A culture of discrimination:
Evidence from civil rights litigation against U.S. corporations

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Abstract

Between 1992 and 2018, U.S. public corporations faced over 36,000 workplace discrimination lawsuits in federal court. Litigation frequency differs widely across firms and regions, correlates with local attitudes toward race and gender, and varies with shocks to cultural tolerance of misconduct. Studying two natural experiments, we find no evidence that economic forces provide a source of discipline. Consequences are small: shareholder value drops by roughly $1 million around the typical lawsuit, with declining effects for persistent offenders. We conclude that the incidence of workplace discrimination is largely determined by employees' cultural norms, rather than a firm's economic environment.

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1. Introduction

In his 1962 book, *Capitalism and Freedom*, Milton Friedman claims that a competitive market "protects men from being discriminated against in their economic activities for reasons that are irrelevant to their productivity." He argues that business decisions motivated by prejudice entail a competitive disadvantage that lowers profit and eventually drives the discriminating firm out of the market. 60 years later, top management consulting firms echo Friedman's message and advise that the "business case for inclusion and diversity is stronger than ever." (McKinsey & Company 2020). Nonetheless, we find that allegations of workplace discrimination are prevalent, with over 36,000 federal civil rights lawsuits filed against nearly 3,000 U.S. public firms between 1992 and 2018. In this paper, we study novel litigation data from the Federal Judicial Center to document the incidence of workplace discrimination, uncover its determinants, and assess whether economic forces provide a source of discipline.

Civil rights litigation presents a unique setting to study workplace discrimination for at least two reasons. First, lawsuit case files provide insight into the nature of alleged discrimination. Federal civil rights law grants members of a protected class the right to file a discrimination complaint if they were subject to an adverse employment action, "severe or pervasive" harassment, or retaliation.[1] Using Latent Dirichlet Allocation (LDA) topic modeling to categorize discrimination cases, we show that complaints almost always include allegations of retaliation or harassment even when a hostile work environment is not the focal topic. In our sample, 88% of complaint documents mention the word "retaliation" or include at least ten harassment-related words. This evidence suggests that alleged discrimination is often motivated by animus.

[1] Federal civil rights law prohibits discrimination based on the following protected characteristics: race, color, national origin, religion, sex, age, disability, and genetic information. Adverse employment actions include demotion/firing, unfair compensation/benefits, failure to hire/promote, and failure to provide adequate training/accommodations.

Second, lawsuit filings provide a discrete measure of disparate treatment as perceived by employees. We find that 29% of U.S public corporations face at least one employment discrimination lawsuit in federal court between 1992 and 2018. The average (median) firm faces 0.31 (0) cases per year. These dynamics yield a highly skewed distribution, with the top 100 firms defending 49% of all discrimination cases despite contributing only 24% of total employment and 19% of total assets.[2] We also find significant variation across industry and geography; firms in the manufacturing industry or headquartered in the Southeast are roughly twice as likely to face a discrimination suit as firms in the healthcare industry or headquartered in the Northwest.

We conjecture that these patterns reflect differences in corporate culture. Grennan and Li (2023) define corporate culture as "an informal institution typified by patterns of behavior and reinforced by people, systems, and events." Consistent with the view that corporate culture emerges from social interactions, we find a robust association between discrimination lawsuits and local attitudes toward race and gender. Estimates from Poisson regressions imply that the number of discrimination suits is 15% higher for firms headquartered in a state with one standard deviation above average racial bias, relative to the sample mean, and 29% lower for firms headquartered in one of the "least sexist" states.[3] Variance decompositions further reveal that 80% of explainable variation is driven by firm fixed effects and 18% by CEO fixed effects. These results suggest that discriminatory behavior is ingrained in employee culture but can be influenced by firm leadership.

An important limitation of our setting is that we can only observe allegations of workplace discrimination that advance to federal court. This progression involves a series of non-random

[2] Kline, Rose, and Walters (2022) find similar results in a correspondence experiment that sent fictitious applications with randomized characteristics to 108 large U.S. employers. The authors show that discrimination against distinctively Black names is concentrated among a select set of employers, with two-digit SIC industry classification explaining roughly half of the cross-firm variation in job application contact gaps.

[3] Following Levine, Levkov, and Rubinstein (2008), we measure racial bias as the difference between predicted and actual interracial marriage rates during 1970 and, following Giannetti and Wang (2023), we define a state as "least sexist" if it is in the lowest two sexism ranking categories based on the General Social Survey.

decisions by both the plaintiff and defendant. Consequently, our findings may be influenced by measurement error like other research that uses observable proxies to study the prevalence of underlying behavior (e.g., corporate fraud, innovation, etc.). Throughout the paper, we discuss how the data generating process could affect inferences and take steps to mitigate the scope for bias. In addition, we directly confront this issue by studying within-firm changes in litigation frequency around shocks to corporate culture and economic conditions.

We begin by analyzing events that are uncorrelated with firm characteristics but potentially shift social norms: allegations of misconduct against a local politician. We hypothesize that allegations against a U.S. congressperson may influence employee behavior by revealing society's tolerance for misconduct. If a local politician resigns after an allegation, employees may become less likely to engage in workplace discrimination due to higher perceived probability of punishment (Sah 1991) or greater stigma associated with misconduct (Bernheim 1994; Easley and O'Hara 2023). If a local politician is not forced to resign after an allegation, however, an employee with discriminatory preferences may become emboldened to act on this predisposition. In either case, we view high-profile misconduct allegations as a catalyst for cultural change and posit that their aftermath guides behavior by reinforcing norms of acceptable conduct.

We test this hypothesis using a "stacked" difference-in-differences (DiD) research design and the universe of misconduct allegations against members of U.S. Congress collected by GovTrack.us. Specifically, we compare within-firm changes in discrimination lawsuit frequency pre/post a political misconduct event (first difference) depending on the firm's headquarter location (second difference). The identifying assumptions are that exposure to a misconduct event is higher for firms headquartered in the politician's state (i.e., relevance) and that the frequency of litigation would have followed parallel trends at firms headquartered inside/outside the politician's

state absent the misconduct event (i.e., exogeneity). While these assumptions are inherently untestable, we believe they are realistic because individuals are more likely to be aware of local scandals due to news coverage and personal interactions (Gentzkow, Shapiro, and Sinkinson 2011; Giannetti and Wang 2016) but cannot influence the occurrence of a political misconduct event.

Our analyses suggest that cultural tolerance of misconduct shapes employee behavior. Discrimination litigation becomes 4% more frequent after the revelation of legislator misconduct if the politician does not resign – according to estimates from Poisson regressions that include firm and year fixed effects – but becomes roughly 9% *less* frequent if the politician *does* resign after the allegation. These results are robust across specifications that layer in industry-year fixed effects and control variables, and exhibit coefficient dynamics supporting the parallel-trends assumption.

To further assess the role of culture, we study within-firm changes in discrimination lawsuit frequency around CEO turnovers. Graham, Grennan, Harvey, and Rajgopal (2022) survey corporate executives and find that they consider the CEO to be the most influential factor setting corporate culture. Moreover, 85% of the executives "believe an ineffective culture increases the chances that an employee might act unethically or even illegally." We gauge this channel using a stacked DiD research design and CEO turnover data from ExecuComp. Our fixed-effects Poisson regressions reveal that the appointment of a female CEO is associated with a 19% reduction in discrimination litigation, but fail to reject the null that appointing a male CEO has no effect. Incremental to Tate and Yang's (2015) finding that female leadership moderates the gender pay gap for lower-level employees, our results indicate that the presence of a female CEO reduces other forms of discriminatory behavior, such as harassment, that might be more difficult than wages to change in a centralized manner.

We take several steps to mitigate the scope for alternative interpretations of our evidence.

First, we analyze cross-sectional variation in the estimated effect of our location-based cultural proxies and find that the relation between discrimination litigation frequency and local attitudes toward race and gender is significantly stronger for firms with operations more centralized in their headquarter state. Similarly, the drop in litigation after the resignation of a local politician is larger at more concentrated firms, implying that cultural effects diminish with distance from the shock. Second, we examine whether culture influences lawsuit outcomes and find that our shocks have no significant effect on the rate at which discrimination suits are dismissed or settled, lessening concerns about employees' inclination to file a frivolous suit or firms' willingness to settle. Together, these analyses bolster our conclusion about the influential role of culture in determining the frequency of workplace discrimination.

In contrast, we observe scant evidence that economic forces shape litigation rates. Using the China Trade Shock (Pierce and Schott 2016, 2018) and the American Jobs Creation Act (Cohn and Wardlaw 2016; Xu and Kim 2021) as natural experiments that exogenously increase competition and financial resources, respectively, we find no causal evidence that discrimination litigation is related to firm-level economic conditions. To better understand why discrimination might persist regardless of economic environment, we examine the effect of lawsuit filings on firm value. Consequences appear small: the median five-day cumulative abnormal return (CAR) around a filing is -10 basis points, which corresponds to roughly a million-dollar loss in value.[4] Moreover, losses tend to decrease with subsequent litigation, falling from -37 basis points for a firm's first lawsuit to -4 basis points after its tenth suit. Collectively, these findings cast doubt on Milton Friedman's claim that a "market solution" will eventually temper corporate discrimination.

[4] Our evidence on the cost of discrimination complements research on the benefits of an inclusive culture. Mkrtchyan, Sandvik, and Zhu (2023) find that shareholder value increases by 0.2% when CEOs speak out about diversity issues, and Lins, Roth, Servaes, and Tamayo (2022) show that firms with non-sexist corporate culture (i.e., those with women among the five-highest paid executives) earn excess returns of 1.6% during the Harvey Weinstein and #MeToo events.

Our paper contributes to the corporate finance and labor economics literature on workplace discrimination. Prior research focuses primarily on disparities in pay (Tate and Yang 2015; Sorkin 2017; Bennedsen, Simintzi, Tsoutsoura, and Wolfenzon 2022; Lagaras, Marchica, Simintzi, and Tsoutsoura 2023) and hiring/promotion (Kline, Rose, and Walters 2022; Sherman and Tookes 2022; Huang, Mayer, and Miller 2023). A general challenge for this stream of research is identifying whether differential labor market outcomes are driven by differences in human capital, preferences, or bias. Moreover, it is possible for employees to face disparate treatment even with the same wages/titles. For example, a growing literature explores differences in non-wage benefits (Liu, Makridis, Ouimet, Simintzi 2023; Ouimet and Tate 2023). We contribute by highlighting that workplace discrimination often involves harassment, an action devoid of economic merit, and by documenting the incidence of discrimination lawsuits against U.S. public corporations.

We find that discrimination litigation is concentrated in particular firms and geographic regions. Our interpretation that culture is a key source of these differences builds on research that links the incidence of corporate misbehavior with social norms, such as religious adherence (McGuire, Omer, and Sharp 2012), trust (Hayes, Jiang, and Pan 2021), and personal ethics (Biggerstaff, Cicero, and Puckett 2015; Davidson, Dey, and Smith 2015; Liu 2016; Parsons, Sulaeman, and Titman 2018). We advance this literature by highlighting the role of cultural tolerance; workplace discrimination lawsuits decrease after the revelation of misconduct by a local congressperson if the accused politician resigns but increase if the politician is not reprimanded.

Finally, we extend the literature on corporate Environmental, Social, and Governance (ESG) practices by studying civil rights litigation as a novel measure of a firm's 'Social' performance. Prior research suggests that corporate social responsibility can improve firm value by increasing consumer demand, raising labor productivity, and reducing risk (Flammer 2015;

Lins, Servaes, and Tamayo 2017; Albuquerque, Koskinen, and Zhang 2019). Hacamo (2023) shows that foot traffic declines at stores after an allegation of workplace racial prejudice is reported on a popular job-search website. Consistent with the notion that workplace discrimination erodes firm value, we find that lawsuit filings are met with negative stock price reactions. However, we find no evidence that discrimination litigation varies with product market competition or financial constraints. Instead, our results suggest discriminatory behavior is shaped by employee culture rather than optimized based on economic conditions.

2. Institutional background and data

2.1 Federal employment discrimination lawsuits

In the United States, federal law prohibits employment discrimination based on the following protected characteristics: race, color, national origin or ancestry, religion or creed, sex (including gender, pregnancy, sexual orientation, and gender identity), age, physical or mental disability, genetic information, citizenship, and veteran status. Title VII of the Civil Rights Act of 1964 (42 U.S.C. § 2000e) is the primary federal statute that protects applicants, employees, and former employees from workplace discrimination.[5] Title VII prohibits a wide set of actions that entail disparate treatment or disparate impact on members of a protected group, including adverse employment actions, failure to prevent or eliminate harassment, and retaliation.[6]

To file a discrimination lawsuit under Title VII, individuals must first complete the administrative process of filing a discrimination charge with the Equal Employment Opportunity

[5] Other key federal laws include Title I and Title V of the Americans with Disabilities Act (42 U.S.C. §§ 12101-12113), which prohibits discrimination based on disability, the Age Discrimination in Employment Act (29 U.S.C. §§ 621-634), which prohibits discrimination based on age, and the Genetic Information Nondiscrimination Act (42 U.S.C. § 2000ff), which prohibits discrimination based on genetic information.

