

Corporate Discrimination, Competition, and Shareholder Wealth

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Abstract

We study novel data on the universe of employment discrimination lawsuits filed in federal court against U.S. public corporations between 1992 and 2018. Shareholder value drops by \$30 million, on average, in the three days surrounding a discrimination lawsuit filing. However, we find no evidence that discrimination rates are related to product market competition, financial resources, governance, or CEO turnover. Instead, workplace discrimination is highly persistent and correlates with slow-moving proxies of firm culture, such as headquarter location. These results suggest that corporate discrimination is largely determined by the beliefs and preferences of employees, rather than a firm's economic environment.

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1. Introduction

In his 1962 book *Capitalism and Freedom*, Milton Friedman argues that a competitive market “protects men from being discriminated against in their economic activities for reasons that are irrelevant to their productivity.” Business decisions motivated by animus against people of a certain race, gender, or religion impose a cost that lowers profits and eventually drives the discriminating firm out of the market. Yet, in 2020, the U.S. Equal Employment Opportunity Commission received 67,448 workplace discrimination charges and secured \$439.2 million for victims. Moreover, the Center for American Progress estimates that workplace discrimination costs U.S. firms \$64 billion annually due to higher employee turnover and lower productivity. In this paper, we test whether firm-level economic conditions affect the frequency of employment discrimination lawsuits and assess the implications for investors.

Federal civil rights law prohibits employment discrimination based on eight protected characteristics: race, color, religion, sex, national origin, age, disability, and genetic information. An individual can file a discrimination complaint if they believe that one of these protected characteristics was the subject of “pervasive and severe” harassment or was the basis of an adverse employment action (e.g., bad job assignment or working conditions, lack of hiring or promotion, demotion or firing, unfair compensation or benefits, lack of training or accommodations, etc.). Becker (1957) argues that some employers engage in this behavior because they hold a “taste for discrimination” that provides non-pecuniary benefits. In taste-based models of discrimination such as Becker (1957), employers maximize a utility function that is the sum of profits plus the monetary value of utility they derive from engaging in discriminatory behavior. Employers may be willing to forfeit profits to engage in discriminatory behavior, but these firms will always have higher net unit costs than non-discriminating employers. In general, firms with a lower than

average “taste” for discrimination are more profitable and tend to expand relative to other firms. In the long run with free entry or constant returns to scale, non-discriminating firms will arbitrage the cost of discrimination, expand, and eventually compete discriminating firms out of the market.¹

We test this economic framework using the universe of employment discrimination lawsuits recorded in the Federal Judicial Center’s (FJC) integrated database between 1992 and 2018. The FJC collects information on the nature, jurisdiction, plaintiff, defendant, and outcome for all civil lawsuits filed in district court. We match defendant names in “Civil Rights-Jobs” lawsuits to firms in the CRSP-Compustat merged database and find that 27.8 percent of U.S public corporations faced at least one employment discrimination lawsuit in federal court over our 26-year sample period.² Although there is considerable cross-sectional variation in discrimination rates across the size and industry distribution, the percentage of corporations facing a discrimination lawsuit remained remarkably stable over time. Wholesale/retail firms are consistently among the most likely to be sued and healthcare firms are consistently among the least, with little variation relative to the business cycle.

Investors appear to view discrimination lawsuits as costly: the mean (median) three-day cumulative abnormal return (CAR) around a suit filing is -0.03% (-0.09%), which corresponds to a roughly \$30 million loss in shareholder value for the average firm. While it is impossible to disentangle the portion of this value destruction that is attributable to legal fees, reputational penalties, and/or lost productivity, the negative and significant stock price reaction suggests that

¹ Phelps (1972) and Arrow (1973) argue that discrimination may be value-increasing if it occurs for “statistical” reasons (i.e., hiring managers use protected characteristics as a signal of worker quality and make hiring/pay decisions based on these observable signals). This hypothesis is less plausible in our setting because our focus is on all civil rights employment lawsuits, including harassment cases. Moreover, workplace discrimination is subject to legal penalties regardless of motivation, making it costly ex ante.

² We note that lawsuit filings do not perfectly capture underlying discrimination because there may exist some cases where an employee is discriminated against but does not file a federal lawsuit and other cases where an employee files a lawsuit despite no underlying discrimination.

discrimination lawsuits impose substantial costs on the typical firm. Since these costs lower firm profits, taste-based models predict that discrimination rates should be positively correlated with financial slack. Hart (1983) argues that product market competition reduces slack and can force managers to maximize firm value rather than consume private benefits. Similarly, a reduction in financial slack should limit the scope for employers to indulge in their taste for discrimination rather than maximize profits.

To test this hypothesis, we study three natural experiments that induce a plausibly exogenous change in financial slack. First, we examine a change in U.S.-China trade relations that heterogeneously increased product market competition across manufacturing industries. In 2000, U.S. Congress unexpectedly voted to grant China permanent Most-Favored Nation trade status. Pierce and Schott (2016) and Griffin (2021), among others, show that this “China trade shock” decreased profitability and increased exit rates depending on firms’ exposure to the shock. Second, we study the Interstate Banking and Branching Efficiency Act (IBBEA). Rice and Strahan (2010) and Basu, Kaviani, and Maleki (2021) show that IBBEA deregulations increased banking competition, leading to lower deposit concentration and interest rates. Finally, we examine a shock that increased financial slack: the 2004 American Jobs Creation Act (AJCA). Faulkender and Petersen (2012) show that the AJCA’s temporary decrease in repatriation tax created a cash windfall for U.S. firms with significant earnings held by foreign subsidiaries. If financial slack broadens the scope for costly discrimination, we expect the frequency of civil rights employment lawsuits to decrease at firms facing higher product market competition after the China trade shock and IBBEA and increase at firms receiving financial constraints relief from the AJCA.

In all three natural experiments, we find no statistically significant or economically meaningful relation between discrimination lawsuit frequency and the economic shock. For

example, the coefficient capturing the effect of a one standard deviation increase in exposure to the China trade shock on the number of discrimination lawsuits is -0.002, which is 1.8% of the magnitude of the sample mean and 0.5% of the sample standard deviation. Importantly, the standard errors are small (0.007), implying that the estimated effect is tightly bound around zero. Similarly, the effect of IBBEA deregulation is -0.000 with a standard error of 0.018, and the effect of the AJCA tax windfall is -0.019 with a standard error of 0.026. Although AJCA estimates have the largest economic magnitudes, they are statistically insignificant and directionally opposite to predictions of taste-based models of discrimination. These null results are particularly striking because prior research shows the AJCA cash windfall lessened cost-cutting behavior along other dimensions. For example, Xu and Kim (2021) show that firms reduce toxic emissions after the AJCA cash flow shock and, using the same regression specification as ours, Cohn and Wardlaw (2016) show that the AJCA led to a decrease in workplace injury rates. Thus our (non)results suggest that firms either view workplace discrimination to be less costly than workplace injuries or lack sufficient power to align incentives so employees do not discriminate.

Together, the three natural experiments provide no evidence that workplace discrimination is related to firm-level economic conditions. Therefore, as an alternative hypothesis, we conjecture that discrimination may be a sticky variable that is ingrained in culture rather than a dynamic variable that can be optimized or competed away. To test this hypothesis, we run ordinary least squares (OLS) regressions that relate employment discrimination lawsuit frequency to lagged firm, industry, and headquarter-area characteristics.

These regressions provide three pieces of evidence that link workplace discrimination with culture. First, corporations headquartered in the Southeastern U.S. are more than twice as likely to face a discrimination lawsuit than firms headquartered in the Northwest. Second, discrimination

suits are significantly more likely in rural areas and in counties that have a high percentage of black residents. A one standard deviation increase in the percentage of black residents is associated with a 13.5% higher number of discrimination cases relative to the sample mean. Finally, our estimates reveal a positive relation between the number of discrimination suits and corporate social responsibility (CSR) concerns, while once again producing no evidence in support of taste-based model predictions regarding economic fundamentals (proxied by prior stock returns, return on assets, leverage, cash holdings, and industry markups).

Thus far, our results cast doubt on Milton Friedman's claim that competition provides a "market solution" that will eventually reduce discrimination. As a final test of Friedman's conjecture, we examine i) how discrimination lawsuit frequency varies with corporate governance, ii) whether CEOs are held accountable for recent discrimination lawsuits, and iii) if firms reduce future discrimination in response to previous lawsuits. If discrimination is related to financial slack like other managerial private benefits, we expect strong corporate governance to temper the frequency of lawsuits even if product market competition is lacking (Giroud and Mueller 2010, 2011). However, we find no evidence that strong shareholder rights, blockholders, or board independence lowers the frequency of discrimination suits. Moreover, we find no evidence that the three-year rolling number of employment discrimination lawsuits is associated with future CEO dismissal. Instead, discrimination appears to be highly persistent, with the number of past discrimination lawsuits positively related to future suits. Overall, these results suggest that the consequences of employment discrimination lawsuits are limited, allowing discrimination to persist regardless of economic environment.

