A culture of discrimination: Evidence on the causes and consequences of civil rights litigation against U.S. corporations

Casey Dougal, Thomas P. Griffin, Irena Hutton[†]

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Abstract

Between 1992 and 2018, U.S. public corporations faced over 38,000 federal civil rights lawsuits. We find that the frequency of discrimination litigation varies with societal attitudes toward race and gender and intensifies after a rightward shift in local news slant. However, studying two well-established natural experiments, we find no evidence that economic forces provide a source of discipline. Consequences are small: shareholder value drops by $8.25 million, on average, around a lawsuit, with no effect on CEO turnover. We conclude that corporate discrimination is largely determined by the cultural values of employees, rather than a firm's economic environment.

Keywords: discrimination, culture, competition, shareholder value, CEO turnover
JEL Classification: G30, G32, J71, K31, M14

[†]Casey Dougal: Florida State University, phone: (850) 644-2038, email: cdougal@fsu.edu. Thomas Griffin: Villanova University, phone: (610) 519-5460, email: thomas.griffin@villanova.edu. Irena Hutton: Florida State University, phone: (850) 645-1520, email: ihutton@business.fsu.edu. We thank Bernard Black, Don Bowen, Lily Li, Michelle Lowry, Hosein Maleki, Lalitha Naveen, Greg Nini, Chris Parsons, and participants at the Philly Five Conference (2021).

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). Nevertheless, we find that U.S. corporations are accused of employment discrimination at an alarming pace: 38,055 federal civil rights lawsuits were filed against nearly 3,000 U.S. public firms between 1992 and 2018. In this paper, we study novel civil rights litigation data from the Federal Judicial Center to document the incidence of corporate discrimination, uncover its determinants, and assess whether economic forces provide a source of discipline.

Economic theory proposes two potential motives for employment discrimination: taste-based and information-based. In taste-based models espoused by Milton Friedman, employers engage in discriminatory behavior because they hold a personal prejudice devoid of economic merit (Becker 1957). In contrast, information-based (i.e., "statistical") models posit discrimination as an optimal solution to a signal extraction problem (Phelps 1972; Arrow 1973; Aigner and Cain 1977). If employers have limited information about an applicant's skill, statistical models assert that employers should partially base decisions on observable group characteristics that they believe are correlated with productivity. Empirical tests of these theories typically measure discrimination using the observed difference in wages/hiring between groups. This approach is challenging, however, because it is nearly impossible to control for all correlated factors needed for a ceteris paribus comparison. And, even if wage/hiring gaps are cleanly observed, it is difficult to attribute

motive without also observing how employers form expectations. As a result, the source and extent of workplace discrimination continues to be hotly debated among academics and the media.

Civil rights litigation presents a unique setting to study corporate discrimination for at least two reasons. First, litigation case files offer an avenue to identify 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] This distinction is important because, while statistical models argue that wage/hiring discrimination can be economically efficient if group characteristics correlate with productivity, there is no economic justification for harassment and retaliation.

We find that most discrimination suits allege behavior with no economic basis. 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 corporate discrimination is often motivated by animus rather than economic rationale.

A second advantage of studying civil rights litigation is that lawsuit filings provide a holistic measure of disparate treatment. This is important because individuals may face unequal working conditions even when wages/titles are the same. We find that 29.7% of U.S public corporations face at least one employment discrimination lawsuit in federal court between 1992 and 2018. The average (median) firm faces 0.32 (0) cases per year. These dynamics yield a highly

[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.

skewed distribution, with the top 100 firms defending 49% of all discrimination cases despite contributing to only 19% of total market capitalization and 15% of total employment.[2] We also find significant variation in litigation rates across industry and geography. For example, 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.

These patterns can best be explained by differences in societal and corporate culture. Using OLS regressions that relate discrimination suits to lagged firm, industry, and headquarter-area characteristics, we find a robust association between litigation rates and attitudes toward race and gender. The likelihood of facing a discrimination suit is 8.2% higher for firms headquartered in a state with one standard deviation above average racial bias, relative to the sample mean, and 14.2% lower for firms headquartered in one of the "least sexist" states.[3] Moreover, the presence of a female CEO is negatively correlated with discrimination suit frequency, consistent with Tate and Yang's (2015) finding that female leadership moderates the gender wage gap for lower-level employees at their firm. Finally, we find some evidence that discrimination suits are more likely to be filed against firms with poor corporate social responsibility and governance.

Building on research that defines culture as a set of values that inform behavior, we conjecture that the mechanism through which culture influences discrimination is its ability to regulate social interactions.[4] Indeed, surveys suggest that tolerance of workplace discrimination

[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 (2022), we define a state as "least sexist" if it is in the lowest two sexism ranking categories based on the General Social Survey.

[4] O'Reilly and Chatman (1996) define culture as a social control system based on shared norms and values. Guiso, Sapienza, and Zingales (2015) argue that this definition most closely captures the prevailing concept of culture used in neoclassical economic models (e.g., Guiso, Sapienza, and Zingales 2008, 2011; Tabellini 2008).

varies considerably with political ideology. Republicans are half as likely as Democrats to say that "women not being believed is a major problem when it comes to sexual harassment in the workplace" (Pew Research Center 2018), roughly 20% less likely to say that "it should be illegal to fire employees based on LGBT status" (Washington Post 2020), and over 50% more likely to admit to committing some "harassing behaviors" at work (New York Times 2017). In line with these surveys, our OLS regressions reveal a 7.8% higher discrimination lawsuit incidence at firms headquartered in a county with one standard deviation above average Republican vote share. This finding suggests that at least one element of culture, political ideology, may influence workplace discrimination rates through the tolerance of such incidents.

To evaluate whether this mechanism plays a causal role, we study Sinclair Broadcast Group's expansion across local TV markets as a natural experiment. Martin and McCrain (2019) show that Sinclair imposes a conservative slant on acquired local news stations while retaining original branding and affiliation (e.g., NBC 24 Toledo). In doing so, Sinclair exposes viewers to segments that are designed to look like news but involve conservative commentary, for example criticizing an Obama policy that protects school bathroom choice for transgender students.[5] Kaviani, Li, and Maleki (2021) show that Sinclair's entry into a market does not appear to be driven by economic or demographic factors, beyond population size, and leads to a rightward shift in election outcomes and political contributions.

Using a staggered difference-in-differences research design, we find that Sinclair's entrance leads to a statistically and economically significant increase in civil rights litigation against firms headquartered nearby. This finding is robust across a set of analyses recommended

[5] See https://www.newyorker.com/magazine/2018/10/22/the-growth-of-sinclairs-conservative-media-empire

by Baker, Larker, and Wang (2022) for causal inference with staggered treatment timing and treatment effect heterogeneity. Sinclair's entrance leads to a 9.7% higher probability of a discrimination lawsuit in our baseline two-way fixed effects regression, prompts an 11.2% increase in our stacked regression, and exhibits dynamic treatment effects consistent with the parallel-trends assumption in our event-study plot using Sun and Abraham's (2021) interaction weighted estimator. Together, these results support taste-based theories that model discrimination as a function of prejudice and highlight the important role of culture in determining this parameter.

We find little evidence that economic forces shape civil rights litigation rates. In contrast to statistical models that argue the choice to discriminate is profit-maximizing, we find a negative association between litigation and firm performance in the cross-section. Although this negative correlation is consistent with taste-based models, workplace discrimination does not appear to be optimized or competed away based on economic conditions. 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 litigation is related to firm-level economic conditions.

To better understand why discrimination persists regardless of economic environment, we examine the effect of discrimination lawsuits on firm value and CEO turnover. Consequences of discrimination litigation appear small: the mean (median) three-day cumulative abnormal return (CAR) around a suit filing is -0.03% (-0.10%), which corresponds to a roughly $8.25 million loss in shareholder value for the average corporate defendant.[6] Moreover, we find no evidence that

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

CEOs are held accountable for discrimination lawsuits at their firm. Finally, we note that maximum legal penalties are low relative to the financial resources of a typical public company, with a cap of $300,000 per plaintiff. While we acknowledge that litigation filings are an imperfect proxy of underlying discrimination because some discriminatory behavior may not trigger a lawsuit filing and some lawsuit filings may be frivolous, these findings cast doubt on Milton Friedman's claim that a "market solution" will eventually temper corporate discrimination.

Our paper contributes to the broad economics literature on workplace discrimination by studying civil rights litigation as a novel measure of differential treatment. Prior studies focus on differences in employment and wages (Ashenfelter and Hannan 1986; Black and Strahan 2001; Bertrand and Mullainathan 2004; Kline, Rose, Walters 2021; Sherman and Tookes 2021). However, it is possible that individuals face different working conditions even if titles and wages are the same. We add to this literature by showing that workplace discrimination often involves harassment, an action that has no economic merit and cannot be justified by statistical models.