[6] Adverse employment actions include demotion/termination, unfair compensation/benefits, failure to hire/promote, discriminatory preferences in job postings, discriminatory classification or segregation of employees/applicants, and failure to provide training/accommodations. Harassment includes quid pro quos and hostile work environments. Retaliation is a form of adverse employment action taken against an individual for opposing discriminatory practices, filing a discrimination charge, or assisting in a discrimination investigation.

Commission (EEOC), or an equivalent state or local fair employment practices agency (FEPA), within 180 days of the alleged incident.[7] The agency investigates the allegations and attempts to resolve violations. If the agency fails to conciliate a violation, it issues a right-to-sue letter allowing the individual to file a lawsuit in federal court within 90 days or, in rare circumstances, it files suit on behalf of the charging party.[8] Since the cause of action arises under federal law, employment discrimination is generally considered to be a federal question and therefore under the subject-matter jurisdiction of federal courts (28 U.S.C., § 1331).

We obtain federal lawsuit data from the Federal Judicial Center (FJC) Integrated Database. The FJC records the universe of federal litigation cases according to 28 U.S.C. § 620–629. We collect all litigation cases classified as "Civil Rights-Jobs" (Nature of Suit 442), merge defendant names with firms in the CRSP-Compustat Merged Database, and hand-clean matches to ensure accuracy. This process yields a sample of 36,244 employment discrimination lawsuits filed against 2,917 U.S. public companies with data available in Compustat and CRSP between 1992 and 2018. The sample begins in 1992 because the Civil Rights Act of 1991 (enacted November 21, 1991) strengthened many aspects of civil rights law, such as expanding the right to trial by jury and the right to sue for punitive damages. The sample ends in 2018 because it is the last full year that we have litigation data, given FJC reporting lags and the time elapsed between the filing and resolution of a typical case.

We believe that our FJC-Compustat sample is the most comprehensive dataset assembled

[7] It may be possible to cross-file a claim up to 300 days after an alleged incident with an authorized FEPA. These cases may be filed in state court if they involve only state claims and avoid a diversity of citizenship. Satisfying these conditions in discrimination cases is challenging because federal discrimination law is broad in scope and provides federal courts subject-matter jurisdiction over most discrimination cases.

[8] Discrimination complaints brought under the Age Discrimination in Employment Act and Equal Pay Act (EPA) do not require a right-to-sue letter. Suits alleging a violation of the EPA can be filed within two years of the last discriminatory paycheck. Since gender-based pay discrimination also violates Title VII, however, most complaints occur within 180 days of the incident so they can be brought under both the EPA and Title VII.

to date that contains workplace discrimination allegations matched to U.S. public firms. One popular alternative is to collect lawsuit announcements from the EEOC's website or news archives. However, this approach misses most cases because the EEOC files only a small number of lawsuits and news organizations cover only a few high-profile incidents.[9] A host of papers use the Violation Tracker database from GoodJobsFirst.org, which contains wide-ranging corporate misconduct data and penalties from over 450 government agencies, including the EEOC.[10] Due to its focus on government agencies, this dataset similarly misses most discrimination litigation. For example, Heese, Perez-Cavazos, and Peter (2022) merge Violation Tracker with U.S. public firms and report only 537 employment discrimination cases in their 2000-2017 sample. A final alternative is to obtain information on EEOC charges through Freedom of Information Act requests (Hersch 2011, 2018; Au, Barnes, and Tremblay 2023) or via special administrative access (Dahl and Knepper 2022, 2023). This approach yields a larger number of discrimination cases because it includes complaints that do not prompt a federal lawsuit, but cannot be merged to specific corporate defendants due to anonymization.

2.2 The nature of alleged discrimination

To better understand the nature of discrimination cases, we begin by performing textual analysis on complaint filings, which we obtain via Public Access to Court Electronic Records (PACER). We applied for a fee waiver to collect data from all 94 federal district courts and received a waiver from 64 of these districts.[11] Out of 64 districts granting fee waivers, 54 had

[9] For example, Barnes (2023) collects 131 lawsuit announcements from the EEOC's website between 2012 and 2018 and Borelli-Kjaer, Schack, and Nielsson (2021) obtain 212 sexual harassment cases from LexisNexis and EEOC press releases from 2005 to 2019. Omer Unsal studies several types of employee litigation across many papers and finds roughly 5,000 discrimination suits against S&P 1500 companies from 2000-2015 using Bloomberg and S&P Capital IQ (Unsal, Hassan, and Zirek 2017; Unsal 2019).

[10] See, e.g., Heese and Perez-Cavazos (2020), Li and Raghunandan (2021), Raghunandan (2022), Cai, Raghunandan, Rajgopal, and Wang (2023), Heese and Pacelli (2023), and Baker, Larcker, McClure, Saraph, and Watts (2023).

[11] Absent the fee waivers, this data would have cost roughly $300,000. Fee waivers are granted at the discretion of district courts. A list of waiver-granting and non-granting districts is available upon request.

downloadable complaint filings. Of these complaints, 97% were filed in the post-2000 period, reflecting advances in record-keeping technology. This data collection process yields a sample of 13,693 lawsuits for which we have access to the full-text case complaint document.

In Table 1, we use Latent Dirichlet Allocation topic modeling to classify complaint documents into categories. LDA forms clusters of words that tend to appear jointly in the text, enabling us to sort documents based on the relative use of each word cluster. Figure 1 presents word clouds illustrating the most salient terms in each LDA category, with word sizes indicating the relative importance to the topic grouping. To further visualize complaint types, Table 1 reports the top-10 words for each topic by saliency. For example, the top-5 words in Topic 5, "harassment", "work", "manager", "sexual", "environment", suggest that complaints in this category likely allege a hostile work environment. Complaints include many boilerplate terms, however, limiting our classification efforts. Therefore, we also extract the stated legal cause of action from case files and report the percentage in each topic.[12] While generic employment discrimination is the most common stated cause of action, the bottom row of Table 1 provides external validation for our LDA classification. Notably, 30% of cases in Topic 5 state a gender-based cause of action.

Table 1 also reports the percentage of complaints classified into each topic, the average number of words per document devoted to each topic, the average percentage of these words that are harassment-based, and the percentage of cases that cite retaliation. Harassment-based words are most frequent in Topic 5 complaints, averaging 5.35% of total words, but are often present

[12] The cause of action summarizes the set of allegations in a lawsuit. For each case, the FJC lists a cause of action corresponding to the law, statute, precedent, or regulation allegedly violated. Unfortunately, these classifications are often general. For example, the most common cause of action listed in our FJC-Pacer-Compustat matched sample is 42:2000 – Job Discrimination, which represents 49% of our sample. We find 158 unique causes of action in our sample, which we classify into 10 categories: General Employment, Race, Age, Gender, Disability, FMLA, Compensation, Religion, Miscellaneous, and Unknown.

even when a hostile work environment is not the focal topic.[13] A high percentage of cases appear to allege retaliation, with over 60% of complaints in seven of our eight LDA topics mentioning retaliation. In sum, 88% of complaints mention the word "retaliation" or include at least ten harassment-related words. Given that there is no economic justification for these actions, our evidence suggests that alleged workplace discrimination is often driven by animus.

Although the high prevalence of harassment and retaliation-based litigation is consistent with taste-based theories that argue discrimination is due to individual preferences, our data does not preclude the existence of "statistical" discrimination. Statistical theories posit that differential treatment arises because group identities convey information about productivity (Phelps 1972) or engender inaccurate stereotypes (Bordalo, Coffman, Gennaioli, and Shleifer 2016). Litigation data cannot capture the full extent of discrimination because plaintiffs must establish a plausible claim and some discriminatory behavior may not produce enough evidence to meet the burden of proof. Moreover, workplace discrimination is notoriously underreported (Dahl and Knepper, 2022). For example, Adams and Lowry (2022) find that 61% of female and 36% of male research faculty report having experienced discrimination in the field of finance during their last ten years. Therefore, we cannot speak to the share of unlitigated workplace discrimination. Instead, our goal is to use observational data to shed light on allegations of overt discrimination by U.S. public corporations, complementing research that studies the nature and extent of discrimination using laboratory/field experiments (Bohren, Haggag, Imas, and Pope 2023) and audit/correspondence studies (Kline, Rose, and Walters 2022).

2.3 Distribution of discrimination litigation

Table 2 presents descriptive statistics for our FJC-Compustat merged sample, which

[13] Our harassment-based word dictionary is available upon request.

consists of 95,045 firm-year observations from 10,237 U.S. public companies with data available in Compustat between 1992 and 2018. In contrast to economics research that argues "labor market discrimination is no longer a first-order quantitative problem in American society" (Heckman 1998), we find that employment discrimination lawsuits are prevalent. Roughly 30% of firms face at least one lawsuit during our sample period. Moreover, lawsuits are not uniformly distributed across firms, as one would expect if discrimination was randomly driven by "a few bad apples." Table 2 shows that the incidence of discrimination is concentrated and persistent, with the top 100 firms facing roughly 49% of all lawsuits in our sample but only contributing 24% (19%) of total employment (assets).

Figure 2 presents the industry and geographic distribution of discrimination litigation. Panel A shows that, among the 12 Fama-French industries, Healthcare and Business Equipment are consistently least sued, with less than 10% of firms facing a discrimination suit in a typical year, while Wholesale & Retail face the most litigation. Panel B reveals a strong geographic component in litigation frequency. Firms headquartered in the South and Rustbelt face discrimination lawsuits at roughly twice the rate as firms in the Northeast and West.[14] In the next section, we consider factors that may explain this variation using Poisson regressions and variance decompositions.

3. Which firms face discrimination lawsuits?

An employer's preference for discrimination is the key primitive in taste-based models. In Becker (1957), for example, employers are endowed with "discrimination coefficients" that capture their non-pecuniary value of indulging in workplace discrimination.[15] Becker notes that

[14] We assign headquarter location using zip codes listed in 10-Ks, supplemented with data from Compact Disclosure.
[15] While Becker (1957) describes discrimination as an aversion to interacting with the minority group, follow-on work models discrimination as malice toward the minority (Alexis 1973) or nepotism toward the majority (Goldberg 1982).

"although these coefficients are the proximate determinant of choices, they are in turn, like other tastes, influenced by more fundamental variables." We posit that culture is a fundamental determinant of this prejudice parameter and hypothesize that cultural and economic factors may influence the ability of employees to act on their preferences. In this section, we test these hypotheses by examining the relation between discrimination lawsuit frequency and firm, industry, and headquarter-area (HQ) characteristics. Appendix 1 and Appendix 2 report variable definitions and summary statistics, respectively.

Results in Table 3 provide initial support for the role of culture in determining the incidence of workplace discrimination. Poisson regressions in Panel A reveal that discrimination lawsuits are more frequently filed against firms headquartered in states with high rates of racial bias and reported sexism.[16] We measure racial bias as the difference between predicted and actual interracial marriage rates during 1970, following Levine, Levkov, and Rubinstein (2008), and define a "least sexist state" as those ranked in the lowest two sexism categories in the General Social Survey, following Giannetti and Wang (2023). Column (3) of Panel A shows that the expected number of discrimination suits is roughly 15% higher for firms headquartered in a state with one standard deviation above average racial bias and 29% lower for firms headquartered in one of the "least sexist" states, controlling for other demographic and firm characteristics. Moreover, Column (4) shows that discrimination suits are negatively associated with Corporate Social Responsibility (CSR) scores in pooled regressions that primarily exploit across-firm variation. Together, these findings support the view espoused by the co-chairs of the EEOC's Select Task Force on the Study of Harassment in the Workplace that "workplace culture has the

[16] We use fixed-effects Poisson regressions per Cohn, Liu, and Wardlaw's (2022) advice about count-based outcome variables. We also standardize all continuous variables to have unit variance to ease economic magnitude comparisons.

greatest impact on allowing harassment to flourish, or conversely, in preventing harassment."[17]

Turning to economic determinants, we find mixed results. No firm characteristics are significantly related to discrimination litigation across all specifications other than employee count and total assets. ROA has a positive coefficient in pooled regressions reported in Columns (1)-(3) but becomes insignificant when including CSR score in Column (4) and remains insignificant when including firm fixed effects in Columns (5)-(8). In contrast, a firm's buy-and-hold abnormal stock return over the previous fiscal year is insignificant in pooled regressions but negatively associated with lawsuits within-firm. Finally, coefficients on industry price-cost margin (proxying for market power) provide no support for Becker's (1957) proposition that discrimination should be less common in competitive industries.

Panel B presents an analysis of covariance (ANCOVA) that decomposes the explained variation in discrimination lawsuit frequency attributable to various factors. Using the methodology of Lemmon, Roberts, and Zender (2008), we run OLS regressions of lawsuit count on different combinations of lagged explanatory variables and fixed effects. We then compute the Type III partial sum of squares for each effect and normalize estimates by the sum across effects (forcing the sum to one) to understand the relative importance of each determinant. In Column (1), for example, we include firm, industry, and headquarter-area characteristics and year fixed effects as regressors. The adjusted R-squared for this regression is 0.31, indicating that the variables combine to explain 31% of the variation in discrimination lawsuit frequency. Number of employees and year fixed effects have the most explanatory power in this model, explaining 78% and 7% of the variation respectively.

