closure on men and women. Controlling for worker and firm characteristics, we find that women face more severe wage cuts. One year following plant closure, women who found new jobs suffer an additional 5.2 percentage point wage loss compared to men. The difference is persistent and grows over time. Moreover, the gap survives careful controls for differences in the job choices of men and women both before and after plant closure.

Our findings are difficult to reconcile fully with the "human capital" model, in which differences between men and women reflect differences in firm-specific human capital investment. The gap appears for older and younger women, for high wage earners and low wage earners, and for "stayers" and "non-stayers." A potential explanation is continuing taste-based discrimination in hiring decisions. Consistent with this possibility, we find that men and women who move to a new firm with female leadership are treated more equally than workers who move to male-led firms.

We conclude not only that plant closures provide a significant adverse shock to workers, but also that they exacerbate the disadvantages of women relative to men in the marketplace. Moreover, our results suggest that improving the ability of women to break through the "glass ceiling" and attain top leadership positions may improve the opportunities of women lower in corporate hierarchy. Thus, changing leadership may be an important mechanism to change the culture of the firm in a direction which is friendlier to female workers. If differences in the treatment of men and women in the labor market reflect discrimination rather than differences in productivity, these changes may improve firm value by removing distortions in worker incentives.

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TABLE 1: SUMMARY STATISTICS - PLANT LEVEL

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

	Panel A: All Firms			Panel B: Multi-Unit Firms Only		
	Closing Plants in the LBD (N=143,370)	Random Plants in the LBD (N=655,929)	Closing Plants in the LBD Matched with the LEHD (N=12,439)	Closing Plants in the LBD (N=70,811)	Random Plants in the LBD (N=383,238)	Closing Plants in the LBD Matched with the LEHD (N=1,850)
Plant Employee	188	194	134	187	202	142
	(647)	(514)	(292)	(565)	(473)	(224)
Firm Emplyee	22,084	25,765	4,780	44,521	43,968	31,379
	(57,124)	(83,464)	(26,992)	(74,912)	(105,480)	(63,789)
Payroll (in $ 000's)	$5,299	$6,830	$2,333	$6,676	$7,590	$3,703
	($66,606)	($383,230)	($6,709)	($92,809)	($178,102)	($9,611)
% of Multi-Unit Firms	0.49	0.58	0.15			
% of Diversified Firms	0.39	0.42	0.10	0.79	0.71	0.69
Industry Distribution						
SIC = 1	0.03	0.03	0.06	0.02	0.02	
SIC = 2	0.05	0.06	0.05	0.08	0.09	
SIC = 3	0.05	0.07	0.04	0.09	0.10	
SIC = 4	0.05	0.04	0.03	0.08	0.08	N/A*
SIC = 5	0.18	0.20	0.19	0.30	0.36	
SIC = 6	0.06	0.04	0.03	0.10	0.07	
SIC = 7	0.13	0.09	0.16	0.18	0.13	
SIC = 8	0.11	0.14	0.09	0.13	0.15	
Geographic Distribution						
LEHD State	0.57	0.55	.	0.57	0.55	.
Region = NE	0.28	0.28	0.08	0.22	0.21	0.09
Region = MW	0.27	0.32	0.16	0.22	0.25	0.18
Region = S	0.31	0.29	0.23	0.25	0.24	0.26
Region = SW	0.16	0.15	0.19	0.12	0.12	0.19
Region = W	0.21	0.19	0.29	0.15	0.14	0.22
Region = RM	0.04	0.05	0.05	0.03	0.04	0.06
Yearly Distribution						
Year = 1994	0.08	0.10	0.08	0.07	0.10	0.05
Year = 1995	0.08	0.11	0.10	0.08	0.10	0.07
Year = 1996	0.11	0.11	0.12	0.11	0.11	0.13
Year = 1997	0.10	0.11	0.09	0.10	0.11	0.07
Year = 1998	0.11	0.11	0.13	0.11	0.12	0.12
Year = 1999	0.12	0.12	0.12	0.14	0.12	0.10
Year = 2000	0.12	0.12	0.14	0.13	0.12	0.22
Year = 2001	0.21	0.12	0.14	0.17	0.12	0.17