This paper also contributes to the growing literature on corporate social responsibility. Building on studies that show firms can "do well by doing good" (Edmans 2011; Servaes and Tamayo 2013, Deng, Kang, and Low 2013; Chava 2014; Krüger 2015, Flammer 2015, 2021; Lins, Servaes, and Tamayo 2017; Albuquerque, Koskinen, and Zhang 2019), we find that discrimination lawsuit filings are met with negative stock price reactions.[7] However, we find no evidence that litigation varies with product market competition or financial resources, nor do we find evidence that CEOs are held responsible for suits at their firm. Instead, our results suggest discriminatory

[7] This literature suggests that CSR activities can improve reputation, consumer demand, and labor productivity. Hacamo (2022) finds that store foot-traffic falls by 3 to 4 percentage points after an incident of workplace racial prejudice is reported on a popular job-search website. Barnes (2022) finds that employee morale declines after major workplace discrimination cases are made public.

behavior is ingrained in employee culture rather than optimized based on economic conditions.

2. Institutional background and data

2.1 Employment discrimination laws

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.[8] 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.[9]

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 Commission (EEOC), or an equivalent state or local fair employment practices agency (FEPA), within 180 days of the alleged incident.[10] The agency investigates the allegations and attempts to resolve violations. If the agency fails to establish probable cause or 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

[8] 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.

[9] 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.

[10] 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.

rare circumstances, it files suit on behalf of the charging party.[11] Since the cause of action arises under federal law, employment discrimination is considered to be a federal question and therefore under the subject-matter jurisdiction of federal courts (28 U.S.C., § 1331).

2.2 The nature of alleged discrimination

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 38,055 employment discrimination lawsuits filed against 2,952 U.S. public companies from 1992 through 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.

To better understand the nature of these cases, we begin by performing textual analysis on complaint filings. We applied for a fee waiver to collect data from all 94 Federal district courts and received a waiver from 64 of these districts.[12] Out of 64 districts granting fee waivers, 54 had complaint filings available for download from the Public Access to Court Electronic Records (PACER) service. Of these complaints, 97% were filed in the post-2000 period, reflecting advances in record-keeping technology. From the 54 districts with available data, we obtain the

[11] Discrimination complaints brought under the Age Discrimination in Employment Act and Equal Pay Act (EPA) do not need 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.

[12] Absent a fee waiver, this data would have cost roughly $300,000. Fee waivers are granted at the discretion of district courts. The Internet Appendix provides a list of waiver granting and non-waiver granting districts.

complaint filing for 81% of filings in our main FJC-Compustat dataset during the post-2000 period, which is equivalent to 14,310 cases.

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.[13] 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 further 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. There exists substantial variation in complaint size across LDA topics, ranging from 981 words, on average, for Topic 1 to 3,114 words for Topic 8. Harassment-based words are most frequent in Topic 5 complaints,

[13] 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.

averaging 5.35% of total words, but are often present even when a hostile work environment is not the focal topic.[14] 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 discrimination is often driven by taste-based, rather that information-based, motives.

2.3 Distribution of discrimination lawsuits

Table 2 presents descriptive statistics for our FJC-Compustat merged sample, which consists of 95,015 firm-year observations from 10,229 U.S. public companies with data available in the CRSP-Compustat Merged Database 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 widespread; 29.7% of firms and 13.4% of firm-years face at least one discrimination lawsuit. The Healthcare and Finance industries contain the lowest fraction of firms that face at least one lawsuit, with 18.5% and 20.0% respectively, while the Wholesale & Retail and Utilities industries have the highest incidence, with roughly 43% of firms in each industry facing at least one suit.

Figure 2 shows that the industry-level ranking of litigation rates remains relatively constant over time. Each year, a little under 15% of Manufacturing firms face at least one lawsuit, while only about 7% of Business Equipment (including Tech) firms face a suit. Figure 3 plots the fraction of firm-year observations facing an employment discrimination lawsuit by headquarter state.[15] The plot reveals evidence of a strong geographic component in discrimination lawsuit frequency, with firms headquartered in the South and Rustbelt exhibiting significantly higher discrimination rates

[14] The Internet Appendix lists our harassment-based word dictionary.
[15] We assign headquarter location using zip codes listed in 10-Ks, supplemented with data in Compact Disclosure.

than firms in the Northeast and West.

Table 2 documents a strong correlation between discrimination lawsuits and firm size and credit rating. Given that size and credit rating are two of the most reliable proxies of financial constraints (Hadlock and Pierce 2010), these correlations provide preliminary support for taste-based models that predict a positive relation between discrimination and financial slack. Of course, these correlations are likely confounded by omitted variables. We therefore caution against causal interpretations of these relations absent further investigation. Finally, we report variable definitions and summary statistics in Appendix 1 and 2, respectively.

3. Which firms face discrimination lawsuits?

In this section, we present results from OLS regressions that relate discrimination lawsuits to lagged firm, industry, and headquarter-area characteristics. Our focus is on cultural factors that may shape employers' "taste" for discrimination as well as economic factors that may influence the ability to act on this preference. The dependent variable in Columns (1)-(3) of Table 3, Discrimination Lawsuit, is an indicator that equals one if the firm faces at least one discrimination lawsuit during the year, capturing the extensive margin. The dependent variable in Columns (4)-(6), Discrimination Lawsuit Frequency, is the log of one plus the number of discrimination suits in the firm-year, capturing the intensive margin. Columns (1) and (4) examine the role of societal culture. Since these proxies are based on headquarter location and are generally time invariant, we use pooled regressions that primarily exploit across-firm variation. Columns (2) and (4) include firm fixed effects to examine within-firm changes in economic conditions. Finally, Columns (3) and (6) add time-varying proxies of firm culture as additional regressors in the fixed effects regressions. The number of observations falls dramatically in these columns because the intersection of firm-level cultural proxies is only available for a select set of large firms.

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.[16] Becker notes that "although these coefficients are the proximate determinant of choices, they are in turn, like other tastes, influenced by more fundamental variables." (pg. 154). We conjecture that culture is a fundamental determinant of this prejudice parameter. Assessing the role of culture is challenging, however, because it is a slow-moving and multidimensional construct with no consensus definition or measurement. Therefore, we follow Guiso, Sapienza, and Zingales' (2015) broad definition of culture as a set of values that inform behavior. This definition is consistent with a large body of finance research that shows individual and firm behavior reflects local cultural values (e.g., Hilary and Hui 2009; Kumar, Page, and Spalt 2011; Hoi, Wu, and Zhang 2019; Hayes, Jiang, and Pan 2021).

Results in Table 3 provide initial support for this view; discrimination lawsuits are significantly more likely to be filed against firms headquartered in states with high rates of racial bias and reported sexism. Specifically, we measure racial bias as the difference between predicted and actual interracial marriage rates during 1970 (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 (Charles, Guryan, and Pan 2018; Giannetti and Wang 2022). Column (1) shows that the likelihood of facing a discrimination suit is 8.2% higher for firms headquartered in a state with one standard deviation above average racial bias, relative to the sample mean, and 14.2% lower for firms headquartered in one of the "least sexist" states, controlling for other demographic

[16] While Becker (1957) describes discrimination as an aversion to interacting with the minority group, follow-on work describes discrimination as malice toward the minority group (Alexis 1973) or nepotism toward the majority group (Goldberg 1982) and arrives at broadly similar conclusions.

and firm characteristics.[17] Moreover, Column (6) shows that firms face discrimination suits less frequently under the leadership of a female CEO, consistent with Adhikari, Agrawal, and Malm (2019) and Liu's (2018, 2021) finding that firms with female leadership face fewer operations-related lawsuits. Finally, we find some evidence that suits are more likely to be filed against firms with greater KLD corporate social responsibility concerns and managerial entrenchment, using the E-index of Bebchuk, Cohen, and Ferrell (2009). Together, our 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 greatest impact on allowing harassment to flourish, or conversely, in preventing harassment."[18]

Turning to economic determinants, we find no evidence supporting statistical models that predict discrimination lawsuits should be positively correlated with profitability. Instead, the negative and significant coefficients on lagged stock return and return on assets suggest that discrimination suits are more common at poorly performing firms. We also find that litigation is positively related to firm size and the presence of a credit rating. These estimates suggest that larger, unconstrained firms face more discrimination suits, even when controlling for the number of employees. Finally, we find a positive relation between industry concentration (measured with the Herfindahl-Hirschman Index) and discrimination litigation, consistent with Becker's (1957) conjecture that employer discrimination should, on average, be less in competitive industries than in monopolistic industries. Together, these results provide initial support for taste-based models and paint a picture of the quintessential discriminating firm: a large, poorly performing corporation that is headquartered in an area where residents hold more racist and sexist views.