Column (2) replaces year fixed effects with Fama-French 12 industry-year fixed effects.

[17] See: https://www.eeoc.gov/select-task-force-study-harassment-workplace-report-co-chairs-chai-r-feldblum-victoria-lipnic

This addition increases the adjusted R-squared by 3 percentage points, with industry-year fixed effects capturing 45% of the variation and reducing the amount explained by employee headcount by about half. Column (3) adds HQ-state fixed effects that further increase the R-squared by 2 percentage points and account for 19% of the explained variation, suggesting a meaningful role for local time-invariant cultural factors. However, the explanatory power of all preceding variables is dwarfed by the addition of CEO fixed effects in Column (4) and firm fixed effects in Column (5), which more than double the adjusted R-squared. Finally, we run a horse race between CEO and firm fixed effects in Column (6) and find that the model has an adjusted R-squared of 77%, with 18% of this variation explained by CEO fixed effects and 80% explained by firm fixed effects. These results suggest that variation in discrimination litigation is mostly due to time-invariant firm factors but can be influenced by senior management.

As an example of two firms that have notably different discrimination litigation rates despite similar observable characteristics, consider Lowe's and Home Depot. The companies operate similar home improvement stores across the U.S. that are often located across the street from each other. Yet, according to our data, Home Depot faces over ten times more discrimination suits per year than Lowes despite operating only 30% more stores. What explains this variation?

Econometrically, most of this variation is captured by firm fixed effects. Although ascribing an economic meaning to fixed effects is challenging, Gorton, Grennan, and Zentefis (2022) note that "fixed effects are the primary time-invariant measures of culture." Gorton and Zentefis (2020, 2023) model corporate culture using shared weights that represent the importance a group places on certain elements (e.g., particular norms and values). Managers set a desired corporate culture (i.e., a "tone at the top"), but actual culture can deviate from this intention based on employees' personal cultural weights and social interactions. Results in Table 3 suggest that

workplace discrimination rates are heavily influenced by the persistent component of corporate culture that is captured by firm fixed effects. However, we conjecture that high-profile events can trigger a change in social interaction and new leadership can shift tone at the top. Therefore, in the next section, we examine political scandals and CEO turnovers as cultural "shocks" that may spark within-firm changes in workplace discrimination.

4. Assessing the causal role of culture

The previous evidence paints a picture of taste-based workplace discrimination that varies with corporate culture. However, this interpretation is subject to an important caveat regarding measurement error: the filing of a "Civil Rights-Jobs" lawsuit in federal court against a corporation does not perfectly correspond to underlying discrimination. Some discriminatory behavior may not trigger a lawsuit filing and some lawsuit filings may be frivolous. This type of measurement challenge is common to all empirical research on crime (Chalfin and McCrary 2018) and corporate misconduct (Wang, Winton, and Yu 2010) where there is a conceptual difference between proxies and their unobservable counterpart. The effect of this error depends on its statistical properties. Similar to an omitted variable, measurement error may bias the coefficient if it is correlated with the dependent variable (Roberts and Whited 2012).

There are three stages in the litigation process where our measure of discrimination may depart from its unobserved counterpart. First, the employee must decide whether to ignore an incident of workplace discrimination or initiate a claim (and can also initiate a frivolous claim without a discriminatory incident occurring). Second, the employee can settle out of court or proceed to file a civil rights lawsuit. Third, the employee may have the option to file suit in state or federal court. Our concern is that these decisions may be correlated with cultural factors, which may lead to biased estimates.

Although it is impossible to completely rule out this issue, institutional details regarding legal procedure, discussed in Section 2.1, alleviate concerns that employees can forum shop or strategically time their lawsuit. Notably, federal courts have subject-matter jurisdiction over employment discrimination cases (28 U.S.C., § 1331), as it is prohibited by Title VII of the Equal Opportunity Act of 1964, and claims generally must be filed within 180 days. Moreover, in Section 4.1 and 4.2, we study within-firm variation around natural experiments to eliminate the possibility that our results are driven by unobserved, time-invariant differences (e.g., in the propensity to settle a claim before filing suit or to file suit in state, rather than federal, court). We further assess the credibility of our inferences in Section 4.3 by examining geographic heterogeneity in the effect of our location-based cultural proxies and by analyzing whether our independent variables of interest influence the rate at which discrimination suits are dismissed or settled.

4.1 Legislator misconduct events

Since culture is a multidimensional construct, we focus on tolerance of misconduct as one important element that may play a causal role in shaping workplace discrimination. In the models of Bernheim (1994) and Easley and O'Hara (2023), social norms influence the rate of nonethical behavior by altering the (potential) perpetrator's payoff. As the stigma associated with nonethical behavior increases, individuals become less likely to engage in nonethical conduct because doing so would impair their social status (Bernheim 1994) or cause guilt (Easley and O'Hara 2023). Further, cultural tolerance may influence the choice to behave nonethically by altering the perceived probability of punishment (Sah 1991). In either case, the key mechanism is the ability of culture to regulate social interactions through a shared normative order.

We study misconduct allegations against members of U.S. Congress as events that shift social norms while remaining plausibly exogenous to firm characteristics. According to the

GovTrack.us Legislator Misconduct Database, members of Congress faced 179 misconduct allegations between 1992 and 2018, including accusations of sexual harassment, abuse, bribery, fraud, and other ethics violations. We posit that a legislator misconduct event acts as a shock to local norms concerning acceptable behavior by revealing the consequences of improper conduct. If a local politician does not resign after an allegation, employees with discriminatory preferences may become more likely to act on this predisposition due to perceived cultural acceptability. If the accused politician does resign, however, we predict that the event will lead to fewer discrimination lawsuits at firms headquartered nearby.

Our empirical strategy tests this hypothesis using a stacked DiD research design that compares within-firm changes in discrimination litigation rates around political misconduct events, conditional on the firm's headquarter location. To cleanly estimate treatment effects, we follow Gormley and Matsa (2011) and construct cohorts of treated and control firms for two years pre/post each misconduct event, requiring control firms to not have been previously treated by another event. We choose a short event window because misconduct events should be most salient when they first hit newswires and to accord with the short, 180-day, statute of limitations typically required to file a discrimination charge.[18] We then stack each cohort into a combined dataset and estimate Poisson regressions on the stacked sample with cohort-specific firm and year fixed effects. This approach circumvents potential bias in staggered two-way fixed-effects DiD estimators by avoiding the use of already-treated control units (Baker, Larker, and Wang 2022).

Specifically, we estimate the following baseline stacked DiD specification:

$$\begin{aligned} \textit{Number of Discrimination Lawsuits}_{i,t,c} = {} & \beta \cdot \textit{Alleged Misconduct}_{i,t,c} + \theta \cdot \textit{Controls}_{i,t} \\ & + \gamma \cdot \textit{Firm-Cohort FE}_{i,c} + \tau \cdot \textit{Year-Cohort FE}_{t,c} + \varepsilon_{i,t,c} \end{aligned}$$

[18] Roberts and Whited (2012) recommend the use of short event windows because treatment effects should be concentrated near the onset of the event. Extending the sample window further away from the event enables confounding factors to affect outcomes and threatens internal validity.

where *Number of Discrimination Lawsuits*$_{i,t,c}$ is the number of employment discrimination lawsuits filed in federal court against firm *i* during year *t* within cohort *c*. *Alleged Misconduct*$_{i,t,c}$ is an indicator that equals one for firms headquartered in a state where a U.S. congressperson was accused of misconduct, and zero otherwise. It is implicitly an interaction between a firm HQ-state "treatment" variable and a "post" misconduct event-year indicator, but these constituent terms are absorbed by firm-cohort (*Firm-Cohort FE*$_{i,c}$) and year-cohort (*Year-Corhort FE*$_{t,c}$) fixed effects, respectively. *Controls*$_{i,t}$ are variables that account for time-varying firm characteristics. In some specifications, we include Fama-French 12 industry-year-cohort fixed effects (instead of year-cohort fixed effects) to control for industry trends. Finally, we cluster standard errors by headquarter state in all specifications.

As an example, consider the experience of Senator Al Franken (D-MN). In 2017, Sen. Franken promptly resigned after allegations of sexual misconduct became public and colleagues urged him to step down "in an overwhelming and seemingly unpartisan acknowledgement of a zero-tolerance policy surrounding sexual assault and harassment" (Golshan, 2017). Our identifying assumption is that the frequency of discrimination suits would have followed parallel trends at firms headquartered inside/outside of Sen. Franken's represented state, Minnesota, but for this misconduct event. If Sen. Franken's resignation shocks cultural norms of acceptable behavior, we predict that Minnesota firms will experience a relative decline in discrimination cases after the event. Importantly, we examine scandals in which the accused politician resigns separately from those without a resignation because misconduct events should have opposite effects on cultural tolerance depending on their aftermath. For example, we predict that Florida firms will experience a relative increase in discrimination cases after Rep. Alcee Hastings' (D-FL20) 2011 misconduct event, where he was accused of sexual harassment but did not resign.

Table 4 presents results for our legislator misconduct analyses. The baseline estimate in Column (1) implies that the number of discrimination lawsuits increases about 3.9% following a misconduct allegation where the politician does not resign.[19] Columns (2)-(4) show that the estimate of β is stable to the inclusion of industry-year-cohort fixed effects and controls. Across all models, our estimate of β is positive and statistically significant with economic magnitudes over 10% of the sample mean number of discrimination suits. These results suggest that workplace discrimination intensifies when misconduct is culturally tolerated.

Columns (5)-(8) repeat the regressions using misconduct events in which the politician resigns after the allegation. Coefficient estimates for $Alleged\ Misconduct_{i,t,c}$ are statistically significant and negative in all four models, implying a 6.8%-9.2% drop in the number of discrimination lawsuits against local firms. These results suggest that a zero-tolerance social norm tempers workplace discrimination. To probe the internal validity of these findings, we trace the timing of estimated behavior changes with event study plots in Figure 3. Panel A (Panel B) displays coefficient dynamics around misconduct events where the politician does not (does) resign after the allegation. The regression specifications are identical to those reported in Table 4 Columns (4) and (8), respectively, except that we include individual misconduct-event-time indicators instead of the post-misconduct indicator, so the magnitude of each point estimate is relative to the misconduct event year. The resulting treatment effect patterns plotted in Figure 3 support the parallel-trends assumption, with coefficients exhibiting a sharp break post-treatment. Collectively, these results suggest that social norms influence the occurrence of workplace discrimination.

4.2 CEO turnover events

[19] The Poisson regression coefficient is 0.038 (standard error = 0.018). Exponentiating this coefficient yields 1.039, which implies that a misconduct event with no resignation leads to a 3.9% increase in litigation count at local firms.

Our second natural experiment examines whether the frequency of discrimination litigation changes around the appointment of a new CEO. This analysis is motivated by prior theoretical and empirical research that shows key leaders can shape societal and corporate culture. For example, Acemoglu and Jackson (2015) develop an overlapping-generations model with norms that emerge as a result of social and historical factors but can change quickly in response to prominent leaders. Bertrand and Schoar (2003) track top executives across U.S. firms and find that managerial "style" influences a wide range of corporate behavior. We posit that managerial styles regarding tolerance for workplace discrimination likely differ across gender. Indeed, in a large survey of corporate directors and CEOs in Sweden, Adams and Funk (2012) find that females are more universally concerned and benevolent than males. Therefore, we hypothesize that the appointment of a female CEO may reduce discrimination at their firm by placing greater emphasis on norms of acceptable behavior.

We test this hypothesis using a stacked DiD design that compares within-firm changes in discrimination litigation around CEO turnover events, separately examining the appointment of female and male CEOs. As in Section 4.1, we follow the methodology of Gormley and Matsa (2011) to cleanly estimate treatment effects. Specifically, we construct cohorts of treated and control firms for two years pre/post each CEO turnover event, requiring control firms to not have been previously treated by another event, and stack each cohort into a combined dataset. We then estimate the following model on the stacked sample using fixed-effects Poisson regressions:

$$\begin{aligned} \textit{Number of Discrimination Lawsuits}_{i,t,c} = {} & \beta \cdot \textit{CEO Turnover}_{i,t,c} + \theta \cdot \textit{Controls}_{i,t} \\ & + \gamma \cdot \textit{Firm-Cohort FE}_{i,c} + \tau \cdot \textit{Year-Cohort FE}_{t,c} + \varepsilon_{i,t,c} \end{aligned}$$

where $\textit{Number of Discrimination Lawsuits}_{i,t,c}$ is the number of employment discrimination lawsuits filed in federal court against firm i during year t within cohort c. $\textit{CEO Turnover}_{i,t,c}$ is an indicator that equals one for firms that appointed a new CEO, and zero otherwise. It is implicitly an

interaction between a new CEO "treatment" variable and a "post" turnover event-year indicator, but these constituent terms are absorbed by fixed effects. We cluster standard errors by firm in this analysis because the key variation, CEO turnover, occurs at the firm-level (whereas the key variation in Section 4.1, legislator misconduct, was at the state-level).