This paper contributes to a burgeoning corporate finance literature on the value of diversity, equity, and inclusion. Our finding that shareholder value drops by 0.03-0.09% in the three days

around a discrimination lawsuit filing provides direct empirical evidence that discrimination destroys firm value. This direct evidence on the cost of discrimination complements contemporaneous working papers on the benefits of an inclusive culture. For example, Mkrtchyan, Sandvik, and Zhu (2021) find that shareholder value increases by 0.08-0.12% when CEOs speak out about social and political issues, and Lins, Roth, Servaes, and Tamayo (2021) show that firms with non-sexist corporate culture (i.e., those with women among the five-highest paid executives) earned excess returns of 1.6% during the Harvey Weinstein and #MeToo events.

Our paper also adds to our understanding of how product market competition and financial constraints affect corporate decision-making. While Cohn and Wardlaw (2016) and Xu and Kim (2021) show that financial constraints are related to cost-cutting on workplace safety and toxic emissions respectively, we find no evidence that financial resources affect discrimination rates. Instead, our results suggest that discrimination is a highly persistent construct driven by employee culture and is unlikely to be mitigated by financial market discipline.

Finally, our paper contributes to the broader economics literature on employment discrimination. Bertrand and Mullainathan (2004) show that employers differentially treat resumes that are identical except for the perceived race of the job applicant. Kline, Rose, Walters (2021) use similar methodology to show that discrimination appears to be concentrated among some employers rather than dispersed across all employers. Ashenfelter and Hannan (1986) and Black and Strahan (2001) show that product market competition is associated with a higher share of women in the workforce and a lower gender wage gap in the banking industry. Our contrasting results imply that wages are easier to change in response to economic conditions than employee behavior. In sum, our results suggest that workplace discrimination is largely driven by the culture of firm employees, rather than the firm's financial resources or competitive environment.

2. Data

We obtain public firm data from the CRSP-Compustat Merged Database and federal lawsuit data from the Federal Judicial Center (FJC) Integrated Database. The FJC database contains the universe of federal litigation cases and is maintained according to statute 28 U.S.C. §§ 620–629. From this dataset, we collect all cases classified as “Civil Rights-Jobs” lawsuits (Nature of Suit 442), which we term “discrimination” lawsuits. For each lawsuit, we clean and merge defendant names to our set of Compustat firms. This procedure yields a sample of 55,969 employment discrimination lawsuits filed against 3,614 U.S. public companies between 1992 and 2018. Figure 1 offers a graphical comparison of the number of discrimination lawsuits in the FJC database and our FJC-Compustat merged dataset. While there is substantial time-series variation in the number of lawsuits in the FJC database, this variation is broadly mimicked in our merged sample suggesting no systemic biases in our matching procedure. From this data, we calculate our main dependent variable: the annual number of discrimination lawsuits per firm.

Table 1 presents descriptive statistics for our FJC-Compustat merged sample, which consists of 105,974 firm-year observations from 11,515 U.S. public companies with data available in the CRSP-Compustat Merged Database between 1992 and 2018. We find that 27.8 percent of firms, and 12.6 percent of firm-years, face at least one discrimination lawsuit. The Healthcare, Medical Equipment, and Drugs industry and the Business Equipment: Computers, Software, Electronics industry have the lowest percentage of firms experiencing at least one lawsuit across Fama-French 12 industry classifications, at 16.8 and 19.1 percent respectively. The Utilities industry has the highest incidence of discrimination lawsuits with 41.5 percent of all utility firms experiencing at least one lawsuit. Figure 2 shows that the relative ranking of industry-level discrimination rates has remained generally constant over time.

Table 1 reports a strong correlation between firm size and discrimination lawsuit frequency, an intuitive finding given that larger firms typically have more employees and consequently more opportunities for a lawsuit. We also find that firms with credit ratings have a substantially higher incidence of discrimination lawsuits than firms without credit ratings. Figure 3 shows that these relations hold on a per employee basis. Given that firm size and credit ratings are two of the most reliable proxies of financial constraints (Hadlock and Pierce 2010), these correlations provide initial support for taste-based models that predict a negative relation between discrimination rates and financial constraints. However, we note that these correlations are likely confounded by omitted variables, such as firm complexity, that could also affect discrimination rates and therefore caution against interpreting these relations absent a natural experiment. Finally, Figure 4 plots the fraction of firm-year observations facing an employment discrimination lawsuit by headquarter state. The plot reveals evidence of a strong geographic component in discrimination lawsuit frequency, with firms headquartered in the South and Rustbelt exhibiting significantly higher discrimination rates than firms in the Northeast and West.

3. Stock market response to discrimination lawsuits

In this section, we conduct an event study to examine the stock price reaction around federal discrimination lawsuit filings. Taste-based models define discriminatory behavior as actions devoid of economic merit that are driven by animus against a certain group of people. As such, we expect discrimination lawsuits to be met with a negative market response. However, we note that “statistical discrimination” models of Phelps (1972) and Arrow (1973) describe discrimination as potentially profit-maximizing in the context of hiring and wage-setting if these signals allow employers to better discern the quality of job applicants. Under this scenario, the revelation of discrimination could be met with a positive stock price reaction.

Table 2 reports the mean and median percentage cumulative abnormal return for our full sample of 55,969 employment discrimination lawsuits filed against 3,614 U.S. public companies between 1992 and 2018. We calculate market model and Fama-French 3 Factor CARs using CRSP value-weighted index returns and a one-year estimation window (252 trading days) ending one month (20 trading days) before the [-1, +1] or [-2, +2] event window. Across all models and event-windows, average CARs vary from -0.033% to -0.056%, which is roughly equivalent to a value reduction of \$12 million to \$20 million. Median CARs range slightly lower from -0.092% to -0.096%, or a value reduction of about \$34 million. All estimates are statistically significant from zero, typically at greater than the 1% level. Together, these results provide direct evidence that discriminatory behavior destroys shareholder value.

To close this section, we highlight two limitations regarding the interpretation of our event study results. First, our estimates capture the market reaction to discrimination lawsuits which, in addition to revealing underlying discrimination, can impose legal and reputational penalties that potentially alter future firm behavior. If litigation reduces future discriminatory behavior, the filing of a discrimination lawsuit could increase firm value by limiting future animus-based inefficiencies (Ashenfelter 1972). Second, the stock price reaction to a lawsuit filing may be dampened via market anticipation or information leakage. Thus, the effects measured here are likely a lower bound on the value destroyed by workplace discrimination.

4. Exogenous changes to business conditions and discrimination lawsuits

Given our evidence that shows discrimination is costly, we predict that a reduction in financial slack should limit its occurrence. To test this hypothesis, we study three natural experiments that induce a plausibly exogenous change in financial slack. In Section 4.1, we investigate the effect of an increase in Chinese import competition on discrimination lawsuits

among U.S. manufacturers. In Section 4.2, we examine changes in discrimination lawsuit frequency in the financial services industry around interstate branching deregulation that increased banking competition. Finally, in Section 4.3, we study changes in discrimination lawsuit frequency around the American Jobs Creation Act, which provided a positive cash flow shock to U.S. firms with profitable foreign subsidiaries. In all three natural experiments, we find a null result: the occurrence of discrimination lawsuits appears to be unaffected by changes in firms' economic conditions.

4.1 China trade shock and corporate discrimination

In 2000, U.S. Congress granted China *permanent* Most Favored Nation (MFN) trade status. Prior to this year, China held *temporary* MFN status, which would revert to Non-Market Economy (NME) status – accompanied by substantially higher tariff rates – pending Congressional approval. China's receipt of permanent MFN status in 2000 effectively resolved years of tariff rate uncertainty and led to a massive increase in U.S. investment in China and U.S. imports from China. In this section, we examine whether this increase in import competition reduced U.S. firms' propensity to discriminate, as hypothesized by Becker (1957).

To study this question, we follow the empirical framework of Pierce and Schott (2016) and estimate the following difference-in-difference specification:

$$Y_{i,t} = \beta \cdot Post_t \cdot China\ Trade\ Shock\ Exposure_i + \theta \cdot Post_t \cdot Controls_i + \gamma \cdot Firm_i + \tau \cdot Year_t + \varepsilon_{i,t}.$$

$Y_{i,t}$ is the log of one plus the number of discrimination cases or discrimination cases per employee at firm i in year t . $Post_t$ is an indicator that turns from zero to one in year 2000, when China attained permanent MFN status. This variable captures aggregate trends in discrimination suits. *China Trade Shock Exposure_i* is a continuous variable that measures the potential tariff hike faced by firm i 's 6-digit NAICS industry prior to 2000 (i.e., the gap between MFN and NME tariff rates).