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

TABLE 2 SUMMARY STATISTICS - WORKER LEVEL

Panel A reports summary statistics for a random sample of workers from the LEHD data. Panel B reports summary statistics for workers matched to closing plants in the LBD. We report statistics for the overall sample and for the subsmples of worker from single-unit firms, multi-unit focused firms, and multi-unit diversified firms. We define multi-unit firms as firms which operate at least two distinct plants and diversified firms as firms which operate in more than one two-digit SIC code. Standard errors are reported in parantheses for continuous variables.

Panel A: Random Workers in the LEHD

	Overall (N=251,440)	Single-Unit Firms (N=63,173)	Multi-Unit Focused Firms (N=34,042)	Multi-Unit Diversified Firms (N =154,225)
Annual Wage	$34,999	$30,613	$33,527	$37,121
	(92,402)	(64,364)	(93,173)	(101,461)
Age	41.33	42.59	40.06	41.09
	(11.10)	(11.28)	(11.30)	(10.94)
Tenure (in yrs)	3.36	3.49	3.17	3.34
	(2.61)	(2.68)	(2.52)	(2.59)
Education (in yrs)	13.79	13.89	13.73	13.76
	(2.60)	(2.60)	(2.63)	(2.59)
% of Female	0.46	0.51	0.49	0.43
Race = Black	0.10	0.10	0.10	0.10
Race = Asian	0.04	0.03	0.04	0.04
Race = Hispanic	0.09	0.10	0.09	0.08
Race = Other	0.05	0.05	0.06	0.05
% of Foreigner	0.14	0.14	0.15	0.14

Panel B: Closing Workers in the LEHD

	Overall (N=461,449)	Single-Unit Firms (N=395,338)	Multi-Unit Focused Firms (N=15,947)	Multi-Unit Diversified Firms (N = 50,137)
Annual Wage	$29,933	$29,751	$28,642	$31,781
	(54,517)	(56,278)	(33,666)	(44,897)
Age	39.68	39.53	39.59	40.89
	(11.43)	(11.47)	(11.53)	(10.99)
Tenure (in yrs)	2.57	2.52	2.69	2.96
	(2.20)	(2.18)	(2.51)	(2.17)
Education (in yrs)	13.66	13.64	13.64	13.82
	(2.66)	(2.67)	(2.60)	(2.60)
% of Female	0.41	0.41	0.42	0.41
Race = Black	0.10	0.10	0.13	0.11
Race = Asian	0.04	0.04	0.05	0.04
Race = Hispanic	0.12	0.13	0.10	0.09
Race = Other	0.06	0.06	0.05	0.05
% of Foreigner	0.19	0.19	0.18	0.15

TABLE 3: WAGE CHANGES - THE AVERAGE TREATMENT EFFECT FOR THE TREATED

The table presents the estimated average treatment effect for the treated, where treatment is defined by plant closure. The outcome variable is the percentage change in the annualized wage from quarter t-2 to the quarter indicated in parentheses, where quarter t is the quarter of plant closure. Workers in closing plants are matched to workers in non-closing plants from the same state and 2-digit SIC code at the time of the closure using a nearest neighbour matching algorithm. Match variables are gender, race, age, firm size, and pre-closure annualized wage. Panel A reports the estimated coefficients using the entire sample (treating new wage as zero if unemployed) and Panel B reports the estimated coefficientsfor the subsample of workers who are re-employed following the closure.