To illustrate our empirical design, consider the experience of the Campbell Soup Company. At the beginning of fiscal year 2012, the Cambell Soup Company appointed Denise Morrison as Chief Executive Officer to replace Douglas Conant after his successful 10-year run leading the company. We create a clean 2x2 DiD dataset containing observations two years pre/post this CEO turnover event-year, coding all firms that appoint a female CEO in 2012 as treated and all other ExecuComp firms as controls (excluding "already treated" firms that appointed a female CEO during the "pre" period). We then combine this dataset with all other clean 2x2 datasets (centering around the various female CEO appointment event-years in our sample period) and run fixed-effects Poisson regressions on the stacked sample. As noted by Baker, Larker, and Wang (2022), "the stacked regression estimates the DiD from each of the clean 2x2 datasets, then applies variance weighting to combine the treatment effects across cohorts efficiently … while circumventing the problems introduced by staggered treatment timing and treatment effect heterogeneity." Roughly 3% of firm-years in our ExecuComp sample are led by a female CEO, mirroring statistics reported by He and Whited (2023), but the fraction is an order of magnitude lower at the firm-year-cohort level by nature of the sample construction procedure.

Table 5 presents results for our CEO turnover analyses. The baseline estimate in Column (1) implies that the number of discrimination lawsuits decreases about 22.1%, on average, after the appointment of a female CEO. Columns (2)-(4) show that the estimate of β is stable to the inclusion of industry-year-cohort fixed effects and controls, suggesting that female CEOs cultivate

an inclusive culture that reduces workplace discrimination. In contrast, estimates in Columns (5)-(8) provide no convincing evidence that the appointment of a male CEO influences discrimination litigation rates. Although the marginally significant coefficient in Column (6) hints that changing management, in general, may lead to a reduction in problematic behavior within the firm, this interpretation must be made with caution because CEO turnovers occur at firm inflection points with many confounding factors. Notably, when controlling for firm characteristics in Columns (7) and (8), the coefficient flips signs and becomes insignificant. Collectively, the results in Table 5 show that the appointment of a female CEO is associated with an economically large drop in discrimination lawsuits but fail to reject the null that appointing a male CEO has no effect on discrimination litigation.

Our results add to a growing literature that studies the impact of female leadership on firm behavior.[20] Cardoso and Winter-Ebmer (2010), Tate and Yang (2015), and Lagaras, Marchica, Simintzi, and Tsoutsoura (2023) find that the gender wage gap is smaller at female-led firms using administrative data from Portugal, the U.S., and the U.K., respectively. In contrast, Bradley, Knill, Lowry, and Williams (2023) find no evidence that female leadership reduces the wage gap between male and female business professors when controlling for research productivity. The literature also contains mixed results for the relation between female leadership and workplace harassment. Au, Tremblay, and You (2023) find that female leadership is negatively associated with the incidence of sexual harassment, based on textual analysis of online job reviews, while Adams, Akyol, and Grosjean (2021) find that the share of women in management is not significantly related to sexual harassment issues, based on textual analysis of forms submitted to Australia's gender-equality

[20] Adhikari, Agrawal, and Malm (2019) and Liu (2018, 2021) find that firms with female leaders face fewer operations-related lawsuits, consistent with lower risk-taking by female managers (Faccio, Marchica, and Mura 2016). Dadanlar and Abebe (2020, 2021) show that the presence of a female CEO or at least three female directors reduces the likelihood of a "large-scale discrimination lawsuit" in a sample of 452 U.S. firms and 1,083 lawsuits from 2010-2015.

agency. Our results add a unique perspective to this debate because discrimination lawsuit filings provide holistic measure of perceived disparate treatment.

An important caveat for research on CEO gender is that, unlike legislator misconduct, the occurrence of a CEO turnover is often endogenously determined by firm characteristics (e.g., poor performance). Moreover, Fee, Hadlock, and Pierce (2013) argue that corporate boards may select new leaders based on their managerial style. Although it is impossible to completely rule out these issues, the following features support our inferences. First, our male CEO analysis provides a useful falsification test, where null results lessen the concern that the estimated effect of a female CEO is spuriously driven by confounding factors inherent in CEO turnover events. Second, we conduct a pre-treatment balance test and find that the average number of discrimination lawsuits is not significantly different in the pre-period for firms that appoint a female CEO versus control firms. Finally, event study plots in Figure 4 appear to support the parallel-trends assumption. Panel A depicts a clear reduction in the number of discrimination lawsuits after the appointment of a female CEO while visual inspection of Panel B suggests that the appointment of a male CEO plays an inconsequential role. Together, this evidence reduces the plausibility that the estimated drop in discrimination lawsuits after the appointment of a female CEO is due to omitted variable bias or reverse causality.

4.3 Additional evidence supporting the role of culture

Thus far, our results show that cultural norms influence the frequency of workplace discrimination lawsuits. Below, we conduct several tests to assess the validity of the interpretation that observed changes in litigation rates reflect (unobservable) changes in discrimination rates.

We begin by analyzing cross-sectional heterogeneity in the effect of our location-based

proxies. Tables 3 and 4 show that discrimination litigation varies with cultural tolerance of racism, sexism, and misconduct in the firm's headquarter state. We use headquarter location in our main analysis because firms cluster key employes and core facilities around their headquarters (Pirinsky and Wang 2006). However, many firms (such as Home Depot) maintain geographically dispersed operations. If culture influences workplace discrimination, we expect the effect of location-based proxies to be stronger for firms with more operations concentrated in their headquarter state.

We test this hypothesis in Table 6 using subsidiary data from WRDS. The dataset contains parent company and subsidiary relationships reported in SEC filings since 1995. We construct a *Concentrated Operations* indicator variable that equals one for firms that have subsidiaries in three or fewer states and zero otherwise. The three-state cutoff is based on the sample median. We employ the same regression models as in Tables 3 and 4, except that we interact the independent variable of interest with the *Concentrated Operations* indicator and, for ease of interpretation, use a discrete version of the racism proxy, *Most Racist State*, that equals one if the firm's HQ state is in the top tercile of the *Racial Bias Index* distribution. The estimated coefficients reveal that HQ state-based cultural factors have a larger effect on geographically centralized firms. For example, the estimated effect of headquartering in a *Most Racist State* is nearly 59% higher for concentrated firms compared to dispersed firms [exp(0.463)-1=0.588]. Similarly, Panel B implies that the dampening effect of a misconduct-induced resignation is about 14% larger for firms with more local operations. Together, this cross-sectional heterogeneity lessens the plausibility that our findings are the byproduct of spurious correlations.

We next examine lawsuit outcomes to assess whether our main results capture changes in workplace discrimination or changes in the propensity to file/settle a lawsuit. Although our core interpretation is that misconduct-induced resignations influence workplace discrimination through

their impact on norms of acceptable behavior, the observed drop in litigation could instead reflect an increased propensity of firms to settle cases pre-filing to avoid negative publicity. Along the same lines, we interpret the drop in discrimination litigation after the appointment of a female CEO as evidence that their "tone at the top" reduced discriminatory conduct, but the drop could instead reflect a higher proclivity of female CEOs to settle cases pre-filing. Although we cannot directly examine pre-filing settlement rates, a change along this dimension should be reflected by an observable change in lawsuit outcomes (e.g., a higher dismissal rate).

Table 7 provides evidence that addresses these concerns. Panel A summarizes lawsuit outcomes, revealing that about 32% of cases are dismissed, 43% are settled, and 25% reach final judgment.[21] Panels B and C report Poisson estimates of the effect of cultural shocks on the rate at which discrimination suits are dismissed and settled, respectively. Across all models, we find no evidence that would suggest cultural factors influence the likelihood a case is dismissed or settled. Taken together, the insignificant relation between the shocks and case outcomes lends credibility to our conclusion that culture affects discrimination litigation rates by altering employee behavior.

While we acknowledge that lawsuit filings are an imperfect proxy of underlying discrimination, this additional evidence adds to the credibility of our inferences regarding the influential role of culture in determining the incidence of workplace discrimination.

5. Assessing the causal role of economic conditions

A popular interpretation of Becker (1957) argues that competition will eventually drive discrimination out of the workplace. Employers may be willing to forfeit profits to engage in discriminatory behavior but, in the long run with free entry or constant returns to scale, non-

[21] Unfortunately, we cannot analyze monetary awards/settlements because the FJC Documentation Codebook advises against the use of this data field, as it not mandatory or used uniformly by federal district courts.

discriminating firms will arbitrage this cost, expand, and eventually compete discriminating firms out of the market. Below, we evaluate this claim using two natural experiments that generate plausibly exogenous variation in economic conditions.

5.1 The China Trade Shock

In 2000, U.S. Congress unexpectedly granted China permanent Most-Favored Nation (MFN) trade status. Prior to this vote, China held temporary MFN status that required annual renewal. If Congress chose not to renew this status, U.S. tariffs on imports from China would have spiked from MFN rates, averaging less than 5%, to Non-Market Economy (NME) rates, averaging over 35%. The decision to grant China permanent MFN status in 2000 resolved years of tariff uncertainty and led to a massive increase in imports from China (Pierce and Schott 2016).

Pierce and Schott (2016, 2018), among others, show that the "China Trade Shock" decreased profitability and increased exit rates of U.S. firms. The authors employ a DiD research design that compares U.S. manufacturers before and after China obtained permanent MFN trade status in 2000 depending on industry-level variation in tariff rates. Importantly, exposure to the shock is plausibly exogenous because it derives from variation in NME tariff rates set 70 years prior by the Smoot-Hawley Act. Following Griffin (2023), we estimate:

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

$Y_{i,t}$ represents our outcome variables. $Post_t$ is an indicator that turns from zero to one when China attains permanent MFN status in year 2000. $China\ Trade\ Shock\ Exposure_i$ is a continuous variable that measures the potential tariff hike faced by firm i's industry prior to 2000 (i.e., the gap between MFN and NME tariff rates). Our coefficient of interest, β, is on the interaction of these two variables, capturing the effect of China's receipt of permanent MFN status conditional on firm exposure. $Controls_i$ are variables that account for confounding factors at the industry level, $Firm\ FE_i$ and $Year\ FE_t$ are firm and year fixed effects included in all specifications, and

standard errors are clustered by industry to match the industry-level variation in tariff exposure.

Panel A of Table 8 reports the results. Columns (1)-(2) confirm that the China Trade Shock did indeed have a statistically significant and economically large negative effect on ROA. Despite this shock to profitability, we find no evidence that increased product market competition reduces workplace discrimination. Our Poisson regression estimates in Columns (5)-(7) are positive and insignificant, failing to reject the null that the shock had no effect on litigation rates. Reinforcing this conclusion, the event study plot in Panel A of Figure 5 exhibits no clear break in trend. These (non)results comport with research that argues discrimination can persist if there is imperfect information, imperfect competition, or adjustment costs (Charles and Guryan 2008).

Our null results contrast with prior research that shows a loss in market power reduces wage/hiring gaps. Closest to our setting, Black and Brainerd (2004) show that the change in the gender wage gap between 1976 and 1993 was negatively correlated with the change in import penetration in concentrated industries.[22] We believe that the China Trade Shock provides a cleaner empirical setting and more reliable inferences than prior research. However, results may also differ because our outcome variable provides a more comprehensive measure of workplace discrimination than wage/hiring gaps alone.

5.2 The American Jobs Creation Act

Hart (1983) argues that financial slack increases the scope for managers to extract private benefits rather than maximize firm value. Interpreting through the lens of taste-based models, we predict that financial slack increases employers' ability to indulge in discriminatory behavior. To test this hypothesis, we examine the 2004 American Jobs Creation Act (AJCA). Faulkender and

[22] Ashenfelter and Hannan (1986) and Black and Strahan (2001) show that competition is associated with a higher share of female workers and a lower gender wage gap in the banking industry. Heywood and Peoples (1994) find that deregulation increased the number of black drivers in the trucking industry.

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. Cohn and Wardlaw (2016) and Xu and Kim (2021) use this natural experiment to show that a reduction in financial constraints significantly decreases workplace injury rates and toxic emissions, respectively. We employ their research design to determine whether financial resources affect the frequency of workplace discrimination lawsuits.

Following Cohn and Wardlaw (2016), 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 the following DiD specification using Poisson regressions:

$$Y_{i,t} = \beta \cdot Post_t * AJCA\ Exposure_i + \theta \cdot Post_t * AJCA\ Exposure_i * Leverage_{i,t} + \gamma \cdot Firm\ FE_i + \tau \cdot Industry\ FE_i * Year\ FE_t + \omega \cdot State\ FE_i * Year\ FE_t + \varepsilon_{i,t.}$$

$Y_{i,t}$ represents our outcome variables. $Post_t$ is an indicator equal to one for observations in 2005 and 2006 and zero for observations in 2002 and 2003. $AJCA\ Exposure_i$ is an indicator equal to one if firm i has positive foreign profits from 2001 through 2003, and zero otherwise. Our coefficient of interest is the interaction of these two variables, β, which captures the effect of the AJCA for firms with positive foreign profits. The coefficient θ captures the AJCA's interaction with firm leverage. This triple interaction measures whether the AJCA's effect is stronger for firms with greater financial constraints. If financial slack allows for more discrimination, we expect β and/or θ to be positive. Finally, we include firm, industry-year, and state-year fixed effects and cluster standard errors by firm.