[17] Using summary statistics in Appendix 2, ((0.132*0.083)/0.134) = 8.176% and (-0.019/0.134) = -14.179%.
[18] See: https://www.eeoc.gov/select-task-force-study-harassment-workplace-report-co-chairs-chai-r-feldblum-victoria-lipnic

4. Assessing the causal role of culture and economic conditions

In the previous section, we show that civil rights litigation rates vary across geographic area based on cultural values and across industry based on concentration. It is possible, however, that these correlations are spurious or biased by omitted variables. For example, since industries tend to cluster in certain regions (Dougal, Parsons, and Titman 2015), our geographic results may be driven by an industry effect. And, considering flaws in Compustat-based HHI (Ali, Klasa, and Yeung 2009), our concentration result may not necessarily reflect market power.

To address these concerns, we study three natural experiments that induce a plausibly exogenous change in culture and economic conditions. In Section 4.1, we examine changes in discrimination lawsuit frequency around the entrance of a conservative-leaning local TV news provider, Sinclair Broadcast Inc. In Section 4.2, we investigate the effect of an increase in Chinese import competition on discrimination lawsuits against U.S. manufacturers. Finally, in Section 4.3, we study changes in discrimination lawsuit frequency around the American Jobs Creation Act, which created a cash windfall for U.S. firms with profitable foreign subsidiaries.

4.1 Effect of Sinclair's expansion into local TV markets

Since culture is a multidimensional construct, we focus on political ideology as one important element that may play a causal role in shaping workplace discrimination. We choose to study local political ideology because it i) can be easily measured through voting records, ii) captures relevant attitudes that influence discrimination, and iii) can shift rapidly due to exogenous factors. Our cross-sectional results in Table 3 suggest that political ideology is relevant; firms headquartered in a county with one standard deviation above average Republican vote share are 7.8% more likely to face a discrimination lawsuit.[19] This correlation is consistent

[19] Using summary statistics in Appendix 2, ((0.074*0.142)/0.134) = 7.842%.

with Acquisti and Fong's (2019) correspondence study showing that job callback rates are significantly lower for Muslim candidates in Republican areas and van der Linden and Panagopoulos' (2019) survey evidence documenting that conservatives are less concerned about sexual harassment in the workplace than liberals. DellaVigna and Kaplan (2007) show that local political ideology can shift swiftly due to exogenous changes in media exposure.

To assess whether culture is a causal determinant of corporate discrimination lawsuits, we study the staggered expansion of conservative media conglomerate, Sinclair Broadcast Inc. Sinclair was founded in 1971 by Julian Sinclair Smith as a single independent TV station in Baltimore, morphed into its current form in 1986 when Smith's four sons took over the business, and has rapidly expanded operations since its 1995 IPO through acquisitions and local marketing agreements. As a result, Sinclair is now the largest producer of local TV news in the United States, serving the maximum 39% share of U.S. households allowed by regulators.[20]

We rely on the following institutional features to draw causal inferences from Sinclair's expansion. First, Sinclair alters content, but not branding, upon acquiring control of a local news station. Sinclair airs the same "must-run" segments across all 193 of their local TV news stations regardless of affiliation (e.g., NBC, ABC, Fox, CBS), leaving millions of viewers unaware that they are watching conservative editorials rather than traditional news.[21] Martin and McCrain (2019) analyze a large sample of broadcast transcripts using Gentzkow and Shapiro's (2010) text-based measure of ideological slant and find that Sinclair's entrance leads to roughly a one standard deviation rightward shift in programming.

Second, Kaviani, Li, and Maleki (2021) show that Sinclair's entry into a market does not

[20] See: https://www.cbsnews.com/news/sinclair-broadcast-group-what-you-need-to-know/
[21] See: https://www.vox.com/2018/4/3/17180020/sinclair-broadcast-group-conservative-trump-david-smith-local-news-tv-affiliate

appear to be driven by economic, demographic, or ideological factors beyond population size. While Sinclair's 10-K disclosures reveal that their expansion plans were driven by a profitability motive, with the goal of entering as many TV markets as possible, the execution was shaped by regulatory constraints in an idiosyncratic manner. For example, their 2017 proposed acquisition of Tribune Media was rejected by the Federal Communication Commission for antitrust reasons.

Third, Sinclair's conservative messaging appears to have a real effect on local political ideology. Notably, Miho (2020) shows that exposure to Sinclair-operated local news increases a county's Republican vote share in Presidential elections. In addition, Kaviani, Li, and Maleki (2021) find that Sinclair's entrance leads to a rightward shift in election outcomes for county-level legislative bodies as well as a relative increase in political contributions by local firms to Republican candidates. Though focused specifically on Sinclair, these studies are consistent with a broader literature that shows media slant can influence individual and firm behavior (DellaVigna and Kaplan 2007; Baloria and Heese 2018; Knill, Liu, and McConnell 2022).

To study whether Sinclair's entrance into local TV markets affects the frequency of discrimination litigation, we follow the empirical framework of Kaviani, Li, and Maleki (2021) and estimate the two-way fixed effects (TWFE) difference-in-differences (DiD) specification:[22]

$$Y_{i,t} = \beta \cdot Sinclair\ TV\ Exposure_{i,t} + \theta \cdot Controls_{i,t} + \gamma \cdot Firm_i + \tau \cdot Industry_i * Year_t + \varepsilon_{i,t}.$$

$Y_{i,t}$ represents our outcome variables: Discrimination Lawsuit and Discrimination Lawsuit Frequency. $Sinclair\ TV\ Exposure_{i,t}$ is an indicator that equals one if Sinclair operates a local TV news station in the firm's headquarter county in that year, and zero otherwise. It is implicitly an interaction between Sinclair-county and post-expansion indicator variables, but these constituent

[22] We thank Kaviani, Li, and Maleki for providing us with data on Sinclair's local TV presence between 1996 and 2018. They collect data from Sinclair's 10-K filings and website, the FCC, and Capital IQ's Key Development dataset. The sample begins in 1996, the year after Sinclair's IPO, because it is the first full year with data available.

terms are absorbed by firm and year fixed effects, respectively. Taste-based models of discrimination predict the estimate of β to be positive and statistically significant, implying that discrimination litigation increases with the rightward slant in local TV news. Finally, $Controls_{i,t}$ are variables that account for time-varying confounding factors, $Firm_i$ and $Industry_i*Year_t$ are firm and 3-digit SIC industry-year fixed effects included in all specifications, and standard errors are clustered at the county level.

Table 4 presents results from our baseline two-way fixed effects regression. The dependent variable in Columns (1) and (2) is an indicator for the incidence of a discrimination suit. In Column (1), we estimate a statistically significant baseline effect of Sinclair exposure equal to 0.013 (standard error = 0.006). In terms of economic magnitude, this estimate implies that Sinclair's entrance leads to a 9.7% higher probability of a discrimination lawsuit.[23] Column (2) shows that our estimate of β is stable to the inclusion of controls, providing support for our assumption that Sinclair exposure is exogenously assigned. We repeat the specifications in Columns (3) and (4) using discrimination lawsuit frequency as the dependent variable. In all columns, our estimates of β are statistically significant with economic magnitudes around 10% of the mean. Overall, these results suggest that workplace discrimination intensifies after a rightward shift in local news slant.

We pay careful attention to recent developments in the econometrics literature that show DiD estimates are susceptible to biases introduced by staggered treatment timing and treatment effect heterogeneity. While we cannot perform a Goodman-Bacon (2021) decomposition diagnostic test because our panel is unbalanced, we follow steps recommended by Baker, Larker, and Wang (2022) to increase the credibility of our inferences.

[23] Using summary statistics in Appendix 2, (0.013/0.134) = 9.701%.

- First, we note that homogenous treatment effects cannot be assumed in our setting. It is plausible that treatment effects strengthen with exposure to Sinclair over time, which would bias our estimates because staggered TWFE DiD regressions implicitly use already-treated observations as a comparison group for later-treated units.
- Second, we plot the distribution of treatment timing in Appendix 3 to assess the likelihood of bias. The plots reveal substantial geographic and temporal variation in Sinclair's expansion, which could introduce downward bias if long-run causal effects for firms already treated by the late 1990s are stronger than short-run effects for later-treated firms. However, our diagnostics also reveal that 57,385 of 81,189 sample observations (70.68%) are never exposed to Sinclair TV. The high percentage of never-treated firms is reassuring because biases inherent in staggered TWFE DiD estimators become less problematic as the share of never-treated control units increases (Baker et al. 2022).
- Third, we implement a "stacked regression" alternative estimator. Following Gormley and Matsa (2011), we construct event-specific datasets that each correspond to a certain Sinclair expansion year and contain 10 years of pre- and post-event observations from firms treated during the specific expansion year as well as control firms that are never treated during the event window. We then stack each cohort into a combined dataset and estimate TWFE DiD regressions on the full sample with cohort-specific firm and industry-year fixed effects. This approach circumvents potential bias in staggered TWFE DiD estimators by avoiding the use of already-treated control units. The bottom row of Table 4 shows that the stacked regressions produce estimates similar to our baseline regardless of whether we include time-varying covariates. Across all specifications, we find that exposure to Sinclair TV leads to a statistically significant and economically meaningful increase in discrimination litigation.