Panel A. All Closing Workers

	Coef.	Std. Error	Z	Pr > |Z|
Wage (t+4)	-0.1669	0.0217	-7.68	0.000
Wage (t+8)	-0.0880	0.0221	-3.98	0.000
Wage (t+12)	-0.0663	0.0290	-2.28	0.023

Panel B. Re-employed Closing Workers

	Coef.	Std. Error	Z	Pr > |Z|
Wage (t+4)	-0.0633	0.0127	-4.99	0.000
Wage (t+8)	-0.0241	0.0138	-1.74	0.081
Wage (t+12)	-0.0339	0.0176	-1.92	0.054

TABLE 4: CHANGE OF STATE OR INDUSTRY

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

	Chg of State (1)	Chg of State (2)	Chg of Ind (3)	Chg of Ind (4)
Race = Black	-0.150 *	-0.154 *	0.042	0.043
	(0.080)	(0.080)	(0.037)	(0.037)
Race = Asian	-0.373 ***	-0.361 ***	-0.065	-0.059
	(0.086)	(0.086)	(0.055)	(0.055)
Race = Hispanic	-0.455 ***	-0.443 ***	-0.095 ***	-0.087 ***
	(0.058)	(0.058)	(0.033)	(0.033)
Race = Others Minorities	-0.286 ***	-0.280 ***	-0.086 ***	-0.087 ***
	(0.059)	(0.058)	(0.027)	(0.026)
Ln(Age)	-1.132 ***		-0.471 ***	
	(0.059)		(0.036)	
Female	-0.159 ***		0.041 *	
	(0.035)		(0.022)	
Age(25 to 35)		-0.235 ***		-0.222 ***
		(0.050)		(0.023)
Age(35 to 45)		-0.542 ***		-0.312 ***
		(0.057)		(0.030)
Age(45 to 55)		-0.669 ***		-0.364 ***
		(0.064)		(0.036)
Age(>55)		-0.987 ***		-0.494 ***
		(0.085)		(0.042)
Female * Age(<25)		0.041		0.050
		(0.067)		(0.032)
Female * Age(25 to 35)		-0.107 **		0.082 ***
		(0.047)		(0.025)
Female * Age(35 to 45)		-0.319 ***		0.013
		(0.058)		(0.027)
Female * Age(45 to 55)		-0.207 ***		0.028
		(0.066)		(0.033)
Female * Age(>55)		-0.205 *		-0.016
		(0.122)		(0.044)
Ln(Wage)	0.451 ***	0.430 ***	-0.250 ***	-0.250 ***
	(0.037)	(0.036)	(0.037)	(0.037)
Manager	0.216 **	0.202 **	0.298 ***	0.289 ***
	(0.085)	(0.084)	(0.050)	(0.050)
Ln(Tenure)	-0.342 ***	-0.348 ***	-0.239 ***	-0.241 ***
	(0.021)	(0.021)	(0.021)	(0.021)
Native	-1.119 ***	-1.110 ***	0.002	0.005
	(0.041)	(0.041)	(0.016)	(0.016)
Diversified Firms	0.398 ***	0.397 ***	0.713 ***	0.713 ***
	(0.134)	(0.134)	(0.155)	(0.155)
Ln(Firm Employment)	0.000	0.000	-0.118 ***	-0.119 ***
	(0.024)	(0.024)	(0.034)	(0.034)
Ln(Same_SIC_County)	-0.095 ***	-0.095 ***	-0.021	-0.021
	(0.024)	(0.024)	(0.022)	(0.022)
R-Squared	0.10	0.10	0.11	0.11
N	343,050	343,050	343,206	343,206