Table 8, Panel B presents the results. Columns (1)-(2) confirm that the AJCA led to more

shareholder payouts at exposed firms (Blouin and Krull 2009; Dharmapala, Foley, and Forbes 2011). However, our estimates of the AJCA's effect on discrimination litigation are statistically indistinguishable from zero in Columns (3)-(5) and exhibit no clear trend in Panel B of Figure 5. These estimates offer no strong evidence for the conjecture that financial slack influences workplace discrimination, even for constrained firms with high leverage. Relative to Cohn and Wardlaw (2016), who show that the AJCA cash windfall led to a decrease in workplace injury rates, our null results suggest that firms lack the incentive or ability to reduce the incidence of workplace discrimination after a relaxation of financial constraints.

6. Stock market response to discrimination lawsuit filings

The above analyses provide no evidence to support the assertion that market forces can temper employment discrimination. In this section, we shed additional light on these null results by examining shareholder value consequences of discrimination litigation. If consequences are small, there may be insufficient scope for economic forces to limit discrimination.

Panel A of Table 9 reports mean and median percentage cumulative abnormal returns (CARs) for our sample of 36,244 lawsuit filings against 2,917 U.S. public companies with data available in Compustat and CRSP between 1992 and 2018. Average 5-day market model CARs and Fama-French 3 Factor CARs equal -0.041% and -0.059%, respectively, equivalent to about a $25 million drop in value for the average corporate defendant. Median CARs are slightly larger in percentage terms, from -0.084% to -0.098%, but much smaller in dollar terms, corresponding to roughly a $1 million value loss for the typical defendant.[23] The small stock price reaction to

[23] For comparison, Hersch (1991) studies 260 discrimination cases reported in the Wall Street Journal and finds that shareholder value drops 0.48% and 0.29% around reports of lawsuit filings and settlements, respectively. Selmi (2003) studies 33 discrimination lawsuits and finds no significant effect on stock prices from the announcement of either a filing or a settlement. Hirsh and Cha (2015) examine 174 sex and race discrimination lawsuits and find that shareholder value drops 0.80%, on average, around verdicts and settlements.

discrimination suit filings is consistent with Gantchev, Giannetti, and Li (2022), who show that environmental and social incidents are followed by some, but relatively small, divestitures.

In Panel B, we assess whether investor reactions differ across key sample splits. Sorting on lawsuit outcome, we find no significant difference in CARs across cases that were ultimately dismissed, settled, or reached a final judgment, nor do we find significant differences across cases with a verdict in favor of the plaintiff versus those in favor of the defendant. These null results offer no evidence that would suggest investors can assess case merit at the point of filing. In contrast, market responses do appear to differ by plaintiff and defendant type. The median lawsuit filed by the EEOC is met with a 31 basis point larger stock price drop, consistent with the EEOC bringing forth only a few high-profile cases each year. Similarly, we find that CAR magnitudes are largest for a firm's first suit and decline with additional suits. This result suggests that stock reactions may be dampened by anticipation for frequent defendants (Gande and Lewis 2009).

Collectively, the results show that discrimination litigation destroys shareholder wealth, but the economic magnitude is small, given the negative 0.04% average market model CAR. For comparison, Karpoff, Koester, Lee, and Martin (2017) find that the average securities class action suit alleging financial misrepresentation elicits a negative 1.24% stock price reaction and Bizjak and Coles (1995) find that antitrust litigation defendants experience a negative 0.60% reaction, on average. We conclude that the relatively modest investor reactions to discrimination lawsuit filings provide insufficient incentives to deter future workplace discrimination.

7. Conclusion

In this paper, we use novel litigation data from the Federal Judicial Center to document the incidence of workplace discrimination, uncover its determinants, and assess whether economic forces provide a source of discipline. We find that allegations of workplace discrimination are

prevalent – with over 36,000 federal civil rights lawsuits filed against nearly 3,000 U.S. public firms between 1992 and 2018 – but are concentrated among particular firms and geographic regions. Variation in litigation rates can best be explained by differences in corporate culture. The typical defendant in our sample is a large corporation with a low CSR score and male CEO that is headquartered in an area where residents hold more racist and sexist views. Using textual analysis to gain insight into the nature of discrimination, we find that complaint documents almost always include allegations of harassment or retaliation. These findings echo key features of the seminal taste-based model of discrimination formalized by Becker (1957), which considers discrimination to be driven by individual prejudice with no economic merit. However, we find little evidence supporting dynamic interpretations of Becker's model. Discrimination litigation does not appear to vary with firm-level economic conditions and lawsuit filings correspond to less than a ten basis point loss in market capitalization for the typical corporate defendant.

An important caveat is that we can only observe discrimination allegations that advance to federal court. Although studying civil rights litigation is important in its own right, lawsuits do not map perfectly to underlying discrimination. Therefore, we highlight how the progression between perceived discrimination and a federal lawsuit might affect inferences and take several steps to mitigate the scope for alternative interpretations. First, our focus on within-firm variation around shocks to culture and economic conditions eliminates the possibility that results are driven by unobserved, time-invariant differences (e.g., in the propensity to settle a claim before filing suit). Second, cross-sectional tests confirm that the estimated effect of location-based proxies is stronger for firms with more concentrated operations in their headquarter state, lessening the plausibility that our main estimates are spurious. Third, analyses of case outcomes reveal no significant relation between our cultural shocks and the rate at which discrimination suits are dismissed or

settled. These findings, combined with institutional details in the paper, support the reliability of our inferences and paint a clear picture of taste-based discrimination.

Collectively, our findings suggest that discrimination is ingrained in employee culture and unlikely to be disciplined by market forces. These results echo Kenneth Arrow's (1973) famous criticism of Becker's model. 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." Our results instead suggest that a key mechanism determining the level of workplace discrimination is cultural tolerance of such incidents. Given that discrimination is already prohibited by law, our evidence implies that grassroots movements promoting a zero-tolerance culture toward misconduct may be the most effective method to reduce workplace discrimination.

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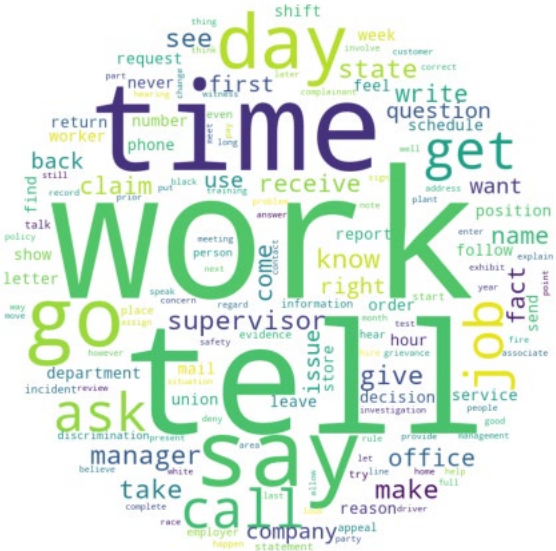
Figure 1: Discrimination lawsuit word clouds. This figure categorizes the contents of employment discrimination lawsuits filed against U.S. public companies between 1992 and 2018. Our full sample consists of 36,244 employment discrimination lawsuits (Nature of Suit 442-Civil Rights Job) in the Federal Judicial Center (FJC) Civil Integrated Database with a defendant in Compustat. We employ Latent Dirichlet Allocation (LDA) topic modeling analysis on a subsample of 13,693 lawsuits for which we have access to the case complaint document via PACER. Word clouds illustrate the most salient terms in each LDA topic grouping, with sizes indicating relative importance.

Topic 1

Topic 2



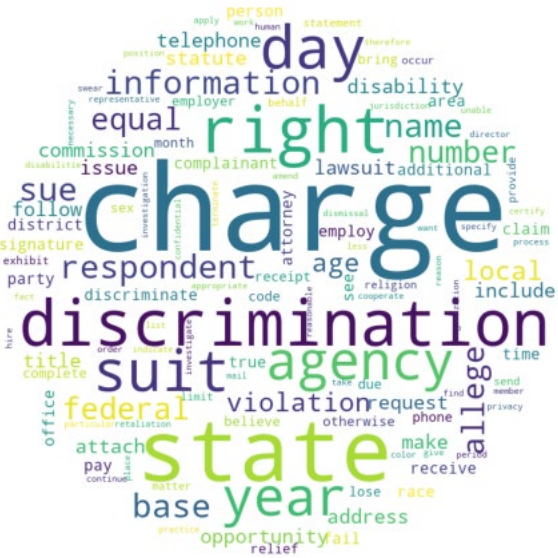
Topic 3

Topic 4

Figure 1: Discrimination lawsuit word clouds. (Continued)

Topic 5

Topic 6

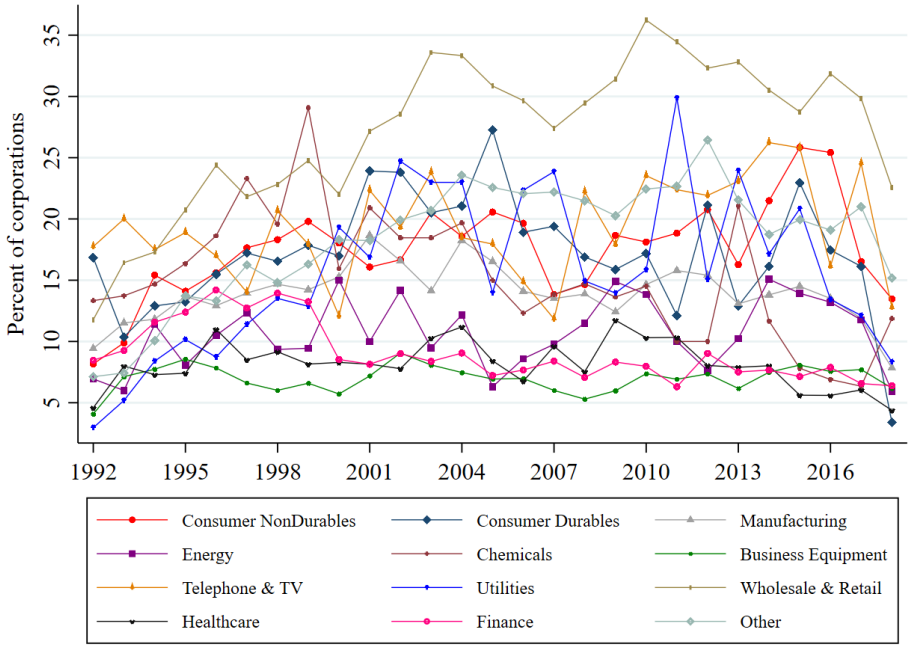


Topic 7

Topic 8

Figure 2: Distribution of discrimination lawsuits. This figure displays the fraction of U.S public companies facing an employment discrimination lawsuit in federal court. The full sample consists of 95,045 firm-year observations from 10,237 U.S. public companies with data available in Compustat between 1992 and 2018. Panel A splits the sample by Fama-French 12 industry classification and calendar year. Panel B splits the sample by headquarter location.

Panel A: Industry distribution



Panel B: Geographic distribution

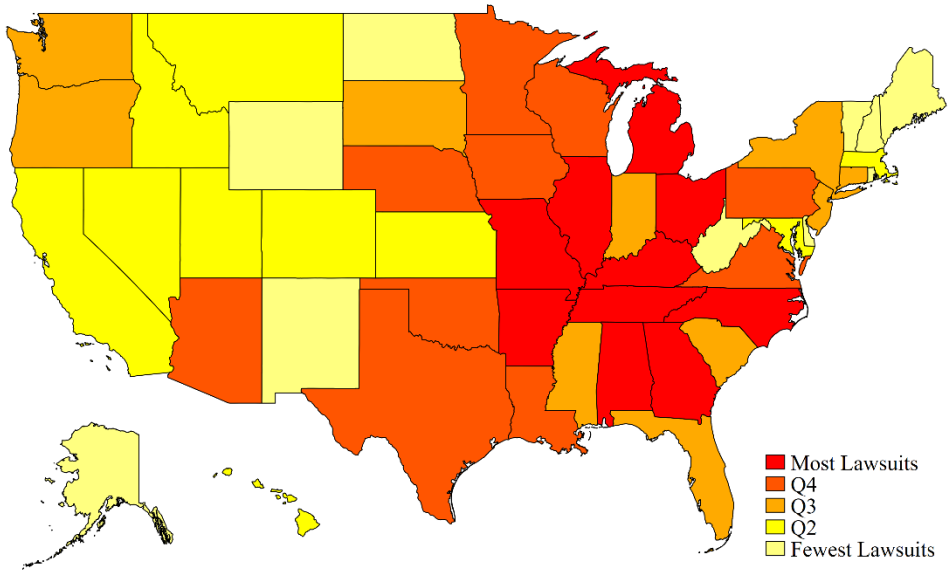
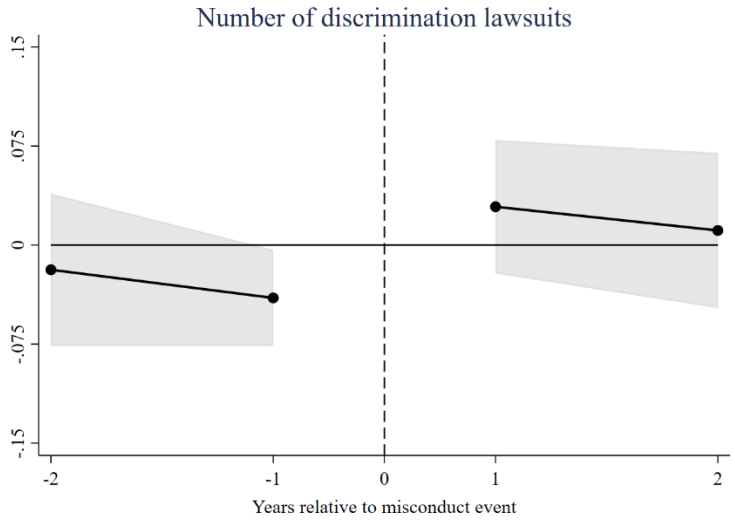


Figure 3: Cultural tolerance and discrimination lawsuit dynamics. This figure plots the effect of a misconduct allegation against a U.S. congressperson on the number of discrimination lawsuits filed against firms headquartered in the represented state. Panel A (Panel B) displays coefficient dynamics around misconduct events where the politician does not (does) resign after the allegation. To cleanly estimate treatment effects, we construct cohorts of treated and control firms for two years pre/post each misconduct event, requiring control firms to not have been previously treated by another event, and pool the data across cohorts. The Poisson regression specification is identical to that reported in Table 4 Column (4), except that we include individual misconduct-event-time indicators instead of the post-misconduct indicator, so the magnitude of each point estimate is relative to the misconduct event year. Shaded areas display 90% confidence intervals, adjusted for clustering by state. Appendix 1 provides variable definitions.