Our primary estimate of interest is the coefficient on the interaction of these two variables, β , which captures the effect of increased Chinese import competition following China's permanent MFN status conditional on a firm's exposure to the event. Taste-based models of discrimination predict β to have a negative statistically significant estimate, implying that firms' reduction in discriminatory behavior is correlated with the degree to which they were impacted by increased competition. Finally, *Controls_{it}* are variables that control for confounding factors at the industry level, firm (*Firm_{it}*) and year (*Year_{it}*) are fixed effects included in all specifications, standard errors are clustered at the 6-digit NAICS industry level, and the sample is restricted to only manufacturing firms.

Panel A of Table 3 presents summary statistics for the sample used in these regressions and Panel B presents regression results. In Columns (1) and (2) of Panel B, the dependent variable is the log of one plus the number of discrimination cases. We estimate β (i.e., the interaction term on *Post * China Trade Shock Exposure*) to equal -0.002 (*t*-stat = -0.286). Hence, contrary to the Becker (1957) hypothesis, we fail to reject the null that increased import competition from China has no effect on firms' level of discrimination lawsuits. In Column (2), we add a control for the post-2000 technology bubble (*High Tech Industry*), which is an indicator that equals one if the industry is classified in the Computers and Electronics Manufacturing Subsector (NAICS 334), and a control for the general decline in America's unskilled-labor intensive industries (*Unskilled Labor Percentage*), which is the fraction of industry employees that are production workers. The addition of these controls does not change our estimate of β . We repeat the same specifications in Columns (3) and (4) using discrimination suits per employee as the dependent variable. In both columns, our estimates of β are statistically insignificant with magnitudes near 0 ($\beta = 0.001$, *t*-stat = 0.333). Overall, our analysis suggests that the China trade shock, which represents one of

the largest increases in product market competition identified by the economics and finance literature, has no significant effect on employment discrimination rates.³

4.2 Banking deregulation and corporate discrimination

Black and Strahan (2001) were among the first to examine the effect of an increase in competition on discrimination. They find that increased competition following interstate banking and branching deregulation of the 80s and 90s lead to a reduction in the male-female wage gap. In 1994, the Interstate Banking and Branching Efficiency Act was passed which, in theory, ended restrictions on interstate bank expansion, but in practice, granted states substantial rights to prevent branch expansion. Over time, states lifted these barriers in a staggered fashion. Following Rice and Strahan (2010), we exploit these state-level changes in regulatory constraints to identify the effect of banking competition on the prevalence of discrimination lawsuits against banks headquartered in deregulating areas.

Specifically, we estimate the following two-way fixed effects regression:

$$Y_{i(s),t} = \beta \cdot Deregulation\ Index_{i(s)} + \theta \cdot Deregulation\ Index_{i(s)} \cdot Unit\ Banking_{i(s)} + \gamma \cdot Firm_i + \tau \cdot Year_t + \varepsilon_{i,t}$$

where $Y_{i(s),t}$ is the log of one plus the number of discrimination cases or discrimination cases per employee in firm i headquartered in state s in year t . $Deregulation\ Index_{i(s)}$ is an index that ranges from 0 for fully regulated states to 4 for fully deregulated states, the inverse of the index used in Rice and Strahan (2010), so that an increase in the index corresponds with an increase in banking competition for that state. $Unit\ Banking_{i(s)}$ is an indicator that equals one if the state began with a complete prohibition on branching and zero if the state only limited branching to the city of the head office. If Becker's theory holds in this context, we expect β

³ Prior research shows that Chinese product market competition led to a decline in U.S. manufacturing employment (Autor, Dorn, and Hanson 2013), induced plant exit (Pierce and Schott 2016), and impeded the entry and performance of U.S. manufacturers (Griffin 2021).

and/or θ to have a negative statistically significant estimate, signifying that banks reduced discriminatory behavior as their local market increased in competitiveness. Finally, firm (*Firmi*) and year (*Yeart*) fixed effects are included in all specifications, standard errors are clustered at the industry level, and the sample is restricted to only U.S. Depository Institutions.

Panel A of Table 4 presents summary statistics for the sample used in these regressions and Panel B presents regression results. As in the previous subsection, we find no evidence that an increase in competition led to a decrease in discrimination lawsuits. Across all specifications, we estimate β to range from -0.003 to 0.004, each with standard errors tightly bound around 0. Similarly, θ is estimated near 0 with standard errors around 0. Based on these estimates, we fail to reject the null that competition has no effect on discrimination, as measured by civil rights employment lawsuits. Reconciling our results with Black and Strahan (2001), we conclude that wages are easier to change in response to economic conditions than employee behavior.

4.3 American Jobs Creation Act and corporate discrimination

In this subsection, we examine the effect of a large cash windfall on discrimination lawsuit frequency. Theory suggests that a reduction in financial slack will redirect managers away from consuming private benefits in favor of profit maximizing behavior (e.g., Hart 1983). An increase in financial slack should have the opposite effect, allowing managers to engage in more discriminatory behavior. To test this hypothesis, we examine the 2004 American Jobs Creation Act, which allowed firms to temporarily pay a lower repatriation tax. This Act created a large cash inflow for U.S. firms with significant earnings held by foreign subsidiaries (Faulkender and Petersen 2012). Cohn and Wardlaw (2016) and Xu and Kim (2021) use this quasi-natural experiment to show that firm injury rates and toxic emissions significantly decreased after the AJCA relieved financial constraints. We replicate their regression specifications with

discrimination lawsuit frequency as our outcome variable.

To do so, we restrict our sample to the two years before (2002 and 2003) and after (2005 and 2006) the AJCA was implemented and denote all firms with positive foreign profits (Compustat variable $PIFO > 0$) from 2001 to 2003 as treated firms. We match these treated firms with zero-foreign-profit control firms according to 2001 market value of equity, number of employees, stock return, ROA, leverage, and cash-to-assets. We then estimate OLS regressions using the following difference-in-difference specification:

$$Y_{i,t} = \beta \cdot Post_t \cdot AJCA\ Exposure_i + \theta \cdot Post_t \cdot AJCA\ Exposure_i \cdot Leverage_i + \gamma \cdot Firm_i + \tau \cdot Industry_i \cdot Year_t + \omega \cdot State_i \cdot Year_t + \varepsilon_{i,t}^4$$

$Y_{i,t}$ is the log of one plus the number of discrimination cases or discrimination cases per employee filed against firm i in year t . $Post_t$ is a variable equal to one for observations in 2005 and 2006 and zero for observations in 2002 and 2003. $AJCA\ Exposure_i$ is equal to one if firm i has positive foreign profits from 2001 through 2003 and zero otherwise. Our primary estimate of interest is the coefficient on the interaction of these two variables, β , which captures the effect of the AJCA shock for firms with positive foreign profits. The coefficient θ further interacts the AJCA shock with leverage, capturing the effect of the AJCA cash windfall on firms conditional on their leverage. This triple interaction tests to see if the effect of a cash windfall on discrimination is strongest for firms with the greatest financial constraints. Thus, if greater financial slack allows for more discrimination, we expect β or θ to be positive. Finally, firm ($Firm_i$), industry-year ($Industry_i \cdot Year_t$), and state-year ($State_i \cdot Year_t$) fixed effects are included in all specifications, and standard errors are clustered at the firm level.

Panel A of Table 5 presents summary statistics for the sample used in these regressions

⁴ Additional interaction terms are included in the regression, following Cohn and Wardlaw (2016), but suppressed here for clarity.

and Panel B presents regression results. Our estimates of β and θ are statistically insignificant with point estimates near 0 for all specifications in Panel B. Thus, we find no evidence that the AJCA cash windfall increased workplace discrimination, even for constrained firms with high leverage. Given that Cohn and Wardlaw (2016) show that the AJCA led to a decrease in workplace injury rates, our insignificant results suggest that firms either view workplace discrimination to be less costly than workplace injuries or lack sufficient power to align incentives so employees do not discriminate.

5. Characteristics of discriminating firms

In the previous section, we find no connection between financial slack and discrimination lawsuit frequency. Therefore, as an alternative to the Becker (1957) model, we hypothesize that discriminatory behavior is a sticky, deeply embedded feature of firm culture. To examine this conjecture, we run regressions of discrimination lawsuit frequency on firm characteristics. Table 6 presents the results.

In Column (1) of Panel A, we regress the log of one plus the number of discrimination suits on market capitalization, number of employees, stock return, ROA, leverage, cash-to-assets, and industry price-cost margin, as of the previous fiscal year end. These pooled regressions include year fixed effects to control for aggregate trends but exploit both within and across firm variation. Column (2) includes governance characteristics measuring the percentage of the board that is female, the percentage of the board that is independent, the presence of a 10% blockholder, and the managerial entrenchment index of Bebchuk, Cohen, and Ferrell (2009). Columns (3) and (4) repeat the same specifications as Columns (1) and (2) but replace the dependent variable with discrimination suits per employee.