- Fourth, we trace Sinclair's dynamic treatment effect with an event study plot. To do so, we implement Sun and Abraham's (2021) interaction weighted estimator using a never-treated control group.[24] Specifically, we (i) estimate our baseline regression specification augmented with relative time indicators referencing the first full year of Sinclair TV Exposure, cohort indicators, and interactions of these indicators, (ii) calculate the cohort shares underlying each relative time, and (iii) plot weighted averages of cohort-specific average treatment effects on the treated (ATTs) based on each cohort's share of sample. Following Sun (2021), we avoid binning except for distant, sparsely populated relative-time periods and report granular estimates from 16 years before to 16 years after Sinclair's expansion. The resulting estimates trace a treatment effect pattern in Figure 4 that is consistent with the parallel-trends assumption. The estimated effect exhibits no clear trend prior to Sinclair's expansion into a local TV market, inclines in the first full year after Sinclair's expansion, and trends upward with additional Sinclair exposure.

Together, these results provide causal evidence that workplace discrimination lawsuits become more frequent after a rightward shift in local news slant.

4.2 Effect of the China trade shock

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-discriminating firms will arbitrage this cost, expand, and eventually compete discriminating firms

[24] We use never-treated firms as a comparison group because the alternative option (using the last-treated cohort as a control group) is not feasible. Appendix 3 shows that Sinclair's last expansion during our sample, 2018, only treats 3 firms. Moreover, we believe that the parallel-trends assumption is likely to hold for the never-treated control group because Sinclair's expansion was shaped in an idiosyncratic manner by regulatory constraints and its entry into a market does not appear to be driven by economic or demographic factors beyond population size.

out of the market. To test this prediction, we study one of the largest increases in product market competition identified by the economics and finance literature: 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 the U.S. government chose not to renew China's trade status in a certain year, U.S. tariffs on imports from China would have jumped from MFN rates, averaging less than 5%, to Non-Market Economy (NME) rates, averaging over 35%. Consequently, this vote resolved years of tariff rate uncertainty and led to a massive increase in U.S. imports from China.

Using 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, Pierce and Schott (2016, 2018), among others, show that the China trade shock decreased profitability and increased exit rates of U.S. firms. 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. We employ Pierce and Schott's (2016, 2018) framework and estimate:

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

$Y_{i,t}$ 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 is on the interaction of these two variables, β, which captures the effect of China's receipt of permanent MFN status conditional on a firm's exposure. The dynamic interpretation of Becker (1957) predicts the estimate of β to be negative and statistically significant. Finally, $Controls_i$ are variables that account for confounding factors at the industry level, $Firm_i$ and $Year_t$ are firm and year fixed effects included in all specifications, standard errors are clustered at the industry level, and the

sample is restricted to only manufacturing firms.

Table 5 presents the results. In Column (1), we estimate β (i.e., the interaction term on Post * China Trade Shock Exposure) to equal -0.024 with a standard error of 0.045. Hence, we fail to reject the null that increased Chinese import competition has no effect on the incidence of discrimination lawsuits. In Column (2), we add a control for the post-2000 technology bubble (High Tech Industry), which is an indicator that equals one if the industry is classified in the Computers and Electronics Manufacturing Subsector, and a control for the general decline in America's unskilled-labor intensive industries (Unskilled Labor Percentage), which is the fraction of industry employees that are production workers. The addition of controls does not change our estimate of β. Our estimates of β are also statistically insignificant with magnitudes near 0 when we use Discrimination Lawsuit Frequency as the dependent variable in Columns (3)-(4). Finally, the event study plot in Figure 5, Panel A exhibits no clear break in trend.

To lessen the concern that the null effect is due to a lack of statistical power, we repeat our regression specifications in Columns (5) and (6) with ROA as the dependent variable. As in Hombert and Matray (2018), we find that the China trade shock did indeed have a statistically significant and economically large negative effect on firm profitability. Thus, our analysis provides no evidence that the China trade shock altered discrimination litigation rates despite its negative effect on firm profitability. These (non)results are consistent with research that argues discrimination can persist if there is some form of 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 is negatively related to the change in import penetration

in concentrated industries.[25] We believe that the China trade shock provides a cleaner empirical setting and more reliable inferences than prior research. However, an alternative interpretation is that our contrasting results are driven by differences in the outcome variable. Since lawsuits offer a more comprehensive measure of workplace discrimination than wage/hiring gaps alone, our null results may imply that wage/hiring decisions are easier to change in response to economic conditions than other forms of discriminatory behavior, such as harassment.

4.3 Effect of the American Jobs Creation Act tax holiday

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 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 adapt their research design with our outcome variables to determine whether financial resources affect the frequency of workplace discrimination.

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

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

of equity, number of employees, stock return, ROA, leverage, and cash-to-assets. We then estimate OLS regressions using the following DiD specification:

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

$Y_{i,t}$ 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 or θ to be positive. Finally, we include firm ($Firm_i$), industry-year ($Industry_i * Year_t$), and state-year ($State_i * Year_t$) fixed effects and cluster standard errors at the firm level.

Table 6 presents the results. Our estimates of the AJCA's effect on discrimination litigation are statistically insignificant and near 0 in Columns (1)-(4) and exhibit no clear break when plotted in Figure 5, Panel B. Columns (5) and (6) of Table 6 validate that the AJCA did indeed result in a positive cash flow shock leading to more shareholder payouts at exposed firms (Blouin and Krull 2009; Dharmapala, Foley, and Forbes 2011). Thus, we conclude that there is no evidence that increased financial slack alters the frequency of workplace discrimination litigation, even for constrained firms with high leverage. Relative to Cohn and Wardlaw (2016), who show that the AJCA led to a decrease in workplace injury rates, our null results suggest that firms lack the incentive or ability to optimize the incidence of workplace discrimination.

5. Consequences of discrimination lawsuits

The above analysis provides no evidence that market forces can temper employment

discrimination. In this section, we shed additional light on these null results by examining the consequences of discrimination suits. If consequences are small, there may be insufficient scope for economic forces to limit employment discrimination. First, we measure the stock market reaction around civil rights lawsuit filings to test whether investors view discrimination as costly. Second, we examine CEO labor market consequences to assess whether managers are held responsible for discrimination litigation against their firm. Finally, we study case outcomes to better understand the legal penalties associated with workplace discrimination.

5.1 Stock market response

We begin with an event study that quantifies investor reactions to federal discrimination lawsuit filings. This test helps us differentiate between taste-based and information-based motives and determine the expected cost of litigated discrimination. Taste-based models argue that workplace discrimination is motivated by prejudice devoid of economic merit. As such, we expect discrimination lawsuit filings to be met with a negative stock price response reflecting potential legal, reputational, and productivity costs. In contrast, statistical models posit that discrimination is profit-maximizing if group characteristics enable employers to better discern the quality of job applicants. Under this alternative, discrimination yields a benefit net of expected costs, and the revelation of discrimination could be met with a positive stock price reaction.

Table 7 reports the mean and median percentage cumulative abnormal return (CAR) for our full sample of 38,055 employment discrimination suits against 2,952 U.S. public companies with data available in Compustat and CRSP between 1992 and 2018. We calculate market model and Fama-French 3 Factor CARs using CRSP value-weighted index returns and a one-year estimation window (252 trading days) ending one month (20 trading days) before the [-1, +1] or [-2, +2] event window relative to the lawsuit filing (t=0). Across all models and event-windows,

average CARs are negative and vary from -0.031% to -0.064%, roughly equivalent to a $8.25 to $17.02 million value reduction for the average defendant firm. Median CARs range slightly more negative, from -0.085% to -0.100%, corresponding to roughly $22.61 to $26.60 million of value loss.[26] All estimates are statistically different from zero, typically at the 1% level.

Together, these results suggest that discriminatory behavior destroys shareholder wealth, but the magnitude is modest relative to the average corporate defendant's $26.6 billion market capitalization. For comparison, Karpoff, Koester, Lee, and Martin (2017) find that the average federal securities class action lawsuit alleging financial misrepresentation elicits a negative 1.24% stock price reaction, over an order of magnitude larger than discrimination suits, and Bizjak and Coles (1995) find that defendants of antitrust litigation experience a 0.60% loss in value. While the value loss from a single discrimination suit is relatively small – perhaps small enough to provide insufficient incentives to temper future discrimination – the aggregate value destruction is large. Summing over 38,055 lawsuits, we estimate the total value destruction to be between $314 and $648 billion, or roughly $12 to $24 billion per year.

Our interpretation of these results is subject to the following limitations. First, event study estimates capture the market reaction to discrimination lawsuit filings which, in addition to revealing discrimination, can impose legal and reputational penalties that could alter future firm behavior. If litigation deters future discriminatory behavior, the filing of a lawsuit could increase firm value by limiting future animus-based inefficiencies. Second, the stock price reaction to a lawsuit filing may be dampened via market anticipation or information leakage (Gande and Lewis

[26] 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.