TABLE 5: CHANGES IN WAGE FOR WORKERS IN CLOSING PLANTS

The dependent variable is the difference in the natural logarithms of the pre- and post- plant closure wage. The pre-closure wage as the annualized wage two quarters prior to plant closure and the post-closure wage as the annualized wage four quarters following the plant closure. The omited race caegory is "White". Age is worker age. Wage is the annualized wage. Tenure is measured as the number of quarters that a worker has spent in the firm. All worker-level variables are measured two quarters prior to plant closure. Manager is defined as the highest paid employee in the plant. We define diversified firms (Diversified) as firms that operate in at least two distinct two-digit SIC codes. Firm Employment is measured as the total number of workers for the entire firm (across all its plants). In all specifications, we control for state, industry and year fixed effects. In addition, we control for plant fixed effects in column (2), and control for the pair (closing plant & new firm) fixed effect in column (3). Standard errors are clustered by closing plant in column (1) and (2) and by pair (closing plant & new firm) in column (3), and are reported in parentheses. *, ** and *** represent significance at 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)
Race = Black	-0.050 ***	-0.042 ***	-0.034 ***
	(0.004)	(0.003)	(0.003)
Race = Asian	0.002	-0.003	-0.014 ***
	(0.006)	(0.004)	(0.004)
Race = Hispanic	-0.039 ***	-0.032 ***	-0.031 ***
	(0.003)	(0.003)	(0.003)
Race = Others Minorities	-0.014 ***	-0.015 ***	-0.014 ***
	(0.003)	(0.003)	(0.002)
Female	-0.041 ***	-0.052 ***	-0.037 ***
	(0.002)	(0.002)	(0.002)
Ln(Age)	-0.095 ***	-0.071 ***	-0.072 ***
	(0.004)	(0.003)	(0.003)
Ln(Wage)	-0.088 ***	-0.130 ***	-0.116 ***
	(0.004)	(0.004)	(0.004)
Manager	-0.040 ***	0.020 ***	0.026 ***
	(0.007)	(0.007)	(0.008)
Ln(Tenure)	-0.005 ***	-0.005 ***	-0.015 ***
	(0.002)	(0.001)	(0.001)
Diversified	-0.036 ***		
	(0.010)		
Ln(Firm Employment)	0.005 ***		
	(0.002)		
R-Squared	0.046	0.170	0.640
N	359,537	359,537	359,537

TABLE 6: CHANGES IN WAGE BY AGE

The dependent variable is the difference in the natural logarithms of the pre- and post- plant closure wage. We measure the pre-closure wage as the annualized wage two quarters prior to plant closure and the post-closure wage as the annualized wage four, eight, or twelve quarters following the plant closure (indicated in parentheses). The omited race caegory is "White". Age is worker age. Wage is the annualized 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. The ommited age category is age less than 25. In all specifications, we control for state, industry, year, and closing plant fixed effects. All standard errors are clustered by closing plant, and are reported in parentheses. *, ** and *** represent significance at 10%, 5% and 1% level, respectively.

	Wage Change (t+4)	Wage Change (t+8)	Wage Change (t+12)
	(1)	(2)	(3)
Race = Black	-0.045 ***	-0.051 ***	-0.057 ***
	(0.003)	(0.003)	(0.004)
Race = Asian	-0.004	0.002	0.002
	(0.004)	(0.005)	(0.005)
Race = Hispanic	-0.034 ***	-0.042 ***	-0.048 ***
	(0.003)	(0.003)	(0.004)
Race = Other Minorities	-0.016 ***	-0.011 ***	-0.006 *
	(0.003)	(0.003)	(0.004)
Ln(Wage)	-0.136 ***	-0.158 ***	-0.174 ***
	(0.004)	(0.004)	(0.005)
Manager	0.025 ***	0.041 ***	0.039 ***
	(0.007)	(0.008)	(0.010)
Ln(Tenure)	-0.006 ***	-0.017 ***	-0.024 ***
	(0.001)	(0.001)	(0.002)
Age(25 to 35)	0.007 **	-0.015 ***	-0.035 ***
	(0.003)	(0.004)	(0.004)
Age(35 to 45)	-0.009 ***	-0.048 ***	-0.088 ***
	(0.003)	(0.004)	(0.004)
Age(45 to 55)	-0.029 ***	-0.082 ***	-0.133 ***
	(0.003)	(0.004)	(0.005)
Age(>55)	-0.100 ***	-0.177 ***	-0.257 ***
	(0.005)	(0.006)	(0.008)
Female * Age(<25)	-0.070 ***	-0.100 ***	-0.123 ***
	(0.004)	(0.005)	(0.006)
Female * Age(25 to 35)	-0.068 ***	-0.087 ***	-0.100 ***
	(0.003)	(0.003)	(0.004)
Female * Age(35 to 45)	-0.046 ***	-0.058 ***	-0.064 ***
	(0.002)	(0.003)	(0.003)
Female * Age(45 to 55)	-0.045 ***	-0.052 ***	-0.055 ***
	(0.003)	(0.003)	(0.004)
Female * Age(>55)	-0.032 ***	-0.047 ***	-0.058 ***
	(0.004)	(0.006)	(0.008)
R-Squared	0.172	0.198	0.271
N	359,537	316,950	275,109