Across all specifications, we estimate positive coefficients on our proxies of firm size, the

log market capitalization and log number of employees, which are statistically significant at the 1% level. These estimates suggest that larger and more complex firms face more discrimination suits, even on a per employee basis. Stock returns, ROA, and cash-to-assets are all generally negatively related to future discrimination. These estimates contrast with taste-based models of discrimination, which predict that lawsuits should be positively correlated with profitability. Moreover, we find no evidence suggesting that discrimination suits are related to board or shareholder monitoring. This non-result is notable given the large corporate governance literature that argues these monitors reduce the scope for corporate activities that reduce shareholder value.

We repeat these regressions in Panel B, except that we replace our governance variables with proxies of firm culture. Specifically, we include the number of CSR concerns and demographics variables from the firm's headquarter location, including the unemployment rate, income per capita, log population, and percentage of the population that is black. In Column (1), we estimate the coefficient on CSR concerns to be positive, suggesting that poor corporate social responsibility is correlated with discriminatory behavior, but this relation disappears when focusing on litigation rates per employee. We also find that discrimination rates are higher for firms headquartered in rural areas and for firms headquartered in areas with a high percentage of black residents. Notably, a one standard deviation increase in the percentage of black residents is associated with a 13.5% higher number of discrimination cases relative to the sample mean. Together, these results paint a picture of the "quintessential" discriminating firm: a struggling, large corporation located in a rural area with a high proportion of black residents.

In Table 7, we present results from an analysis of covariance (ANCOVA) that decomposes variation in discrimination lawsuit frequency according to the relative contribution of different factors. To do so, we run OLS regressions of log one plus the number of

discrimination lawsuits on lagged firm characteristics and year, industry, headquarter state, firm, and CEO fixed effects. We compute the Type III partial sum of squares for each effect and then normalize each estimate by the sum across the effects, forcing the sum to one. Thus, we calculate the proportion of variation explained by each regressor. In Column (1), for example, we include firm level controls and year fixed effects as regressors. The adjusted R-squared for this regression is 0.332, indicating that these variables together explain 33.2% of the variation in log discrimination suits. The log number of employees and year fixed effects have the most explanatory power in this specification, explaining 69.2% and 9.2% of the variation respectively.

Column (2) adds Fama-French 48 industry fixed effects to this specification. This addition increases the regression R-squared by roughly 4%, but reduces the variation explained by the number of employees by half, leaving 33.5% of the explained variation due to industry fixed effects. Column (3) adds state fixed effects, which further increases the R-squared by a little over 1% and accounts for 11.8% of the explained variation. The addition of firm fixed effects in Column (4) increases the R-squared to 70.7%, almost double the previous model's R-squared, and contributes to 98% of the explained variation in discrimination. Finally, Column (5) includes CEO fixed effects. Given that CEOs change infrequently, CEO and firm fixed effects capture similar variation in slow moving firm characteristics. The R-squared for this model is 74.8%, with 19.1% of this variation explained by CEO fixed effects and 79.7% of the variation explained by firm fixed effects. Based on these results, we conclude that most of the variation in discrimination lawsuit frequency is explained by cross-sectional differences rather than time-series variation. Therefore, workplace discrimination appears to be a slow-moving product of firm culture rather than a time-varying construct that can be reduced depending on economic conditions.

6. Firm response to discrimination lawsuits

As a final test of Milton Friedman’s conjecture that market forces will eventually reduce discrimination, we examine the relation between past discrimination lawsuits and future firm outcomes. Although our prior analyses provide no evidence that discrimination is tempered by economic conditions, CEO labor markets may provide an additional source of discipline. We collect CEO turnover data from Gentry, Harrison, Quigley, and Boivie (2021), who provide a public database containing the cause of S&P 1500 CEO departures. In Table 8, we report regressions of a CEO dismissal indicator on lagged discrimination lawsuit frequency and control variables. Across all specifications, we find no evidence that discrimination lawsuits are followed by CEO turnover. Instead, the insignificant relation between discrimination litigation filings and CEO dismissals suggests that CEO labor markets may not be a sufficiently strong mechanism to limit workplace discrimination.

Finally, we examine the persistence of discrimination. In Table 9, we run Arellano-Bond regressions of log discrimination on five lags of log discrimination and firm and year fixed effects. We use the full sample for regressions in Columns (1) and (2) and restrict the sample to firm-year observations with a non-zero number of discrimination lawsuits in Columns (3) and (4) (i.e., firms that have discrimination lawsuits in all years). Rather than past discrimination reducing future discrimination, as would be expected if competition or the CEO labor market sufficiently disciplines discriminating firms, Table 9 shows that past discrimination rates are positively correlated with future discrimination. Collectively, these results provide further evidence that workplace discrimination is a persistent feature ingrained in firm culture.

7. Conclusion

In the aftermath of the #MeToo and #BlackLivesMatter movements, the Business

Roundtable stated: “America’s largest employers know that the economy works best when employees can be who they are, without fear of bias, discrimination and inequality.”⁵ This statement echoes key features of the canonical economic model of discrimination formalized by Gary Becker more than 60 years earlier. Becker considered discrimination to be a “taste”, with no economic merit, that is driven by animus against a certain group of people. An employer will discriminate if the non-pecuniary benefit of indulging in his animus outweighs the financial costs, but doing so will place his firm at a competitive disadvantage relative to less discriminatory peers. Thus, taste-based models of discrimination predict that a reduction in financial slack should limit the scope for employers to engage in discriminatory practices rather than maximize firm value.

We examine this economic framework using novel data on employment discrimination lawsuits filed in federal court between 1992 and 2018. Consistent with the Business Roundtable’s statement, discrimination appears to destroy shareholder value: the mean (median) three-day CAR around a suit filing is -0.03% (-0.09%), which corresponds to a roughly \$30 million loss for the average firm. However, we find no evidence suggesting that firms internalize this cost and optimize discriminatory behavior depending on economic conditions, as predicted by taste-based models.

Studying three quasi-natural experiments that exogenously alter firms’ financial slack, we find no significant relation between discrimination lawsuit frequency and product market competition or financial constraints. Instead, the strongest predictor of discrimination lawsuit frequency is a firm’s headquarter location. Corporations headquartered in the Southeastern U.S. are more than twice as likely to face a discrimination lawsuit as firms headquartered in the

⁵ The Business Roundtable is a trade association of 200+ CEOs leading U.S. companies that combine to employ over 20 million people. <https://www.businessroundtable.org/business-roundtable-statement-on-workplace-discrimination>

Northwest. Our analyses imply that the typical discriminating firm is a large corporation with a high number of CSR concerns, headquartered in a rural area with a high fraction of black residents.

Together, our findings suggest that discrimination is a highly persistent construct ingrained in employee culture and is unlikely to be mitigated by financial market discipline. These results echo Kenneth Arrow's (1973) famous criticism of Becker's model of discrimination. If competition could eventually drive discriminating firms out of the market, Arrow argued, Becker's model "predicts the absence of the phenomenon it was designed to explain."

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Figure 1: FJC-Compustat sample comparison. This figure presents the time-series of employment discrimination lawsuits filed in Federal Court (Nature of Suit 442-Civil Rights Job). The blue line displays the number of lawsuits recorded in the Federal Judicial Center Civil Integrated Database and the red line displays the number of lawsuits filed against U.S. public companies in our FJC-Compustat combined sample. The FJC-Compustat sample consists of 105,974 firm-year observations from 11,515 U.S. public companies with data available in the CRSP-Compustat Merged Database between 1992 and 2018.