2009). Third, investors may not fully appreciate the costs of workplace discrimination at the time of the filing and their reaction may be biased by their own views toward discrimination. For example, Denes and Seppi (2022) identify a structural break in stock price reactions to race-related events after the widely covered murder of George Floyd in 2020, and Pan, Pikulina, Siegel, and Wang (2022) show that firms with more inequality-averse shareholders experience a more negative market response to the disclosure of a high CEO-worker pay ratio. Thus, our estimates may not capture the total value destroyed by employment discrimination.

5.2 CEO turnover

A large literature shows that CEOs are held accountable for personal indiscretions (Biggerstaff, Cicero, and Puckett 2015; Cline, Walkling, and Yore 2018) and firm performance (Gilson 1989; Huson, Parrino, and Starks 2002; Lehn and Zhao 2006) even when poor performance is caused by factors beyond their control (Jenter and Kanaan 2015). If investors and boards perceive workplace discrimination to be costly, lawsuit filings may increase the likelihood of subsequent CEO turnover at defendant firms. To test this conjecture, we next examine the relation between discrimination litigation and CEO turnover.

We collect CEO turnover data from Gentry, Harrison, Quigley, and Boivie's (2021) public database containing the date and reason for all CEO departures at S&P 1500 firms between 2000 and 2018. From this data, we construct two dependent variables: CEO Turnover, which is an indicator that equals one if the CEO departed during the firm-year, and Involuntary CEO Turnover, which is an indicator that equals one if Gentry et al. (2021) classify the departure as "Involuntary – Dismissed for Performance or Behavioral Issues" based on relevant news article. Table 8 reports OLS regressions of these dependent variables on lagged discrimination lawsuit frequency, control variables, and firm and year fixed effects. Although these regressions exploit

within-firm variation and control for observable characteristics known to influence CEO turnover, we caution against interpreting the resulting estimates as causal.

Across all specifications, we find no evidence that discrimination litigation increases the likelihood of CEO turnover. The economically small and statistically insignificant coefficient for a discrimination lawsuit pales in comparison to labor market consequences for other types of corporate litigation.[27] For example, Jenter and Lewellen (2021) document an elevated CEO turnover rate after federal securities class action lawsuits and Karpoff, Lee, and Martin (2008) show that 88.4% of CEOs culpable for financial misrepresentation lose their job by the end of the regulatory enforcement period. One potential reason why discrimination litigation may not increase the chance of CEO turnover is that these suits are associated with relatively small value losses compared to other types of litigation. Indeed, Karpoff et al. (2008) show that the likelihood of dismissal increases with the cost of misconduct to shareholders and Jenter and Lewellen (2021) conclude that boards appear willing to forgive misconduct if firm performance is sufficiently high. Therefore, in the absence of a larger stock price reaction to lawsuit filings, CEO labor markets may not be a sufficiently strong mechanism to limit workplace discrimination.

5.3 Case outcomes

As a final step toward understanding why economic forces may not deter workplace discrimination, we examine legal penalties. Courts have substantial discretion over legal remedies, including awarding injunctive relief (such as affirmative steps to prevent future discrimination), back pay, compensatory damages, and punitive damages. We note, however, that monetary damage caps are low relative to the financial resources of a typical public company. The maximum

[27] Aharony, Liu, and Yawson (2015) study a sample of lawsuits filed against S&P 1500 firms between 2000 and 2007. The authors find that CEO turnover is positively associated with securities and contractual suits, not significantly related to antitrust suits, and negatively associated with intellectual property and environmental suits.

damage that can be awarded in a federal civil rights lawsuit is $300,000 per plaintiff (42 U.S.C. § 1981a(b)(3)), which accounts for less than 1.5% of the median estimated loss in market value around a lawsuit filing.[28] In comparison, Karpoff, Lee, and Martin (2008) find that the average securities class action lawsuit settlement is $37.7 million and Karpoff, Lott, and Wehrly (2005) find that the average damage awarded for an environmental violation is $13.2 million with an additional $93.6 million cost for compliance and cleanup. Under optimal penalty theory, the expected total penalty for an illegal activity should equal the activity's total social cost (Becker 1968). While it is impossible to quantify the total social cost of workplace discrimination, Hsieh, Hurst, Jones, and Klenow (2019) estimate that over 20% of U.S. GDP growth from 1960 to 2010 can be attributed to greater gender and racial balance in the workplace. Given the small stock price reaction to discrimination lawsuit filings and the even smaller monetary damage limit, we conclude that current legal and reputational penalties are insufficient to deter workplace discrimination.

6. Limitations of the analysis

The combined evidence paints a clear picture of taste-based corporate discrimination that does not appear to be tempered by market forces. 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

[28] The FJC cautions against using their data to assess damages because dollar values are inconsistently reported. In the 5,989 suits in our sample with non-missing data, the median monetary damage sought by the plaintiff is $200,000.

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 points where our proxy may depart from its unobserved counterpart. First, the employee must decide whether to ignore an incident or initiate a discrimination claim. 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 economic and cultural factors, biasing estimates in an unclear direction.

Although it is impossible to completely rule out this issue, the following features provide credibility for our inferences. First, institutional details regarding legal procedure and jurisdiction alleviate concerns that employees can forum shop or strategically time their lawsuit. 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 federal discrimination claims generally must be filed within 180 days of an incident. Second, within-firm variation used in our natural experiments eliminates 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 a suit in state, rather than federal, court). Finally, we examine whether lawsuit outcomes are related to firm, industry, and headquarter-area characteristics. If cultural or economic factors are related to the likelihood of settling a case before filing suit, the likelihood of a case going to trial should vary along these dimensions. In contrast to this alternative explanation, Appendix 4 shows that the fraction of cases that ultimately go to trial is not significantly related to our independent variables of interest. While we acknowledge that litigation filings are an imperfect proxy of underlying discrimination, these details support the reliability of our inferences and enable us to provide novel insights into the prevalence, causes, and consequences of workplace discrimination.

7. Conclusion

In the aftermath of the #MeToo and #BlackLivesMatter movements, the Business Roundtable stated: "America's largest employers know that the economy works best when employees can be who they are, without fear of bias, discrimination and inequality."[29] This statement echoes key features of the seminal taste-based model of discrimination formalized by Gary Becker more than 60 years earlier. Becker considered discrimination to be driven by individual prejudice with no economic merit. He argued that an employer will discriminate if the non-pecuniary benefit of indulging in his animus outweighs the financial costs but doing so will place his firm at a competitive disadvantage relative to less discriminatory peers.

We examine this economic framework using novel data on employment discrimination lawsuits filed in federal court against U.S. public companies between 1992 and 2018. Consistent with the Business Roundtable's statement and Becker's model, the filing of a discrimination lawsuit corresponds to a $8.25 to $17.02 million loss in market capitalization at the defendant firm, on average. Over our 27-year sample period, this translates to between $314 and $648 billion in total value destruction, or roughly $12 to $24 billion per year.

Variation in discrimination litigation rates appears to be driven by differences in societal and corporate culture. Our analyses imply that the typical discriminating firm is a large corporation with a male CEO and a high number of CSR concerns that is headquartered in an area where residents hold more racist and sexist views. To evaluate whether culture plays a causal role, we study the staggered expansion of a conservative TV news station and find that a rightward shift in local media slant leads to a significant increase in discrimination suits against firms headquartered

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

nearby. Together, these results provide empirical support for taste-based models by showing that civil rights litigation rates reflect attitudes toward race and gender.

However, we find little evidence supporting dynamic interpretations of Becker's model. Studying two natural experiments that exogenously alter firms' financial slack, either through an increase in competition or a cash windfall, we find no significant relation between discrimination lawsuit frequency and economic conditions. Nor do we find any evidence supporting statistical models that argue discrimination is profit-maximizing if group characteristics allow employers to better discern the quality of applicants. Instead, our textual analysis shows that most discrimination suits allege harassment or retaliation, which are actions with no economic justification.

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 diversity, equity, and inclusion, in addition to formal public policy, may be the most effective method to reduce workplace discrimination.