TABLE 7: CHANGES IN WAGE BY PRE-CLOSURE WAGE

The dependent variable is the difference in the natural logarithms of the pre- and post- plant closure wage. We measure the pre-closure wage as the annualized wage two quarters prior to plant closure and the post-closure wage as the annualized wage four, eight, or twelve quarters following the plant closure (indicated in parentheses). The omited race caegory is "White". Age is worker age. Wage is the annualized 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. The ommitted wage category is wages less than 25K. In all specifications, we control for state, industry, year, and closing plant fixed effects. All standard errors are clustered by closing plant, and are reported in parentheses. *, ** and *** represent significance at 10%, 5% and 1% level, respectively.

	Wage Change (t+4)	Wage Change (t+8)	Wage Change (t+12)
	(1)	(2)	(3)
Race = Black	-0.036 ***	-0.041 ***	-0.046 ***
	(0.003)	(0.003)	(0.004)
Race = Asian	0.000	0.006	0.006
	(0.004)	(0.004)	(0.005)
Race = Hispanic	-0.025 ***	-0.033 ***	-0.039 ***
	(0.002)	(0.003)	(0.004)
Race = Other Minorities	-0.012 ***	-0.007 **	-0.003
	(0.003)	(0.003)	(0.003)
Ln(Age)	-0.079 ***	-0.146 ***	-0.213 ***
	(0.003)	(0.003)	(0.004)
Manager	-0.001	0.006	-0.002
	(0.007)	(0.008)	(0.010)
Ln(Tenure)	-0.008 ***	-0.019 ***	-0.026 ***
	(0.001)	(0.001)	(0.002)
Wage(20K-30K)	-0.076 ***	-0.091 ***	-0.102 ***
	(0.002)	(0.003)	(0.004)
Wage(30K-40K)	-0.108 ***	-0.128 ***	-0.145 ***
	(0.003)	(0.004)	(0.005)
Wage(40K-50K)	-0.127 ***	-0.154 ***	-0.174 ***
	(0.007)	(0.009)	(0.010)
Wage(50K-75K)	-0.143 ***	-0.167 ***	-0.192 ***
	(0.006)	(0.007)	(0.009)
Wage(75K-100K)	-0.169 ***	-0.195 ***	-0.204 ***
	(0.008)	(0.009)	(0.012)
Wage(>100K)	-0.320 ***	-0.350 ***	-0.379 ***
	(0.013)	(0.016)	(0.020)
Female * Wage(<20K)	-0.057 ***	-0.070 ***	-0.080 ***
	(0.002)	(0.003)	(0.003)
Female * Wage(20K-30K)	-0.038 ***	-0.052 ***	-0.059 ***
	(0.003)	(0.003)	(0.004)
Female * Wage(30K-40K)	-0.030 ***	-0.042 ***	-0.052 ***
	(0.004)	(0.004)	(0.005)
Female * Wage(40K-50K)	-0.032 ***	-0.043 ***	-0.053 ***
	(0.006)	(0.007)	(0.010)
Female * Wage(50K-75K)	-0.041 ***	-0.051 ***	-0.054 ***
	(0.006)	(0.007)	(0.009)
Female * Wage(75K-100K)	-0.050 ***	-0.080 ***	-0.102 ***
	(0.014)	(0.015)	(0.020)
Female * Wage (>100K)	-0.047 **	-0.063 ***	-0.076 ***
	(0.019)	(0.023)	(0.027)
R-Squared	0.164	0.188	0.189
N	359537	316950	275109