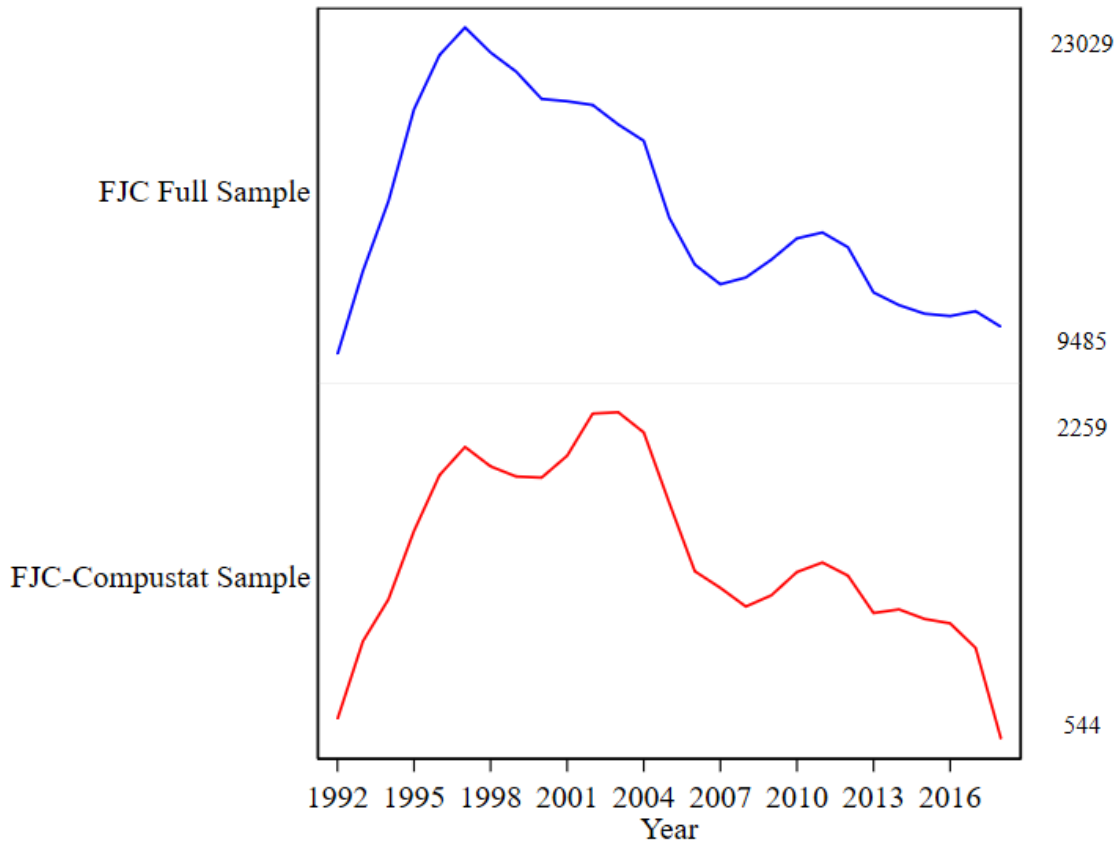


Figure 2: Industry distribution of corporate employment discrimination lawsuits. This figure displays the fraction of U.S public companies facing an employment discrimination lawsuit in Federal Court, split across Fama-French 12 industry classifications. The FJC-Compustat sample consists of 105,974 firm-year observations from 11,515 U.S. public companies with data available in the CRSP-Compustat Merged Database between 1992 and 2018.

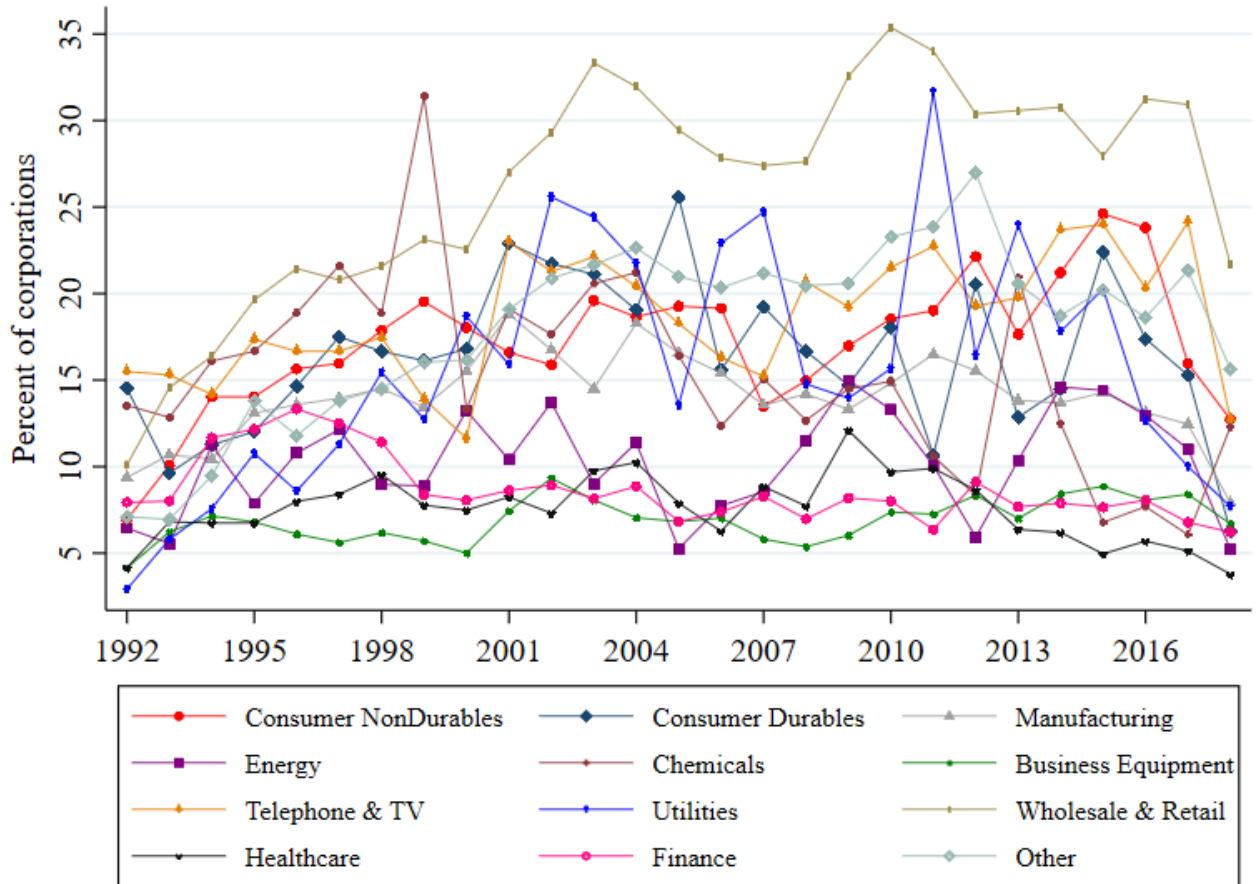


Figure 3: Distribution of corporate employment discrimination lawsuits across firm type. This figure displays binned scatter plots of employment discrimination litigation rates against firm size and credit ratings. Firm size represents total assets at fiscal year-end before lawsuit filing. The FJC-Compustat sample consists of 105,974 firm-year observations from 11,515 U.S. public companies with data available in the CRSP-Compustat Merged Database between 1992 and 2018.

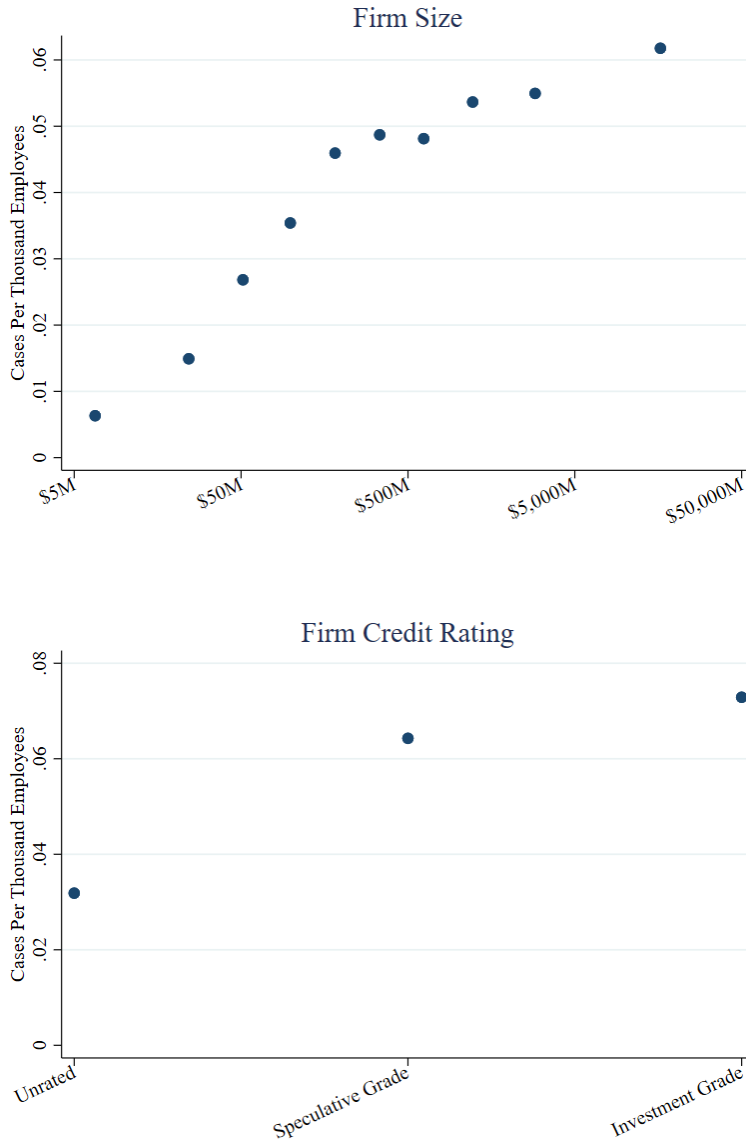


Figure 4: Geographic distribution of corporate employment discrimination lawsuits. This figure displays the fraction of firm-year observations facing an employment discrimination lawsuit in Federal Court, split across geography. The FJC-Compustat sample consists of 105,974 firm-year observations from 11,515 U.S. public companies with data available in the CRSP-Compustat Merged Database between 1992 and 2018.