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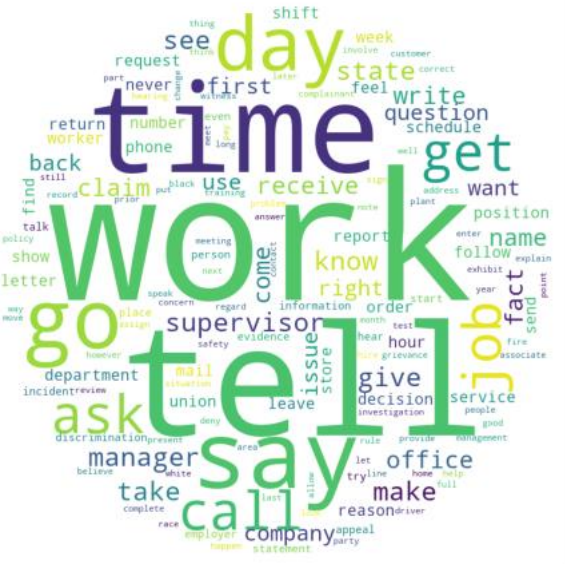
Figure 1: Discrimination lawsuit word clouds. This figure categorizes the contents of employment discrimination lawsuits filed against U.S. public companies between 1992 and 2018. Our full sample consists of 38,055 employment discrimination lawsuits (Nature of Suit 442-Civil Rights Job) in the Federal Judicial Center (FJC) Civil Integrated Database with a defendant in Compustat. We employ Latent Dirichlet Allocation (LDA) topic modeling analysis on a subsample of 14,310 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



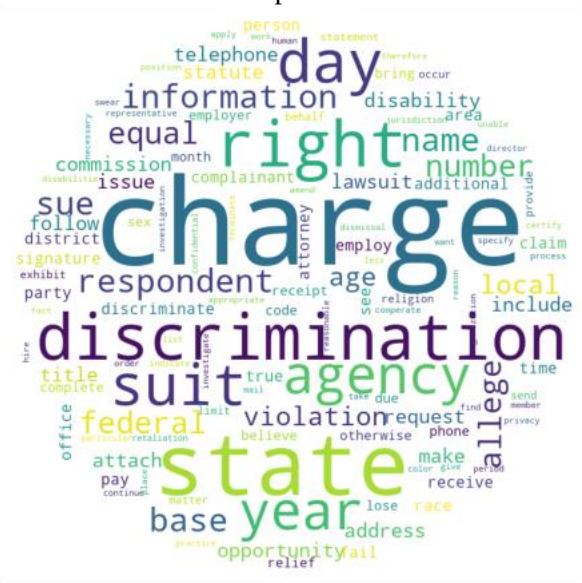
Topic 3

Topic 4

Figure 1: Discrimination lawsuit word clouds. (Continued)

Topic 5

Topic 6



Topic 7

Topic 8

Figure 2: Industry distribution of discrimination lawsuits. This figure displays the annual fraction of U.S public companies facing an employment discrimination lawsuit in Federal Court, split by Fama-French 12 industry classification. The sample consists of 95,015 firm-year observations from 10,229 U.S. public companies with data available in Compustat between 1992 and 2018.

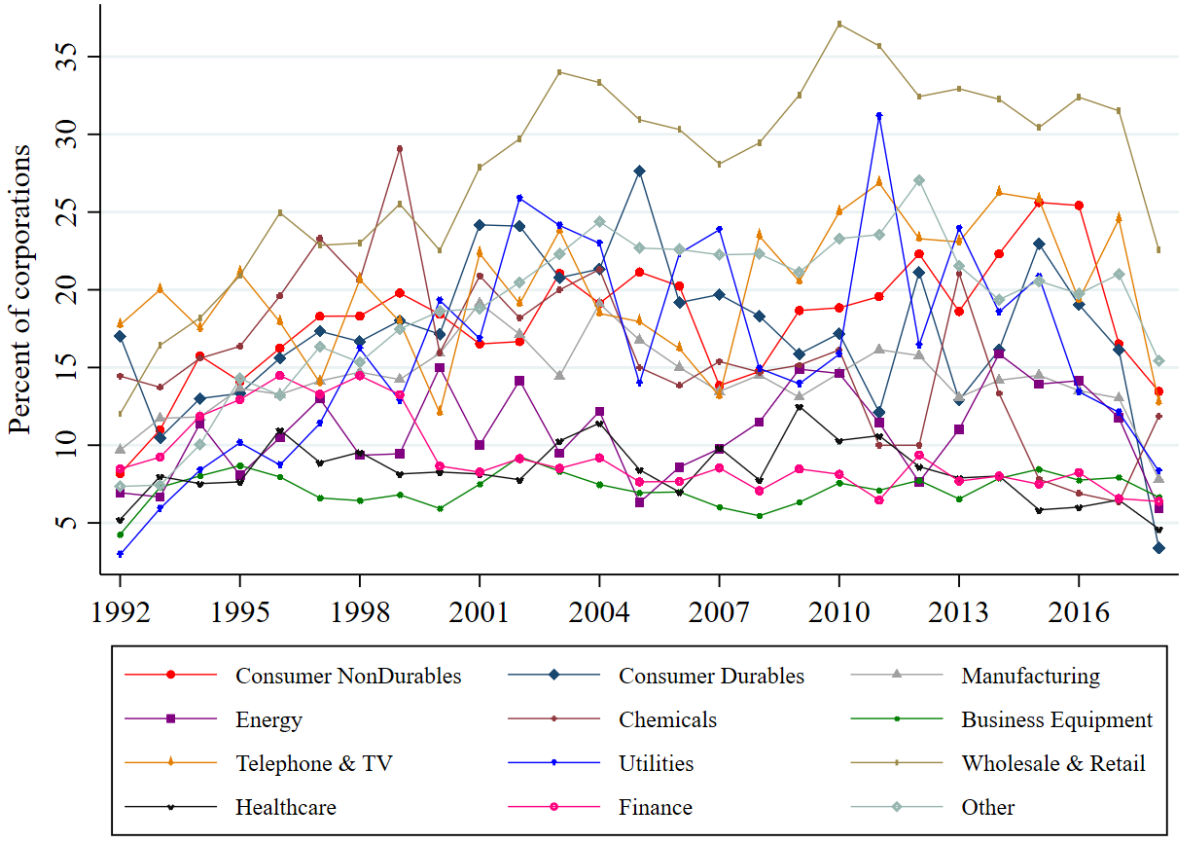


Figure 3: Geographic distribution of discrimination lawsuits. This figure displays the fraction of firm-year observations facing an employment discrimination lawsuit in Federal Court, split by headquarter location. The sample consists of 95,015 firm-year observations from 10,229 U.S. public companies with data available in Compustat between 1992 and 2018.

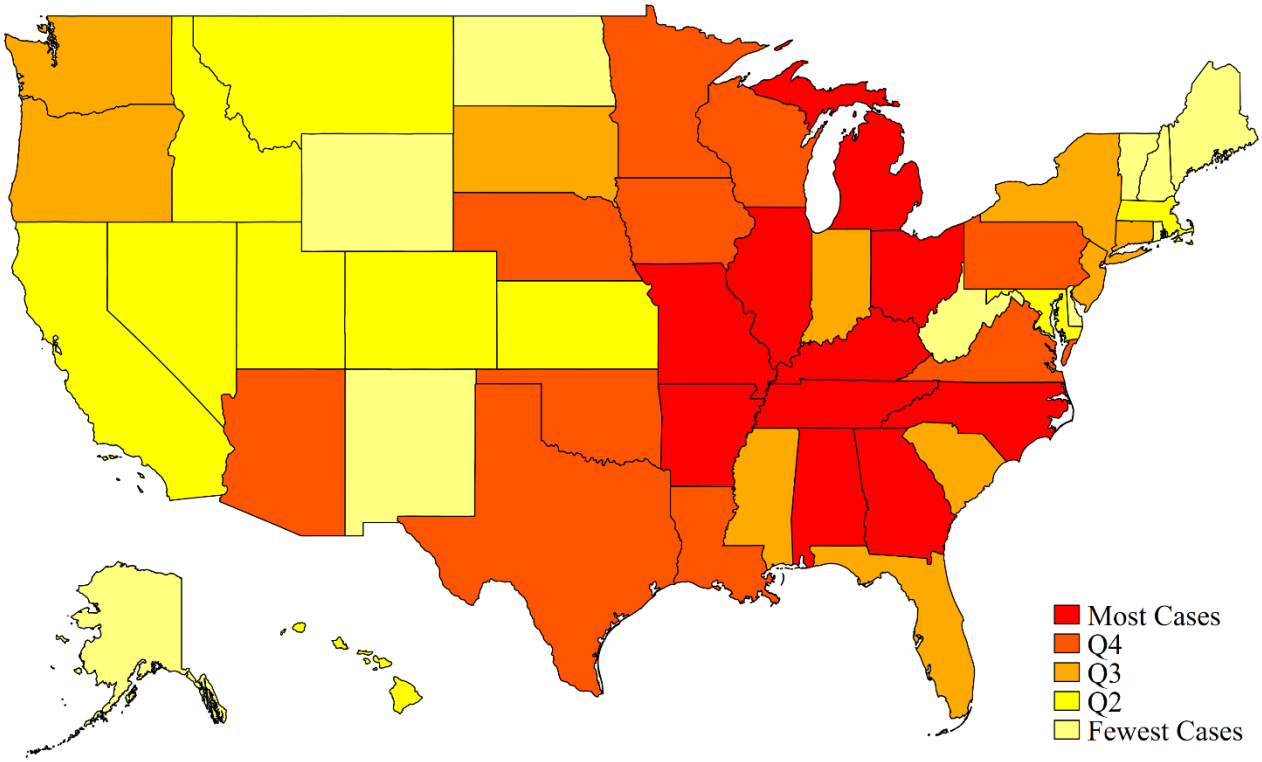


Figure 4: Local news ideology and discrimination lawsuit dynamics. This figure plots the effect of exposure to right-wing local news on the frequency of employment discrimination lawsuits. To cleanly estimate dynamic treatment effects, we implement Sun and Abraham's (2021) interaction weighted estimator using a never-treated control group. Specifically, we estimate the regression from Table 4 Column (4) augmented with time indicators relative to the first full year of Sinclair TV Exposure, cohort indicators, and interactions of these indicators, calculate the cohort shares underlying each relative time, and plot average cohort-specific ATTs weighted by each cohort's share of sample. The gray shading represents 90% pointwise confidence intervals using heteroskedasticity-consistent standard errors clustered by county.