Panel A: Politician does not resign after alleged misconduct



Panel B: Politician resigns after alleged misconduct

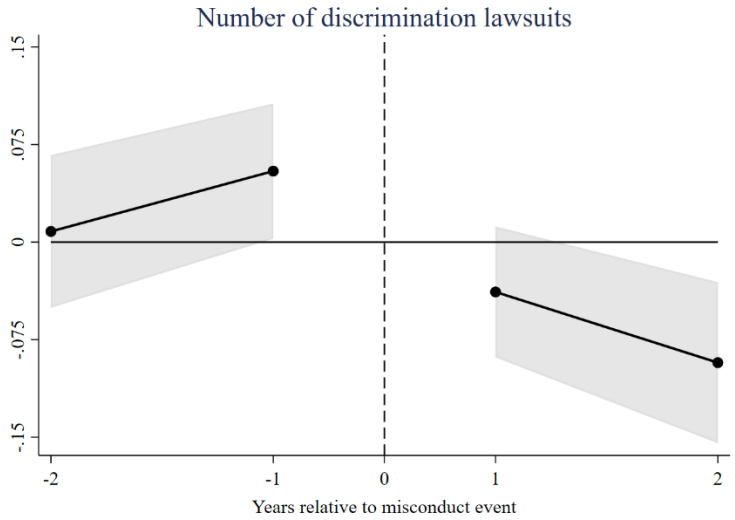
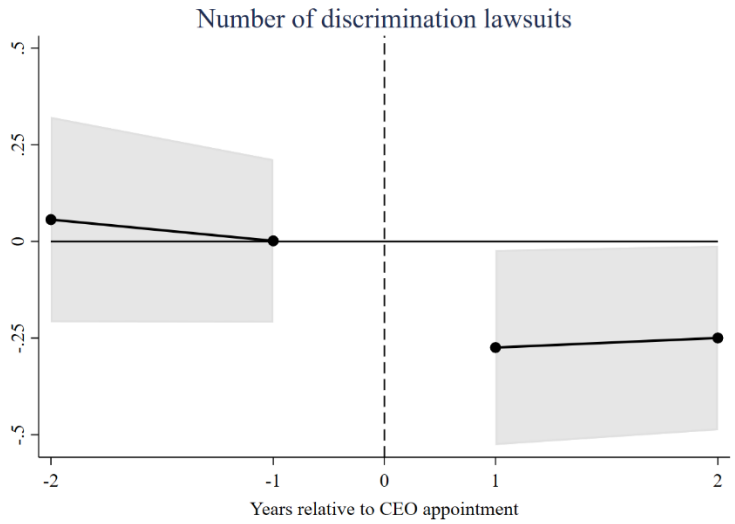


Figure 4: CEO turnover and discrimination lawsuit dynamics. This figure plots the effect of CEO turnover on the number of discrimination lawsuits filed against a firm. Panel A (Panel B) displays coefficient dynamics around turnover events where a female (male) CEO is appointed. To cleanly estimate treatment effects, we construct cohorts of treated and control firms for two years pre/post each CEO turnover event, requiring control firms to not have been previously treated by another event, and pool the data across cohorts. The Poisson regression specification is identical to that reported in Table 5 Column (4), except that we include individual turnover-event-time indicators instead of the post-turnover indicator, so the magnitude of each point estimate is relative to the appointment event year. Shaded areas display 90% confidence intervals, adjusted for clustering by firm. Appendix 1 provides variable definitions.

Panel A: Female CEO appointment



Panel B: Male CEO appointment

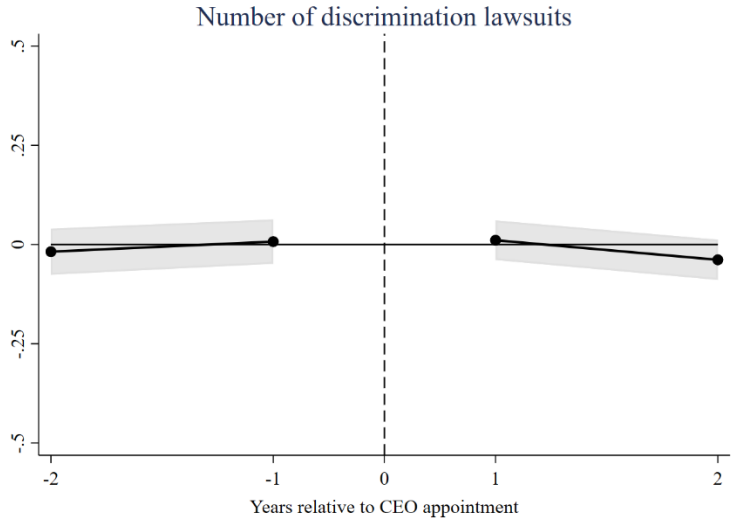
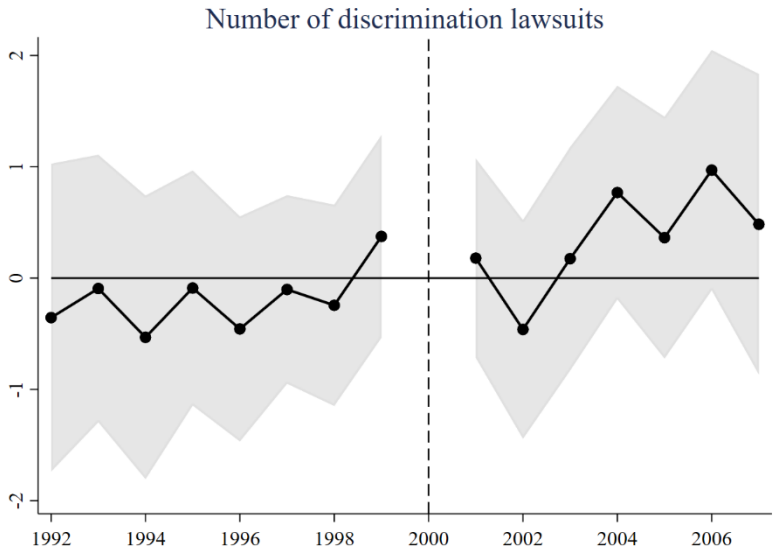


Figure 5: Economic conditions and discrimination lawsuit dynamics. This figure plots the effect of changing economic conditions on the number of discrimination lawsuits filed against a firm. Panel A plots the dynamic effect of import competition using the same regression specification as Table 8A Column (4), except that China Trade Shock Exposure is interacted with annual dummies instead of a post 2000 indicator. Panel B plots the dynamic effect of a cash windfall using the same regression specification as Table 8B Column (4), except that American Jobs Creation Act Exposure is interacted with annual dummies instead of a post 2004 indicator. The gray shading represents 90% confidence intervals using heteroskedasticity-consistent standard errors clustered by industry in Panel A and firm in Panel B. Appendix 1 provides variable definitions.

Panel A: Increased import competition



Panel B: Relaxed financial constraints

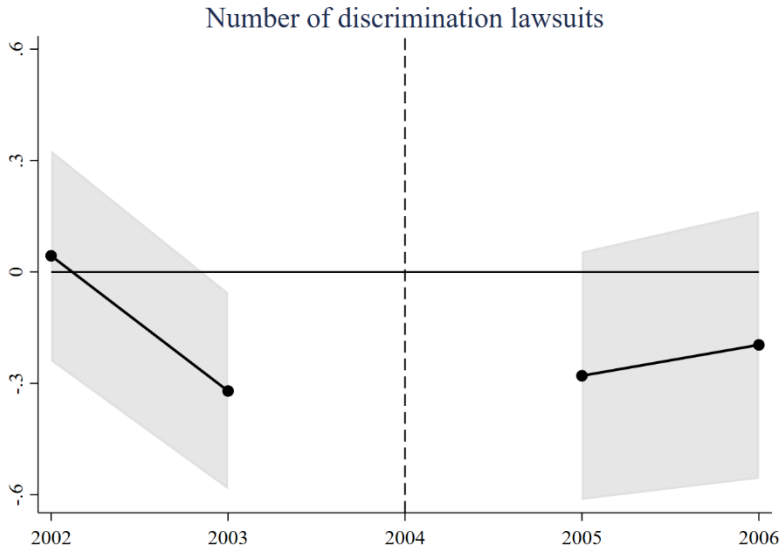


Table 1: Discrimination lawsuit topics. This table categorizes the contents of employment discrimination lawsuits filed against U.S. public companies between 1992 and 2018. We employ LDA topic modeling analysis on a sample of 13,693 lawsuits for which we have access to the case complaint document via PACER. The table reports the percentage of lawsuits classified to each topic, the average number of words per document devoted to each topic, the average percentage of these words that are harassment-based, the percentage of cases that are due to retaliation, the ten most salient words in each topic, and the percentage of cases by legal cause of action in each topic.

Topic Number	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Complaint (%)	38.62%	6.21%	11.14%	10.71%	6.62%	16.80%	3.82%	6.09%
Number of Words	981	2,203	1,225	2,418	1,509	1,143	1,648	3,114
Harassment Words (%)	3.45%	1.99%	2.53%	1.72%	5.35%	2.22%	2.31%	1.40%
Retaliation (%)	62%	61%	63%	61%	81%	78%	67%	53%
Most Salient Words	damage	work	disability	Service	harassment	charge	class	company
	discrimination	tell	leave	damage	work	state	personal	performance
	right	time	work	attorney	manager	discrimination	property	information
	violation	say	medical	party	sexual	right	liability	position
	relief	go	damage	serve	environment	day	labor	manager
	practice	day	accommodation	allege	conduct	suit	product	pay
	suffer	call	time	state	hostile	agency	injury	sale
	attorney	ask	request	cause	store	year	place	time
	trial	job	reasonable	claim	make	respondent	state	plan
	conduct	get	violation	name	tell	sue	enter	year
Distribution of Causes	38% Emp.	52% Emp.	41% Disability	51% Unknown	45% Emp.	46% Emp.	36% Emp.	38% Emp.
	22% Race	18% Race	23% Emp.	23% Emp.	30% Gender	21% Race	16% Age	14% Age
	16% Age	8% Unknown	20% FMLA	7% Race	11% Race	12% Disability	13% Disability	13% Unknown
	9% Gender	7% Disability	8% Unknown	5% Disability	7% Unknown	9% Age	12% Gender	12% Race
	6% Disability	7% Age	3% Age	5% Age	3% Disability	8% Gender	12% Race	8% Disability
	4% Unknown	5% Gender	2% Comp.	4% Gender	2% Age	3% Unknown	5% Unknown	5% Gender
	2% Misc	2% Misc	2% Race	3% FMLA	1% Misc	1% Misc	3% FMLA	3% Misc
	1% FMLA	1% FMLA	1% Misc	2% Misc	1% FMLA	0% FMLA	2% Misc	3% FMLA
	1% Comp.	0% Religion	1% Gender	1% Comp.	0% Religion	0% Comp.	1% Comp.	2% Comp.
	0% Religion				0% Comp.	0% Religion		1% Religion

Table 2: Incidence of discrimination lawsuits. This table reports the distribution of employment discrimination lawsuits filed against U.S. public corporations between 1992 and 2018. The sample consists of 95,045 firm-year observations from 10,237 U.S. public companies with data available in Compustat.