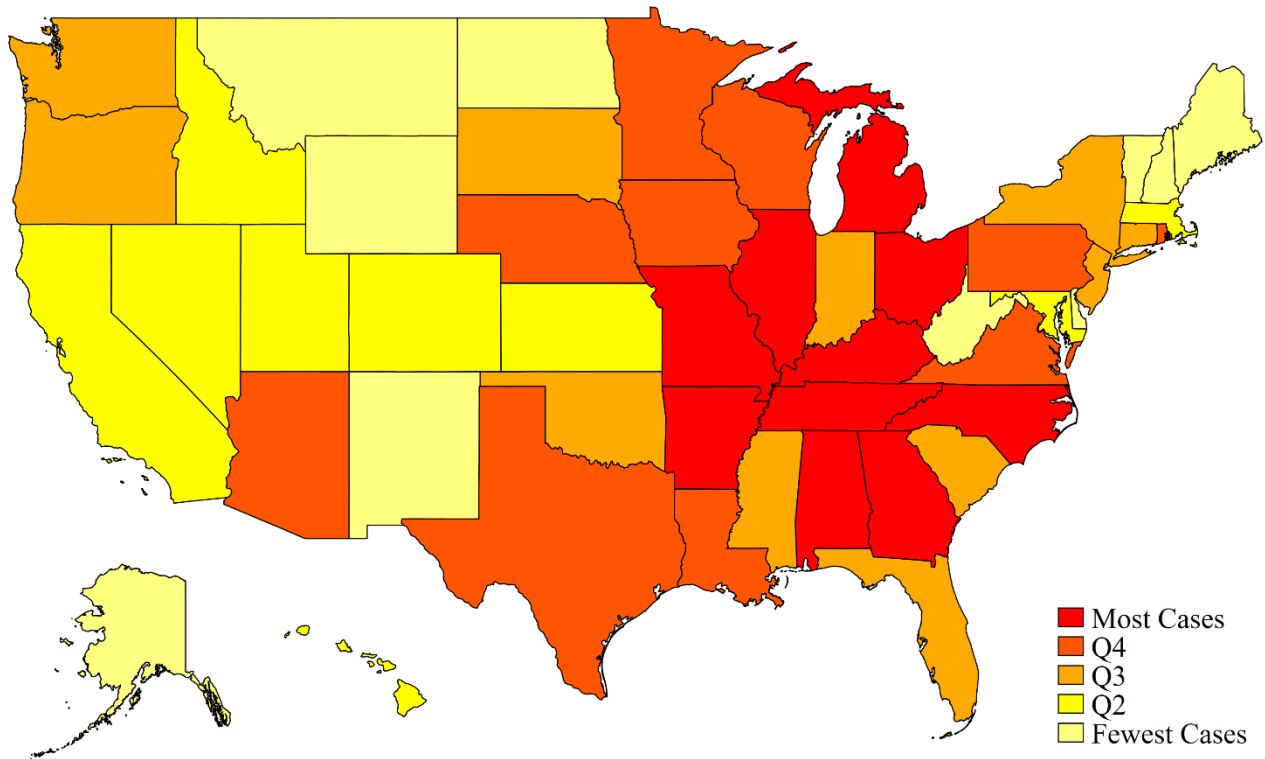


Table 1: Frequency of employment discrimination lawsuits. The FJC-Compustat sample consists of 105,974 firm-year observations from 11,515 U.S. public companies with data available in the CRSP-Compustat Merged Database between 1992 and 2018. Panel A displays the percentage of these firms that face an employment discrimination lawsuit in Federal Court (Nature of Suit 442-Civil Rights Job) at some point during the sample period. Panel B lists the ten firms that faced the most employment discrimination lawsuits over the sample period.

Panel A: Percentage of firms that face discrimination suits

	Percentage
Fraction of firms ever facing discrimination suit	27.8
Fraction of firm-year observations with discrimination suit	12.6
<i>By industry</i>	
Consumer NonDurables	31.8
Consumer Durables	32.1
Manufacturing	32.8
Oil, Gas, and Coal Extraction and Products	26.1
Chemicals and Allied Products	33.9
Business Equipment: Computers, Software, Electronics	19.1
Telephone and Television Transmission	31.1
Utilities	41.5
Wholesale, Retail, and Some Services	40.2
Healthcare, Medical Equipment, and Drugs	16.8
Finance	19.5
Other	30.3
<i>By size</i>	
Less than \$100M	8.8
\$100M to \$250M	21.3
\$250M to \$500M	31.2
\$500M to \$1,000M	39.0
\$1,000M to \$2,500M	47.8
\$2,500M to \$5,000M	54.8
Greater than \$5,000M	73.4
<i>Firm does not have credit rating</i>	17.8
<i>Firm has credit rating</i>	61.4

Panel B: Firms that face the most discrimination suits

Rank	Company Name	Industry	Lawsuits
1	AT&T Inc	Telephone	179
2	Bank of America Corp	Finance	175
3	IBM	Business Equipment	174
4	Tyson Foods	Consumer NonDurables	172
5	Home Depot	Retail	171

Table 2: Value implications of corporate discrimination lawsuits. This table reports percentage cumulative abnormal returns (CARs) around corporate discrimination lawsuit filings. The sample consists of 55,969 employment discrimination lawsuits against 3,614 U.S. public companies with data available in the CRSP-Compustat Merged Database between 1992 and 2018. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variables are winsorized at the 1/99 percent tails. Appendix 1 lists variable definitions.

	3-day CARs		5-day CARs	
	Market Adjusted	Fama-French 3 Factor	Market Adjusted	Fama-French 3 Factor
Mean CAR (%)	-0.033**	-0.039***	-0.040**	-0.056***
Median CAR (%)	-0.092***	-0.093***	-0.096***	-0.095***
Observations	55,969	55,969	55,969	55,969

Table 3: China trade shock and corporate discrimination. This table reports the effect of a positive shock to product market competition on the frequency of corporate discrimination lawsuits. Panel A presents descriptive statistics for a sample of 27,729 firm-year observations from 3,595 U.S. public manufacturing firms with data available in the CRSP-Compustat Merged Database between 1992 and 2007. Panel B presents ordinary least squares (OLS) estimates of the change in corporate discrimination lawsuit frequency around China's receipt of permanent Most-Favored Nation (MFN) trade status depending on the firm's exposure. China Trade Shock Exposure measures the potential tariff hike the firm's industry faced before China obtained permanent MFN trade status in 2000, and has been standardized to have unit variance. Post is an indicator that equals one from 2000 onwards. Heteroskedasticity-consistent standard errors clustered by industry are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Appendix 1 lists variable definitions.

Panel A: China trade shock sample description

	Mean	S.D.	P25	Median	P75	Obs
Discrimination Suits	0.222	0.874	0.000	0.000	0.000	27,729
Ln(1 + Discrimination Suits)	0.110	0.369	0.000	0.000	0.000	27,729
Discrimination Suits / Employees	0.037	0.163	0.000	0.000	0.000	27,729
China Trade Shock Exposure	0.317	0.122	0.295	0.329	0.363	27,729
High Tech Industry (0/1)	0.322	0.467	0.000	0.000	1.000	27,729
Unskilled Labor Percentage	0.597	0.147	0.500	0.583	0.721	27,729

Panel B: Effect of the China trade shock

	Ln(1 + Discrimination Suits)		Discrimination Suits / Employees	
	(1)	(2)	(3)	(4)
Post * China Trade Shock Exposure	-0.002 (0.007)	-0.002 (0.007)	0.001 (0.003)	0.001 (0.003)
Post * High Tech Industry		0.005 (0.016)		-0.003 (0.007)
Post * Unskilled Labor %		0.008 (0.009)		-0.001 (0.004)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	27,729	27,729	27,729	27,729
Adjusted R-squared	0.632	0.632	0.134	0.134

Table 4: Banking deregulation and corporate discrimination. This table reports the effect of a positive shock to product market competition on the frequency of corporate discrimination lawsuits. Panel A presents descriptive statistics for a sample of 4,351 firm-year observations from 801 U.S. depository institutions with data available in the CRSP-Compustat Merged Database between 1994 and 2005. Panel B presents OLS estimates of the effect of the Interstate Banking and Branching Efficiency Act (IBBEA) on the frequency of corporate discrimination lawsuits. Deregulation Index captures state level differences in interstate branching restrictiveness and ranges from 0 for fully regulated states to 4 for fully deregulated states. Unit Banking is an indicator that equals one if the state began with a complete prohibition on branching and zero if the state only limited branching to the city of the head office. Heteroskedasticity-consistent standard errors clustered by industry are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Appendix 1 lists variable definitions.