TABLE 8: CHANGES IN WAGE FOR STAYERS ONLY

The sample is workers who have stayed in the closing plant for at least 5 years prior to the closure. The dependent variable is the difference in the natural logarithms of the pre- and post- plant closure wage. We measure the pre-closure wage as the annualized wage two quarters prior to plant closure and the post-closure wage as the annualized wage four, eight, or twelve quarters following the plant closure (indicated in parentheses). The omited race caegory is "White". Age is worker age. Wage is the annualized 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. In all specifications, we control for state, industry, year, and closing plant fixed effects. All standard errors are clustered by closing plant, and are reported in parentheses. *, ** and *** represent significance at 10%, 5% and 1% level, respectively.

	Wage Change (t+4)	Wage Change (t+8)	Wage Change (t+12)
	(1)	(2)	(3)
Race = Black	-0.026 ***	-0.025 ***	-0.026 ***
	(0.006)	(0.008)	(0.009)
Race = Asian	-0.011	-0.021 *	-0.008
	(0.009)	(0.011)	(0.014)
Race = Hispanic	-0.031 ***	-0.038 ***	-0.047 ***
	(0.005)	(0.006)	(0.008)
Race = Others Minorities	0.002	0.007	0.005
	(0.005)	(0.006)	(0.008)
Female	-0.033 ***	-0.041 ***	-0.042 ***
	(0.004)	(0.004)	(0.005)
Ln(Age)	-0.136 ***	-0.211 ***	-0.279 ***
	(0.007)	(0.009)	(0.011)
Ln(Wage)	-0.107 ***	-0.124 ***	-0.138 ***
	(0.007)	(0.008)	(0.009)
Manager	-0.023	0.006	-0.023
	(0.014)	(0.016)	(0.021)
Ln(Tenure)	-0.008	-0.015	-0.031 **
	(0.010)	(0.013)	(0.016)
R-Squared	0.249	0.236	0.231
N	52983	47805	40115

TABLE 9: WAGE CHANGES AND FEMALE LEADSHIP

The dependent variable is the difference in the natural logarithms of the pre- and post- plant closure wage. We measure the pre-closure wage as the annualized wage two quarters prior to plant closure and the post-closure wage as the annualized wage four quarters following the plant closure. The omited race caegory is "White". Age is worker age. Wage is the annualized 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. Pct_FMgr is the percentage of females in the top-five positions (based on wage) in the post-closure employer. D_FMgrFirm is an indicator variable that equals 1 if the variable Pct_FMgr for the new firm is in the highest quartile (>=0.4). In all specifications, we control for state, industry, year, and closing plant fixed effects. All standard errors are clustered by closing plant, and are reported in parentheses. *, ** and *** represent significance at 10%, 5% and 1% level, respectively.

	(1)	(2)
Race = Black	-0.043 ***	-0.043 ***
	(0.004)	(0.004)
Race = Asian	-0.005	-0.005
	(0.004)	(0.004)
Race = Hispanic	-0.035 ***	-0.035 ***
	(0.003)	(0.003)
Race = Other Minorities	-0.015 ***	-0.015 ***
	(0.003)	(0.003)
Ln(Age)	-0.073 ***	-0.073 ***
	(0.004)	(0.004)
Female	-0.061 ***	-0.057 ***
	(0.035)	(0.002)
Ln(Wage)	-0.146 ***	-0.145 ***
	(0.005)	(0.005)
Manager	0.028 ***	0.027 ***
	(0.009)	(0.009)
Ln(Tenure)	-0.007 ***	-0.007 ***
	(0.002)	(0.002)
Pct_FMgr	-0.198 ***	
	(0.010)	
Female * Pct_FMgr	0.047 ***	
	(0.009)	
D_FMgrFirm		-0.116 ***
		(0.009)
Female * D_FMgrFirm		0.029 ***
		(0.008)
R-Squared	0.20	0.19
N	244,007	244,007