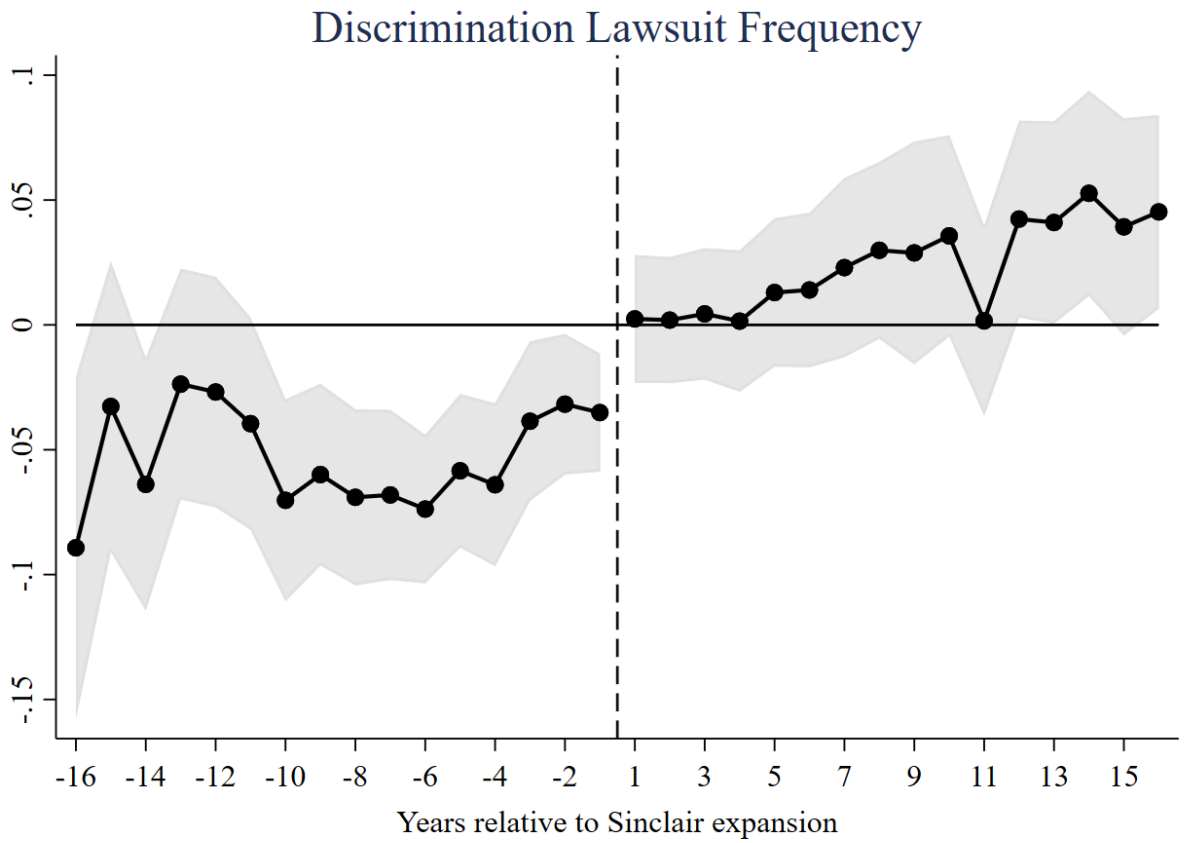
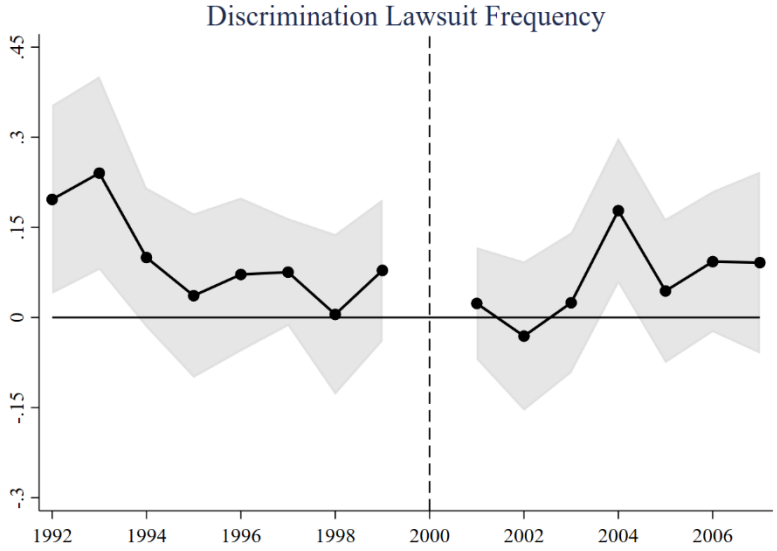


Figure 5: Economic conditions and discrimination lawsuit dynamics. This figure plots the effect of changing economic conditions on the frequency of employment discrimination lawsuits. Panel A plots the dynamic effect of import competition using the same regression specification as Table 5 Column (4), 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 6 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.

Panel A: Effect of the China trade shock



Panel B: Effect of the American Jobs Creation Act

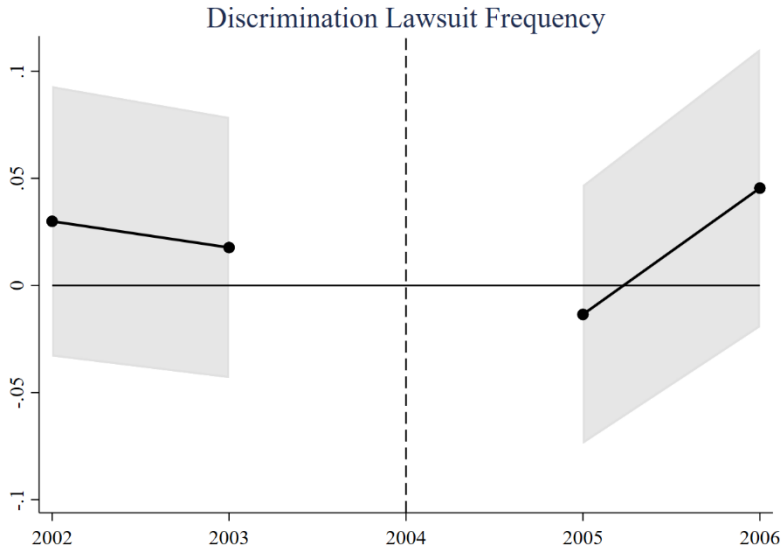


Table 1: Discrimination lawsuit topics. This table categorizes the contents of employment discrimination lawsuits filed against U.S. public companies between 1992 and 2018. We employ LDA topic modeling analysis on a sample of 14,310 lawsuits for which we have access to the case complaint document via 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
% of Complaints	38.62%	6.21%	11.14%	10.71%	6.62%	16.80%	3.82%	6.09%
Avg # Words	981	2,203	1,225	2,418	1,509	1,143	1,648	3,114
% Harass. 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%
Top 10 Words by Saliency	damage discrimination right violation relief practice suffer attorney trial conduct	work tell time say go day call ask job get	disability leave work medical damage accommodation time request reasonable violation	service damage attorney party serve allege state cause claim name	harassment work manager sexual environment conduct hostile store make tell	charge state discrimination right day suit agency year respondent sue	class personal property liability labor product injury place state enter	company performance information position manager pay sale time plan year
Cause Distribution	38% Emp. 22% Race 16% Age 9% Gender 6% Disability 4% Unknown 2% Misc 1% FMLA 1% Comp. 0% Religion	52% Emp. 18% Race 8% Unknown 7% Disability 7% Age 5% Gender 2% Misc 1% FMLA 0% Religion	41% Disability 23% Emp. 20% FMLA 8% Unknown 3% Age 2% Comp. 2% Race 1% Misc 1% Gender	51% Unknown 23% Emp. 7% Race 5% Disability 5% Age 4% Gender 3% FMLA 2% Misc 1% Comp.	45% Emp. 30% Gender 11% Race 7% Unknown 3% Disability 2% Age 1% Misc 1% FMLA 0% Religion 0% Comp.	46% Emp. 21% Race 12% Disability 9% Age 8% Gender 3% Unknown 1% Misc 0% FMLA 0% Comp. 0% Religion	36% Emp. 16% Age 13% Disability 12% Gender 12% Race 5% Unknown 3% FMLA 2% Misc 1% Comp.	38% Emp. 14% Age 13% Unknown 12% Race 8% Disability 5% Gender 3% Misc 3% FMLA 2% Comp. 1% Religion

Table 2: Discrimination lawsuit frequency. This table displays the percentage of U.S. public companies that faced an employment discrimination lawsuit in Federal Court between 1992 and 2018. The sample consists of 95,015 firm-year observations from 10,229 companies with data available in Compustat.