Average number of discrimination suits per firm-year	0.313
Percent of firms that face a discrimination suit:	
Never	70.802
One year	11.371
Two to five years	11.312
Six to ten years	3.605
Eleven to fifteen years	1.426
Sixteen to twenty years	0.811
More than twenty years	0.674
Percent of total discrimination suits filed against:	
Firms in the bottom 25th percent of employee count	0.388
Firms in the 25th to 50th percent of employee count	1.914
Firms in the 51st to 75th percent of employee count	9.553
Firms in the top 25th percent of employee count	87.490
Percent of total discrimination suits filed against:	
Firms in the bottom 25th percent of assets	0.920
Firms in the 25th to 50th percent of assets	4.850
Firms in the 51st to 75th percent of assets	14.111
Firms in the top 25th percent of assets	80.080
Top 100 firms facing discrimination suits contribute to:	
Percent of total discrimination suits	49.165
Percent of total employment	24.019
Percent of total assets	18.938

Table 3: Which firms face discrimination lawsuits? Panel A reports estimates from Poisson regressions that relate the number of discrimination lawsuits to lagged firm, industry, and headquarter-area characteristics. Panel B presents a variance decomposition for several model specifications. We compute the Type III partial sum of squares for each effect in the model and then normalize each estimate by the sum across effects, forcing the sum to one. The full sample consists of 95,045 firm-year observations from 10,237 U.S. public companies with data available in Compustat 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. Appendix 1 lists variable definitions.

Panel A: Poisson regressions

	Number of Discrimination Lawsuits							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Employees	1.431***	1.401***	1.399***	1.204***	0.588***	0.517***	0.566***	0.491***
	(0.048)	(0.049)	(0.048)	(0.048)	(0.101)	(0.115)	(0.097)	(0.115)
Size	0.250***	0.282***	0.293***	0.219***	0.384***	0.372***	0.391***	0.398***
	(0.047)	(0.050)	(0.051)	(0.047)	(0.112)	(0.109)	(0.104)	(0.105)
Age	-0.009	-0.010	-0.011	-0.024	0.205***	0.090	0.177***	0.027
	(0.030)	(0.030)	(0.030)	(0.034)	(0.059)	(0.081)	(0.057)	(0.081)
Stock Return	-0.009	-0.012	-0.013	-0.022	-0.029***	-0.023**	-0.023**	-0.017
	(0.015)	(0.015)	(0.015)	(0.018)	(0.010)	(0.012)	(0.010)	(0.011)
ROA	0.263***	0.272***	0.233***	0.085	0.015	-0.027	0.021	-0.014
	(0.069)	(0.069)	(0.068)	(0.064)	(0.024)	(0.027)	(0.022)	(0.026)
Market-to-Book Ratio	-0.016	0.001	0.030	0.045	0.041*	0.076***	0.034*	0.062***
	(0.041)	(0.041)	(0.039)	(0.044)	(0.023)	(0.022)	(0.019)	(0.021)
Leverage	0.060**	0.061**	0.055*	0.059*	-0.015	-0.045	-0.019	-0.044
	(0.028)	(0.029)	(0.029)	(0.034)	(0.021)	(0.029)	(0.020)	(0.028)
Investment Grade	0.152*	0.140*	0.130*	0.181*	-0.047	-0.023	-0.039	-0.028
	(0.078)	(0.078)	(0.077)	(0.096)	(0.053)	(0.077)	(0.051)	(0.075)
Speculative Grade	0.170**	0.154**	0.153**	0.143	0.054	0.033	0.065	0.025
	(0.072)	(0.072)	(0.070)	(0.098)	(0.055)	(0.077)	(0.054)	(0.077)
Industry Price-Cost Margin		-0.057*	-0.028	-0.019		0.017		
		(0.031)	(0.031)	(0.036)		(0.035)		
HQ Income Per Capita			-0.013	0.009				
			(0.063)	(0.061)				
HQ Unemployment Rate			-0.008	0.001				
			(0.039)	(0.046)				
HQ Black Population			0.023	0.018				
			(0.027)	(0.032)				
HQ Rural Area			-0.027	0.105				
			(0.121)	(0.142)				
HQ Racial Bias Index			0.142***	0.126***				
			(0.034)	(0.040)				
HQ Least Sexist State			-0.253**	-0.228*				
			(0.102)	(0.126)				
CSR Score				-0.042**		-0.010		-0.006
				(0.021)		(0.014)		(0.014)
Firm Fixed Effects	No	No	No	No	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Industry-Year Fixed Effects	No	No	No	No	No	No	Yes	Yes
Observations	95,045	95,045	95,045	25,285	40,911	13,725	40,911	13,725
Pseudo R-squared	0.444	0.444	0.448	0.413	0.476	0.476	0.481	0.482

Table 3: Which firms face discrimination lawsuits? (Continued)

Panel B: Variance decomposition

	Number of Discrimination Lawsuits					
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Employees	0.78	0.35	0.30	0.00	0.00	0.00
Size	0.04	0.09	0.08	0.00	0.00	0.00
Age	0.00	0.00	0.00	0.00	0.00	0.00
Stock Return	0.00	0.00	0.00	0.00	0.00	0.00
ROA	0.01	0.01	0.00	0.00	0.00	0.00
Market-to-Book Ratio	0.02	0.02	0.02	0.00	0.00	0.00
Leverage	0.00	0.00	0.00	0.00	0.00	0.00
Investment Grade	0.00	0.00	0.00	0.00	0.00	0.00
Speculative Grade	0.01	0.02	0.02	0.00	0.00	0.00
Industry Price-Cost Margin	0.00	-	-	-	-	-
HQ Income Per Capita	0.01	0.02	0.00	0.00	0.00	0.00
HQ Unemployment Rate	0.00	0.00	0.00	0.00	0.00	0.00
HQ Black Population	0.00	0.00	0.00	0.00	0.00	0.00
HQ Rural Area	0.00	0.00	-	-	-	-
HQ Racial Bias Index	0.01	0.01	-	-	-	-
HQ Least Sexist State	0.01	0.01	-	-	-	-
CSR Score	0.02	0.02	0.01	0.00	0.00	0.00
Year Fixed Effects	0.07	-	-	-	0.01	0.01
Industry-Year Fixed Effects	-	0.45	0.37	0.02	-	-
HQ State Fixed Effects	-	-	0.19	0.00	-	-
Firm Fixed Effects	-	-	-	-	0.99	0.80
CEO Fixed Effects	-	-	-	0.98	-	0.18
Adjusted R-squared	0.31	0.34	0.36	0.77	0.74	0.77

Table 4: Cultural tolerance and discrimination lawsuits. This table reports the effect of a misconduct allegation against a U.S. congressperson on the number of discrimination lawsuits filed against firms headquartered in the represented state. To cleanly estimate treatment effects, we construct cohorts of treated and control firms for two years pre/post each misconduct event, requiring control firms to not have been previously treated by another event, and pool the data across cohorts. Columns (1)-(4) include treatment events where the politician does not resign after the allegation and Columns (5)-(8) include treatment events where the politician does resign. Heteroskedasticity-consistent standard errors clustered by state are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Appendix 1 lists variable definitions.

	Number of Discrimination Lawsuits							
Aftermath:	**Politician Does Not Resign After Allegation**				**Politician Resigns After Allegation**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Alleged Misconduct	0.038**	0.034*	0.046***	0.043**	-0.096***	-0.084***	-0.083**	-0.070**
	(0.018)	(0.018)	(0.017)	(0.017)	(0.035)	(0.031)	(0.037)	(0.031)
Number of Employees			0.365***	0.373***			0.407***	0.422***
			(0.103)	(0.104)			(0.115)	(0.106)
Size			0.301***	0.302***			0.222**	0.215*
			(0.102)	(0.096)			(0.111)	(0.116)
Age			0.316***	0.303***			0.162	0.158
			(0.084)	(0.076)			(0.104)	(0.098)
Stock Return			-0.016*	-0.014			-0.013	-0.008
			(0.009)	(0.009)			(0.012)	(0.012)
ROA			-0.016	-0.015			-0.013	-0.013
			(0.019)	(0.018)			(0.023)	(0.021)
Market-to-Book Ratio			0.019	0.021			0.030*	0.034**
			(0.017)	(0.015)			(0.016)	(0.017)
Leverage			-0.013	-0.006			-0.008	-0.006
			(0.022)	(0.023)			(0.022)	(0.022)
Investment Grade			0.022	0.019			0.086	0.055
			(0.041)	(0.044)			(0.058)	(0.066)
Speculative Grade			0.106**	0.094*			0.116**	0.085
			(0.052)	(0.053)			(0.059)	(0.064)
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	No	Yes	No	Yes	No	Yes	No
Industry-Year-Cohort FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	75,204	75,204	75,204	75,204	55,273	55,273	55,273	55,273
Pseudo R-squared	0.432	0.437	0.434	0.439	0.431	0.436	0.432	0.437

Table 5: CEO turnover and discrimination lawsuits. This table reports the effect of CEO turnover on the number of discrimination lawsuits filed against a firm. To cleanly estimate treatment effects, we construct cohorts of treated and control firms for two years pre/post each CEO turnover event, requiring control firms to not have been previously treated by another event, and pool the data across cohorts. Columns (1)-(4) include treatment events where the newly appointed CEO is female and Columns (5)-(8) include treatment events where the newly appointed CEO is male. 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.

Aftermath:	**Number of Discrimination Lawsuits**							
	Female CEO Appointment				**Male CEO Appointment**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CEO turnover	-0.250***	-0.267***	-0.200**	-0.216**	-0.032	-0.040*	0.001	-0.003
	(0.092)	(0.098)	(0.096)	(0.100)	(0.024)	(0.023)	(0.023)	(0.022)
Number of Employees			0.396***	0.410***			0.352***	0.350***
			(0.089)	(0.086)			(0.086)	(0.084)
Size			0.200*	0.221**			0.234**	0.284***
			(0.119)	(0.097)			(0.118)	(0.100)
Age			0.118	0.101			0.107	0.084
			(0.078)	(0.083)			(0.084)	(0.089)
Stock Return			-0.017*	-0.016*			-0.019*	-0.018*
			(0.009)	(0.009)			(0.010)	(0.010)
ROA			0.008	0.012			0.007	0.017
			(0.021)	(0.019)			(0.022)	(0.020)
Market-to-Book Ratio			0.017	0.015			0.005	0.002
			(0.022)	(0.021)			(0.023)	(0.022)
Leverage			-0.004	-0.005			-0.012	-0.015
			(0.021)	(0.021)			(0.022)	(0.022)
Investment Grade			-0.009	-0.007			0.006	0.013
			(0.047)	(0.046)			(0.053)	(0.050)
Speculative Grade			0.053	0.055			0.061	0.059
			(0.048)	(0.048)			(0.052)	(0.053)
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	No	Yes	No	Yes	No	Yes	No
Industry-Year-Cohort FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	80,589	80,589	80,589	80,589	69,701	69,701	69,701	69,701
Pseudo R-squared	0.505	0.510	0.507	0.512	0.500	0.505	0.501	0.507

Table 6: Heterogeneity with respect to geographic concentration. This table displays cross-sectional variation in the estimated effect of location-based cultural proxies, depending on the firm's geographic concentration. Regression specifications are the same as those reported in Tables 3 and 4, respectively, except that we interact the independent variable of interest with an indicator that equals one if the firm has above median geographic concentration (i.e., subsidiaries in three or fewer states). The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Appendix 1 lists variable definitions.

Panel A: Tolerance of racism and sexism

	Number of Discrimination Lawsuits			
	(1)	(2)	(3)	(4)
HQ Most Racist State	0.155	-0.035		
	(0.139)	(0.130)		
HQ Most Racist State*Concentrated Operations		0.463**		
		(0.186)		
HQ Least Sexist State			-0.229**	-0.143
			(0.115)	(0.132)
HQ Least Sexist State*Concentrated Operations				-0.255**
				(0.112)
Concentrated Operations		-0.119**		-0.076
		(0.047)		(0.052)
Firm Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	47,370	47,370	47,370	47,370
Pseudo R-squared	0.419	0.420	0.419	0.420

Panel B: Tolerance of misconduct

Aftermath:	**Number of Discrimination Lawsuits**			
	Politician Does Not Resign		**Politician Resigns**	
	(1)	(2)	(3)	(4)
Alleged Misconduct	0.055**	0.058*	-0.101**	-0.048
	(0.023)	(0.030)	(0.043)	(0.052)
Alleged Misconduct*Concentrated Operations		0.011		-0.129***
		(0.063)		(0.044)
Firm Controls	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	43,397	43,397	39,971	39,971
Pseudo R-squared	0.449	0.454	0.439	0.443

Table 7: Discrimination lawsuit outcomes. Panel A summarizes outcomes for a sample of 36,244 employment discrimination lawsuits against 2,917 U.S. public companies with data available in Compustat and CRSP between 1992 and 2018. Panels B and C report Poisson regression estimates of the effect of a cultural tolerance shock on the rate at which discrimination lawsuits are dismissed and settled, respectively. The dismissal rate is the fraction of suits filed during the firm-year that are ultimately dismissed and the settlement rate is the fraction of non-dismissed cases that are ultimately settled. The regression specifications are the same as those reported in Tables 4 and 5, except that we use the subsample of observations with at least one discrimination lawsuit filed against the firm in Panel B and at least one non-dismissed suit against the firm in Panel C. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Appendix 1 lists variable definitions.