Panel A: Banking deregulation sample description

	Mean	S.D.	P25	Median	P75	Obs
Discrimination Suits	0.168	0.780	0.000	0.000	0.000	4,351
Ln(1 + Discrimination Suits)	0.082	0.329	0.000	0.000	0.000	4,351
Discrimination Suits / Employees	0.033	0.157	0.000	0.000	0.000	4,351
Deregulation Index	2.197	1.446	1.000	2.000	4.000	4,351
Unit Banking (0/1)	0.211	0.408	0.000	0.000	0.000	4,351

Panel B: Effect of banking deregulation

	Ln(1 + Discrimination Suits)		Discrimination Suits / Employees	
	(1)	(2)	(3)	(4)
Deregulation Index	-0.003 (0.011)	-0.000 (0.018)	0.004 (0.005)	0.003 (0.008)
Deregulation Index * Unit Banking		-0.006 (0.026)		0.001 (0.009)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	4,351	4,351	4,351	4,351
Adjusted R-squared	0.652	0.652	0.181	0.181

Table 5: Cash flow shock and corporate discrimination. This table reports the effect of a positive cash flow shock on the frequency of corporate discrimination lawsuits. Panel A presents descriptive statistics for a sample of 4,649 firm-year observations from 1,279 U.S. public firms with data available in the CRSP-Compustat Merged Database from 2002 to 2003 and 2005 to 2006. Panel B presents OLS estimates of the change in corporate discrimination lawsuit frequency around the American Jobs Creation Act (AJCA) depending on the firm's exposure. AJCA Exposure is an indicator that equals one if the firm's cumulative reported foreign profits from 2001 to 2003 are positive and zero otherwise. Post is an indicator that equals one from 2004 onwards. Heteroskedasticity-consistent standard errors clustered by firm are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Appendix 1 lists variable definitions.

Panel A: American Jobs Creation Act sample description

	Mean	S.D.	P25	Median	P75	Obs
Discrimination Suits	0.488	1.292	0.000	0.000	0.000	4,649
Ln(1 + Discrimination Suits)	0.232	0.527	0.000	0.000	0.000	4,649
Discrimination Suits / Employees	0.057	0.178	0.000	0.000	0.000	4,649
AJCA Exposure (0/1)	0.523	0.500	0.000	1.000	1.000	4,649
Leverage	0.193	0.176	0.017	0.171	0.310	4,649

Panel B: Effect of the American Jobs Creation Act

	Ln(1 + Discrimination Suits)		Discrimination Suits / Employees	
	(1)	(2)	(3)	(4)
Post * AJCA Exposure	-0.012 (0.021)	-0.019 (0.026)	-0.013 (0.011)	-0.016 (0.016)
Post * AJCA Exposure * Leverage		0.011 (0.125)		0.011 (0.068)
Post * Leverage		-0.026 (0.095)		-0.001 (0.056)
AJCA Exposure * Leverage		-0.241* (0.137)		-0.078 (0.084)
Leverage		0.207** (0.104)		0.076 (0.071)
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Observations	4,649	4,649	4,649	4,649
Adjusted R-squared	0.705	0.705	0.208	0.208

Table 6: Which firms face discrimination lawsuits? This table presents estimates from OLS regressions that relate employment discrimination lawsuit frequency to lagged firm, industry, and headquarter-area characteristics that have been standardized to have unit variance. The FJC-Compustat sample consists of 105,974 firm-year observations from 11,515 U.S. public companies with data available in the CRSP-Compustat Merged Database between 1992 and 2018. Heteroskedasticity-consistent standard errors clustered by firm are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variables are winsorized at the 1/99 percent tails. Appendix 1 lists variable definitions.

Panel A: Discrimination lawsuits and firm characteristics

	Ln(1 + Discrimination Suits)		Discrimination Suits / Employees	
	(1)	(2)	(3)	(4)
Ln(Market Capitalization)	0.067*** (0.007)	0.120*** (0.020)	0.007*** (0.001)	0.007** (0.003)
Ln(Number of Employees)	0.182*** (0.008)	0.407*** (0.022)	0.014*** (0.001)	-0.006** (0.003)
Prior Stock Return	-0.009*** (0.001)	-0.020*** (0.005)	-0.003*** (0.001)	-0.003 (0.002)
ROA	-0.041*** (0.003)	-0.022 (0.020)	0.004*** (0.001)	0.010** (0.005)
Leverage	0.002 (0.003)	0.010 (0.009)	0.004*** (0.001)	0.009*** (0.002)
Cash / Assets	-0.007** (0.003)	-0.003 (0.011)	-0.003*** (0.001)	-0.005 (0.003)
Industry Price-Cost Margin	-0.012*** (0.002)	-0.018** (0.007)	-0.004*** (0.001)	-0.003 (0.002)
Female Board %		0.004 (0.007)		-0.000 (0.002)
Independent Board %		0.013 (0.009)		-0.000 (0.002)
Blockholder		0.015 (0.013)		0.005* (0.003)
E-Index		-0.013* (0.007)		0.001 (0.001)
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	96,849	25,851	96,849	25,851
Adjusted R-squared	0.244	0.311	0.023	0.020

Panel B: Discrimination lawsuits and employee culture

	Ln(1 + Discrimination Suits)		Discrimination Suits / Employees	
	(1)	(2)	(3)	(4)
Ln(Market Capitalization)	0.111*** (0.018)	0.111*** (0.017)	0.006** (0.003)	0.008*** (0.003)
Ln(Number of Employees)	0.320*** (0.017)	0.316*** (0.017)	0.006** (0.002)	0.005** (0.002)
Prior Stock Return	-0.005* (0.003)	-0.005 (0.003)	-0.003** (0.001)	-0.003** (0.001)
ROA	-0.062*** (0.010)	-0.058*** (0.010)	0.008*** (0.003)	0.009*** (0.003)
Leverage	-0.000 (0.008)	-0.000 (0.008)	0.007*** (0.002)	0.007*** (0.002)
Cash / Assets	0.015** (0.007)	0.019** (0.008)	-0.003 (0.002)	-0.001 (0.002)
Industry Price-Cost Margin	-0.023*** (0.005)	-0.022*** (0.005)	-0.005*** (0.002)	-0.005*** (0.002)
Number of CSR Concerns	0.064*** (0.010)	0.065*** (0.010)	0.000 (0.001)	0.000 (0.001)
Unemployment		0.008 (0.014)		0.001 (0.002)
Log(Income per capita)		0.024 (0.018)		-0.004 (0.003)
Log(Population)		-0.027*** (0.008)		-0.005*** (0.002)
Black Population %		0.019** (0.009)		0.005*** (0.002)
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	26,554	26,221	26,554	26,221
Adjusted R-squared	0.331	0.332	0.015	0.018

Table 7: Variance decomposition. This table presents a variance decomposition for different model specifications, with the adjusted R-squared reported at the bottom. We compute the Type III partial sum of squares for each effect in the model and then normalize each estimate by the sum across the effects, forcing the sum to one. The FJC-Compustat sample consists of 105,974 firm-year observations from 11,515 U.S. public companies with data available in the CRSP-Compustat Merged Database between 1992 and 2018. Firm FE are firm fixed effects. Year FE are calendar year fixed effects. HQ State FE are headquarter-state fixed effects, and Industry FE are Fama-French 48 industry fixed effects. Variable definitions are provided in the appendix. Variables are winsorized at the 1/99 percent tails. Appendix 1 lists variable definitions.