Panel A: Frequency of outcomes

	Count	**Percent**
Dismissed	11,638	32.11
Settled	15,683	43.27
Judgment	8,923	24.62
Total	**36,244**	**100.00**

Panel B: Dismissal rate

Aftermath:	**Dismissal Rate**			
	Politician Does Not Resign	**Politician Resigns**	**Female CEO Appt.**	**Male CEO Appt.**
	(1)	(2)	(3)	(4)
Alleged Misconduct	-0.015	-0.099		
	(0.038)	(0.077)		
CEO Turnover			0.171	-0.055
			(0.211)	(0.047)
Firm Controls	Yes	Yes	Yes	Yes
Year FE	No	No	No	No
Firm-Cohort FE	Yes	Yes	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	23,356	17,021	30,704	26,479
Pseudo R-squared	0.084	0.081	0.076	0.080

Panel C: Settlement rate

Aftermath:	**Settlement Rate**			
	Politician Does Not Resign	**Politician Resigns**	**Female CEO Appt.**	**Male CEO Appt.**
	(1)	(2)	(3)	(4)
Alleged Misconduct	0.012	-0.000		
	(0.021)	(0.034)		
CEO Turnover			0.024	0.027
			(0.108)	(0.024)
Firm Controls	Yes	Yes	Yes	Yes
Year FE	No	No	No	No
Firm-Cohort FE	Yes	Yes	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	21,189	15,267	27,961	23,993
Pseudo R-squared	0.053	0.049	0.050	0.051

Table 8: Economic conditions and discrimination lawsuits. Panel A reports the effect of increased import competition on the number of discrimination lawsuits filed against a firm. China Trade Shock Exposure measures the potential tariff hike the firm's industry faced before China obtained permanent MFN trade status in 2000. Post-2000 is an indicator that equals one from 2000 onwards. Panel B reports the effect of a cash windfall from the American Jobs Creation Act on the number of discrimination lawsuits filed against a firm. 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-2004 is an indicator that equals one from 2004 onwards. Heteroskedasticity-consistent standard errors are clustered by industry in Panel A and firm in Panel B. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Appendix 1 lists variable definitions.

Panel A: Increased import competition

	ROA		Number of Discrimination Lawsuits		
	(1)	(2)	(3)	(4)	(5)
Post-2000*China Trade Shock Exposure	-0.022**	-0.018**	0.076	0.070	0.059
	(0.010)	(0.009)	(0.089)	(0.093)	(0.082)
Post-2000*High Tech Industry		-0.047***		0.069	-0.211
		(0.018)		(0.205)	(0.129)
Post-2000*Unskilled Labor Percentage		-0.036**		0.131	-0.049
		(0.016)		(0.135)	(0.098)
Firm Controls	No	No	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	8,394	8,394	8,394	8,394	8,394
Pseudo R-squared	0.652	0.656	0.526	0.526	0.537

Panel B: Relaxed financial constraints

	Shareholder Payouts		Number of Discrimination Lawsuits		
	(1)	(2)	(3)	(4)	(5)
Post-2004*AJCA Exposure*Leverage		0.223**		-0.024	-0.035
		(0.090)		(0.094)	(0.098)
Post-2004*AJCA Exposure	0.182**	-0.160	-0.027	-0.005	0.003
	(0.088)	(0.117)	(0.067)	(0.113)	(0.114)
Post-2004*Leverage		-0.143		-0.064	-0.035
		(0.129)		(0.127)	(0.131)
AJCA Exposure*Leverage		-0.264		-0.051	0.003
		(0.238)		(0.206)	(0.218)
Leverage		-0.264		-0.172	-0.171
		(0.170)		(0.129)	(0.129)
Firm Controls	No	No	No	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1,158	1,158	1,158	1,158	1,158
Pseudo R-squared	0.959	0.960	0.695	0.696	0.697

Table 9: Shareholder value implications of discrimination lawsuits. This table reports percentage and dollar cumulative abnormal returns (CARs) around employment discrimination lawsuit filings. Panel A reports mean and median CARs for the full sample of 36,244 employment discrimination lawsuits against 2,917 U.S. public companies with data available in Compustat and CRSP between 1992 and 2018. Panel B presents CARs for subsamples split by lawsuit characteristics. We test differences in means using *t*-tests and differences in medians using quantile regressions. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Appendix 1 lists variable definitions.

Panel A: Full sample

	5-Day CARs (%)		**5-day Dollar CARs ($M)**	
	Market Adjusted	Fama-French 3 Factor	Market Adjusted	Fama-French 3 Factor
Mean CAR	-0.041*	-0.059**	-25.710***	-23.305***
Median CAR	-0.084***	-0.098***	-0.838***	-1.104***
Observations	36,244	36,244	36,244	36,244

Panel B: Sample splits

	5-Day Market Adjusted CARs (%)		
	Mean	Median	Observations
By lawsuit outcome:			
(1) Dismissed	-0.060	-0.096***	11,638
(2) Settled	-0.044	-0.083***	15,683
(3) Judgment	-0.010	-0.068*	8,923
Diff. between (1) and (2)	-0.016	-0.013	27,321
Diff. between (1) and (3)	-0.050	-0.028	20,561
Diff. between (2) and (3)	-0.034	-0.015	24,606
By judgment:			
(1) In Favor of Plaintiff	0.080	-0.038	576
(2) In Favor of Defendant	-0.042	-0.068	6,429
(3) Unknown/Missing	0.072	-0.073	1,918
Diff. between (1) and (2)	0.122	0.031	7,005
Diff. between (1) and (3)	0.008	0.035	2,494
Diff. between (2) and (3)	-0.114	0.004	8,347
By plaintiff:			
(1) Individual	-0.038	-0.081***	35,728
(2) EEOC	-0.255	-0.388*	516
Diff. between (1) and (2)	-0.218	-0.307**	36,244
By defendant history:			
(1) First lawsuit	-0.177*	-0.368***	2,917
(2) Second to tenth suit	-0.131**	-0.147***	9,222
(3) Eleventh lawsuit or later	0.010	-0.040*	24,105
Diff. between (1) and (2)	-0.046	-0.221**	12,139
Diff. between (1) and (3)	-0.188**	-0.328***	27,022
Diff. between (2) and (3)	-0.141***	-0.107**	33,327

Appendix 1: Variable definitions. This table lists variable definitions and data sources. CCM denotes the CRSP-Compustat Merged Database. CGP refers to Charles, Guryam, and Pan (2022). EXEC refers to the ExecuComp Annual Compensation Database. FJC denotes the Federal Judicial Center Civil Integrated Database. Gov refers to the GovTrack.us Legislator Misconduct Database. JR denotes John Romalis' website. KLD denotes the MSCI ESG KLD STATS Database. LLR refers to Levine, Levkov, and Rubinstein (2008). NBER refers to the NBER-CES Manufacturing Industry Database. WRDS denotes the WRDS Company Subsidiary Dataset.

Variable	Source	Description
Age	CCM	Log number of years since firm was first listed in Compustat
AJCA Exposure	CCM	Indicator that equals one if the firm's cumulative reported foreign profits from 2001 to 2003 are positive, and zero otherwise
Alleged Misconduct - No Resignation	Gov	Indicator that equals one for firms headquartered in a state where a U.S. congressperson was accused of personal misconduct but did not resign, and zero otherwise. Misconduct events include sexual harassment, abuse, bribery, fraud, and other ethics violations
Alleged Misconduct - Resignation	Gov	Indicator that equals one for firms headquartered in a state where a U.S. congressperson resigned after being accused of personal misconduct, and zero otherwise. Misconduct events include sexual harassment, abuse, bribery, fraud, and other ethics violations
China Trade Shock Exposure	JR	Potential tariff hike the firm's 6-digit NAICS 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 in 1999
Concentrated Operations	WRDS	Indicator equal to one if the firm has subsidiaries in three or fewer states, and zero otherwise. The three-state cutoff is based on the sample median
CSR Score	KLD	Total net CSR score, following Lins, Servaes, and Tamayo (2017). Specifically, we compute the net CSR index for five categories (community, diversity, employee relations, environment, and human rights) by dividing the number of reported strengths by the maximum number of possible strengths in that category and subtracting the analogous value for weaknesses, and then sum the net CSR indices across categories
Female CEO Appointment	EXEC	Indicator equal to one if a female was appointed CEO, and zero otherwise
High Tech Industry	CCM	Indicator that equals one if the firm's industry is classified in Computers and Electronics Manufacturing Subsector (NAICS 334), and zero otherwise
HQ Black Population	Census	County-level percentage of population that is Black
HQ Income Per Capita	Census	County-level log income per capita
HQ Least Sexist State	CGP	Indicator that equals one if the firm's headquarter state is in the lowest two sexism ranking categories based on the General Social Survey, and zero otherwise
HQ Racial Bias Index	LLR	Difference between predicted and actual interracial marriage rates during 1970 in the firm's headquarter state
HQ Rural Area	NCHS	Indicator that equals one if the firm's headquarter county is not designated as a Large Central Metro, Large Fringe Metro, or Medium Metro by the 2006 National Center for Health Statistics (NCHS) Urban Rural Classification Scheme, and zero otherwise
HQ Unemployment Rate	Census	County-level unemployment rate
Industry Price-Cost Margin	CCM	Sales minus cost of goods sold plus change in inventories, divided by sales plus change in inventories
Investment Grade	CCM	Indicator that equals one if firm has an S&P long term issuer credit rating of BBB- or higher, and zero otherwise
Leverage	CCM	Long-term debt plus debt in current liabilities divided by total assets
Male CEO Appointment	EXEC	Indicator equal to one if a male was appointed CEO, and zero otherwise
Market-to-Book Ratio	CCM	Ratio of market value of assets to book value of assets
Number of Discrimination Lawsuits	FJC	The number of employment discrimination lawsuits filed in federal court against the firm during the year (Nature of Suit 442-Civil Rights Job)
Number of Employees	CCM	Log number of employees (in thousands)

Appendix 1: Variable definitions. (Continued)

Variable	Source	Description
ROA	CCM	Operating income before depreciation divided by total assets
Shareholder Payouts	CCM	Cash dividends plus purchase of common and preferred stock
Size	CCM	Log total assets
Speculative Grade	CCM	Indicator that equals one if firm has an S&P long term issuer credit rating of BB+ or lower, and zero otherwise
Stock Return	CCM	Buy-and-hold abnormal return (BHAR) over the fiscal year using the CRSP value-weighted index as market proxy
Unskilled Labor Percentage	NBER	The fraction of employees in the firm's industry in 1999 that were production workers
5-Day CARs	CCM	Market model (or Fama-French 3 Factor) percentage cumulative abnormal return (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 5-day [-2, +2] event window

Appendix 2: Sample description. The full sample consists of 95,045 firm-year observations from 10,237 U.S. public companies with data available in Compustat between 1992 and 2018. The number of observations varies across subsamples based on data availability and empirical methodology as described in Section 2. Continuous variables are winsorized at the 1/99% tails throughout the analysis. Appendix 1 lists variable definitions.

	Mean	S.D.	P25	Median	P75	Obs
Number of Discrimination Lawsuits	0.313	1.115	0.000	0.000	0.000	95,045
Number of Employees	-0.129	2.088	-1.661	-0.200	1.352	95,045
Size	5.724	2.196	4.094	5.651	7.198	95,045
Age	2.555	0.838	1.946	2.565	3.178	95,045
Stock Return	0.016	0.610	-0.341	-0.070	0.215	95,045
ROA	0.051	0.200	0.019	0.091	0.154	95,045
Market-to-Book Ratio	1.978	1.647	1.042	1.376	2.201	95,045
Leverage	0.194	0.186	0.021	0.150	0.316	95,045
Investment Grade	0.119	0.324	0.000	0.000	0.000	95,045
Speculative Grade	0.100	0.300	0.000	0.000	0.000	95,045
Industry Price-Cost Margin	0.420	0.184	0.280	0.406	0.538	95,045
HQ Income Per Capita	10.612	0.410	10.308	10.586	10.863	95,045
HQ Unemployment Rate	0.299	0.240	0.251	0.348	0.424	95,045
HQ Black Population	0.138	0.116	0.048	0.106	0.200	95,045
HQ Rural Area	0.076	0.264	0.000	0.000	0.000	95,045
HQ Racial Bias Index	0.253	0.082	0.150	0.280	0.320	95,045
HQ Least Sexist State	0.106	0.307	0.000	0.000	0.000	95,045
CSR Score	-0.129	0.412	-0.333	-0.105	0.110	25,285
Alleged Misconduct - No Resignation	0.092	0.289	0.000	0.000	0.000	75,204
Alleged Misconduct - Resignation	0.044	0.206	0.000	0.000	0.000	55,273
Female CEO Appointment	0.002	0.049	0.000	0.000	0.000	80,589
Male CEO Appointment	0.067	0.250	0.000	0.000	0.000	69,701
China Trade Shock Exposure	0.313	0.121	0.266	0.327	0.361	8,394
High Tech Industry	0.235	0.424	0.000	0.000	0.000	8,394
Unskilled Labor Percentage	0.635	0.151	0.512	0.642	0.779	8,394
AJCA Exposure	0.440	0.497	0.000	0.000	1.000	1,158
Shareholder Payouts	295.511	1,097.045	3.361	35.780	196.000	1,158