	Ln(1 + Discrimination Suits)				
	(1)	(2)	(3)	(4)	(5)
Log(Market Capitalization)	0.069	0.094	0.095	0.000	0.001
Log(Number of Employees)	0.692	0.303	0.270	0.005	0.004
Prior Stock Return	0.000	0.002	0.002	0.000	0.000
ROA	0.024	0.024	0.025	0.000	0.000
Leverage	0.000	0.000	0.000	0.000	0.000
Cash/Assets	0.004	0.001	0.002	0.000	0.000
Industry Price-Cost Margin	0.009	0.000	0.000	0.000	0.000
Number of CSR Concerns	0.086	0.111	0.105	0.000	0.000
Unemployment	0.001	0.001	0.002	0.000	0.000
Log(Income per capita)	0.003	0.003	0.002	0.000	0.000
Log(Population)	0.014	0.012	0.001	0.000	0.000
Black Population %	0.008	0.005	0.001	0.000	0.000
Year FE	0.092	0.109	0.093	0.006	0.007
Industry FE		0.335	0.285		
State FE			0.118	0.006	
Firm FE				0.988	0.797
CEO FE					0.191
Adjusted R-square	0.332	0.369	0.382	0.707	0.748

Table 8: Corporate discrimination and CEO turnover. This table presents estimates from OLS regressions that relate CEO dismissals to employment discrimination lawsuit frequency and lagged firm characteristics. $\ln(1 + \text{Discrimination Suits}_{t-1,t-3})$ measures the rolling number of employment discrimination lawsuits filed against the firm over the prior three years. $\text{Discrimination Suits}/\text{Employees}_{t-1,t-3}$ measures the firm's average employment discrimination litigation rate over the prior three years. The sample consists of 35,960 firm-years from 3,092 U.S. public companies with data available in Execucomp and the CRSP-Compustat Merged Database between 1992 and 2018. Heteroskedasticity-consistent standard errors clustered by firm are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Variables are winsorized at the 1/99 percent tails. Appendix 1 lists variable definitions.

	CEO Dismissal			
	(1)	(2)	(3)	(4)
$\ln(1 + \text{Discrimination Suits}_{t-1,t-3})$	0.000 (0.000)	0.001 (0.000)		
$\text{Discrimination Suits}/\text{Employees}_{t-1,t-3}$			0.008 (0.007)	0.001 (0.009)
$\ln(\text{Market Capitalization})$	-0.003*** (0.001)	0.001 (0.002)	-0.003*** (0.001)	0.001 (0.002)
$\ln(\text{Number of Employees})$	0.005*** (0.001)	0.009*** (0.003)	0.005*** (0.001)	0.009*** (0.003)
Prior Stock Return	-0.013*** (0.002)	-0.012*** (0.002)	-0.013*** (0.002)	-0.012*** (0.002)
ROA	-0.049*** (0.009)	-0.054*** (0.015)	-0.050*** (0.009)	-0.054*** (0.015)
Leverage	0.010* (0.006)	0.012 (0.010)	0.010* (0.006)	0.011 (0.010)
Cash / Assets	0.019*** (0.006)	-0.029** (0.012)	0.019*** (0.006)	-0.029** (0.012)
Firm Fixed Effects	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	35,960	35,960	35,960	35,960
Adjusted R-squared	0.006	0.028	0.006	0.028

Table 9. Discrimination persistence This table presents Arellano-Bond regressions that relate employment discrimination lawsuit frequency to lagged firm and industry characteristics. *t*-statistics are reported in parentheses and are calculated using Arellano-Bond robust standard errors. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	Ln(1 + Discrimination Suits)			
	(1)	(2)	(3)	(4)
L.Ln(1 + Discrimination Suits)	0.075*** (6.51)	0.265*** (12.63)	0.371*** (14.98)	1.280*** (12.98)
L2.Ln(1 + Discrimination Suits)		0.184*** (11.95)		0.787*** (11.57)
L3.Ln(1 + Discrimination Suits)		0.113*** (9.33)		0.443*** (9.30)
L4.Ln(1 + Discrimination Suits)		0.075*** (7.05)		0.276*** (7.32)
L5.Ln(1 + Discrimination Suits)		0.055*** (5.65)		0.123*** (4.35)
Observations	79,419	45,831	11,574	7,756
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Appendix 1: Variable definitions. This table lists variable definitions and data sources. FJC denotes the Federal Judicial Center Civil Integrated Database. CCM denotes the CRSP-Compustat Merged Database. NBER refers to the NBER-CES Manufacturing Industry Database. Romalis denotes John Romalis' website. RM-D and RM-G refer to the RiskMetrics Directors and Governance databases, respectively. TFN denotes the Thomson Reuters s34 Master File.

<i>Variable</i>	<i>Source</i>	<i>Description</i>
Discrimination Suits	FJC	Number of employment discrimination lawsuits in filed in Federal Court (Nature of Suit 442-Civil Rights Job)
Discrimination Suits / Employees	FJC, CCM	Number of employment discrimination lawsuits in filed in Federal Court per thousand of employees
Market Capitalization	CCM	Common shares outstanding times fiscal year closing (in millions)
Number of Employees	CCM	Number of employees (in thousands)
Prior Stock Return	CCM	Buy-and-hold abnormal return (BHAR) over the fiscal year using the CRSP value-weighted index as market proxy
Return on Assets	CCM	Operating income before depreciation, divided by total assets
Leverage	CCM	Long-term debt plus debt in current liabilities, divided by total assets
Cash / assets	CCM	Cash and equivalents divided by total assets
Market value of assets	CCM	Market value of equity minus book value of equity plus total assets
Market value of equity	CCM	Common shares outstanding times the fiscal quarter closing price
Market-to-book	CCM	Ratio of market value to book value of total assets
Unrated	CCM	Indicator that equals one if firm has an S&P long term issuer credit rating, and zero otherwise
Industry Price-Cost Margin	CCM	Sales minus cost of goods sold plus change in inventories, divided by sales plus change in inventories
Female Board %	RM-D	Fraction of the board that is female according to RiskMetrics
Independent Board %	RM-D	Fraction of the board that is independent according to RiskMetrics
Blockholder	TFN	Indicator that equals one if an institutional investor owns more than 10% of shares outstanding, and zero otherwise
E-Index	RM-G	Entrenchment index, developed by Bebchuk, Cohen, and Ferrell (2009), that counts the presence of six shareholder rights provisions: staggered boards, poison pills, golden parachutes, supermajority requirements for charter amendments, supermajority requirements for bylaw amendments, and supermajority requirements for mergers
Number of CSR Concerns	KLD	The number of community, diversity, employee relations, environment, and human rights concerns listed in the KLD database
Unemployment	Census	County-level unemployment rate
Log(Income per capita)	Census	County-level income per capita
Log(Population)	Census	County-level population
Black Population %	Census	County-level percentage of population that is Black

Appendix 1: Variable definitions (Continued).

<i>Variable</i>	<i>Source</i>	<i>Description</i>
China Trade Shock	Romalis	Potential tariff hike the industry faced before China obtained permanent Most-Favored Nation (MFN) status in 2000. It is the average gap between MFN rates and Non-Market Economy (NME) rates on HTS-8 products that map to the industry. We standardize this variable to have unit variance in all regression analyses.
High Tech Industry		Indicator equal to one if industry is classified in Computers and Electronics Manufacturing Subsector (NAICS 334), 0 otherwise.
Unskilled Labor Percentage	NBER	The fraction of industry employees in 1999 that are production workers. We standardize this variable to have unit variance in all regression analyses.
Cumulative abnormal return	CCM	Market model CAR estimated using CRSP value-weighted index returns and a one-year estimation window (252 trading days) ending one month (20 trading days) before the [-1, +1] event window

Appendix 2: FJC-Compustat sample description. This table presents descriptive statistics for the FJC-Compustat sample, which consists of 105,974 firm-year observations from 11,515 U.S. public companies with data available in the CRSP-Compustat Merged Database between 1992 and 2018. Variables are winsorized at the 1/99 percent tails. Appendix 1 lists variable definitions.

	Mean	S.D.	P25	Median	P75	Obs
Discrimination Suits	0.293	1.036	0.000	0.000	0.000	105,974
Ln(1 + Discrimination Suits)	0.141	0.430	0.000	0.000	0.000	105,974
Discrimination Suits / Employees	0.040	0.161	0.000	0.000	0.000	105,974
Market Capitalization (\$Millions)	2,060.514	6,406.487	52.637	223.045	1,041.395	99,953
Number of Employees (Thousands)	5.753	15.478	0.175	0.751	3.665	102,109
Prior Stock Return	0.019	0.610	-0.336	-0.067	0.216	98,217
ROA	0.042	0.227	0.017	0.089	0.154	105,146
Leverage	0.201	0.198	0.021	0.152	0.324	105,327
Cash / Assets	0.197	0.235	0.028	0.093	0.285	105,485
Industry Price-Cost Margin	-2.434	11.586	0.124	0.282	0.381	105,974
Female Board %	0.110	0.104	0.000	0.111	0.167	27,457
Independent Board %	0.717	0.170	0.625	0.750	0.857	27,455
Blockholder	0.296	0.456	0.000	0.000	1.000	103,370
E-Index	2.892	1.508	2.000	3.000	4.000	34,023
CSR Concerns	1.145	1.241	0.000	1.000	2.000	27,477
Unemployment	0.298	0.247	0.251	0.349	0.426	104,626
Income Per Capita	44,355.742	21,941.135	29,830.000	39,378.000	52,129.000	104,626
Population (Millions)	1.525	1.915	0.500	0.910	1.631	104,889
% Black Population	0.139	0.119	0.049	0.105	0.200	105,974
CEO Dismissal	0.027	0.162	0.000	0.000	0.000	35,960