	Percentage
Fraction of firms ever facing discrimination suit	29.7
Fraction of firm-year observations with discrimination suit	13.4
By industry	
Consumer NonDurables	34.9
Consumer Durables	32.6
Manufacturing	33.5
Oil, Gas, and Coal Extraction and Products	29.2
Chemicals and Allied Products	34.5
Business Equipment: Computers, Software, Electronics	20.7
Telephone and Television Transmission	31.8
Utilities	42.5
Wholesale, Retail, and Some Services	43.2
Healthcare, Medical Equipment, and Drugs	18.5
Finance	20.0
Other	32.4
By size	
Less than $100M	9.3
$100M to $250M	23.7
$250M to $500M	32.7
$500M to $1,000M	40.4
$1,000M to $2,500M	49.4
$2,500M to $5,000M	55.3
Greater than $5,000M	71.5
Firm does not have credit rating	19.1
Firm has credit rating	62.4

Table 3: Which firms face discrimination lawsuits? This table reports estimates from OLS regressions that relate employment discrimination lawsuits to lagged firm, industry, and headquarter-area characteristics. The full sample consists of 95,015 firm-year observations from 10,229 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.

	Discrimination Lawsuit			**Discrimination Lawsuit Frequency**		
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Employees	0.065***	0.020***	0.076***	0.085***	0.026***	0.111***
	(0.002)	(0.002)	(0.023)	(0.004)	(0.003)	(0.031)
Size	0.002	0.022***	0.060***	0.006*	0.029***	0.121***
	(0.002)	(0.003)	(0.021)	(0.003)	(0.003)	(0.029)
Age	0.006**	0.023***	-0.008	0.007	0.022***	0.061
	(0.003)	(0.005)	(0.062)	(0.005)	(0.008)	(0.087)
Stock Return	-0.009***	-0.005***	-0.029***	-0.010***	-0.006***	-0.039***
	(0.002)	(0.001)	(0.010)	(0.002)	(0.002)	(0.013)
ROA	-0.078***	-0.030***	-0.152*	-0.156***	-0.035***	-0.216**
	(0.008)	(0.007)	(0.085)	(0.015)	(0.007)	(0.096)
Market-to-Book Ratio	0.010***	0.002**	0.022***	0.013***	0.003***	0.025***
	(0.001)	(0.001)	(0.007)	(0.002)	(0.001)	(0.009)
Leverage	-0.014	-0.009	0.031	-0.023	-0.034***	-0.031
	(0.011)	(0.010)	(0.057)	(0.017)	(0.012)	(0.074)
Investment Grade	0.145***	0.031***	-0.040	0.246***	0.071***	-0.026
	(0.012)	(0.010)	(0.031)	(0.021)	(0.014)	(0.037)
Speculative Grade	0.037***	0.022***	-0.037	0.024*	0.025***	-0.023
	(0.008)	(0.007)	(0.031)	(0.013)	(0.009)	(0.037)
HHI	0.016*	0.015*	-0.015	0.028*	0.038***	0.057
	(0.009)	(0.008)	(0.045)	(0.015)	(0.011)	(0.057)
Racial Bias Index	0.132***			0.168***		
	(0.029)			(0.050)		
Least Sexist State	-0.019***			-0.031***		
	(0.007)			(0.010)		
Republican Vote Share	0.074***			0.083**		
	(0.021)			(0.035)		
Black Population	0.063***			0.087**		
	(0.023)			(0.043)		
Rural	-0.023***			-0.013		
	(0.009)			(0.013)		
Unemployment Rate	-0.030**			-0.019		
	(0.014)			(0.026)		
Income Per Capita	0.007			0.042*		
	(0.012)			(0.023)		
CSR Concerns			0.017***			0.014
			(0.006)			(0.011)
Female CEO			-0.098			-0.148**
			(0.070)			(0.075)
Entrenchment Index			0.018*			0.024
			(0.011)			(0.015)
Firm Fixed Effects	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	95,015	95,015	9,500	95,015	95,015	9,500
Adjusted R-squared	0.237	0.431	0.492	0.267	0.644	0.735

Table 4: Local news ideology and discrimination lawsuits. This table reports the effect of exposure to right-wing local news on the frequency of employment discrimination lawsuits. The sample consists of 81,189 firm-year observations from 9,227 U.S. public companies with data available in Compustat between 1996 and 2018. Heteroskedasticity-consistent standard errors clustered by county are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Appendix 1 lists variable definitions.

	Discrimination Lawsuit		**Discrimination Lawsuit Frequency**	
	(1)	(2)	(3)	(4)
Sinclair TV Exposure	0.013**	0.013**	0.022***	0.022***
	(0.006)	(0.006)	(0.007)	(0.007)
Number of Employees		0.019***		0.023***
		(0.003)		(0.003)
Size		0.023***		0.030***
		(0.003)		(0.003)
Age		0.033***		0.045***
		(0.006)		(0.008)
Stock Return		-0.003**		-0.003***
		(0.001)		(0.001)
ROA		-0.025***		-0.028***
		(0.005)		(0.005)
Market-to-Book Ratio		0.001***		0.002***
		(0.000)		(0.001)
Leverage		-0.014		-0.043***
		(0.013)		(0.016)
Investment Grade Rating		0.014		0.045***
		(0.013)		(0.016)
Speculative Grade Rating		0.020**		0.020**
		(0.008)		(0.008)
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Observations	81,189	81,189	81,189	81,189
Adjusted R-squared	0.455	0.459	0.678	0.681
Stacked Regressions: Sinclair TV	0.015**	0.016**	0.028***	0.029***
	(0.007)	(0.007)	(0.007)	(0.007)

Table 5: Import competition and discrimination lawsuits. This table reports the effect of import competition on the frequency of employment discrimination lawsuits. The sample consists of 25,288 firm-year observations from 3,291 U.S. public manufacturing firms with data available in Compustat between 1992 and 2007. China Trade Shock Exposure measures the potential tariff hike the firm's industry faced before China obtained permanent MFN trade status in 2000. Post is an indicator that equals one from 2000 onwards. Heteroskedasticity-consistent standard errors clustered by industry are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Appendix 1 lists variable definitions.

	Discrimination Lawsuit		**Discrimination Lawsuit Frequency**		**ROA**	
	(1)	(2)	(3)	(4)	(5)	(6)
Post * China Trade Shock Exposure	-0.024	-0.025	-0.016	-0.023	-0.088***	-0.055**
	(0.045)	(0.047)	(0.060)	(0.062)	(0.025)	(0.027)
Post * High Tech Industry		0.001		0.006		-0.041**
		(0.015)		(0.018)		(0.020)
Post * Unskilled Labor Percentage		0.029		0.048		-0.092*
		(0.052)		(0.065)		(0.052)
Firm FE	Yes	Yes	Yes	Yes		
Year FE	Yes	Yes	Yes	Yes		
Observations	25,288	25,288	25,288	25,288	25,288	25,288
Adjusted R-squared	0.440	0.440	0.641	0.641	0.725	0.726

Table 6: Cash windfalls and discrimination lawsuits. This table reports the effect of a cash windfall from the American Jobs Creation Act on the frequency of employment discrimination lawsuits. The sample consists of 4,522 firm-year observations from 1,249 U.S. public firms with data available in Compustat from 2002 to 2003 and 2005 to 2006. AJCA Exposure is an indicator that equals one if the firm's cumulative reported foreign profits from 2001 to 2003 are positive and zero otherwise. Post is an indicator that equals one from 2004 onwards. Heteroskedasticity-consistent standard errors clustered by firm are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Appendix 1 lists variable definitions.

	Discrimination Lawsuit		**Discrimination Lawsuit Frequency**		**Shareholder Payouts**	
	(1)	(2)	(3)	(4)	(5)	(6)
Post * AJCA Exposure	-0.001	-0.026	0.002	-0.014	0.009***	0.010**
	(0.024)	(0.031)	(0.024)	(0.030)	(0.004)	(0.005)
Post * AJCA Exposure * Leverage		0.157		0.103		-0.007
		(0.137)		(0.138)		(0.024)
Post * Leverage		-0.095		-0.071		-0.002
		(0.098)		(0.101)		(0.020)
AJCA Exposure * Leverage		-0.046		0.014		0.007
		(0.146)		(0.153)		(0.026)
Leverage		0.020		-0.014		-0.048**
		(0.103)		(0.112)		(0.019)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,522	4,522	4,522	4,522	4,522	4,522
Adjusted R-squared	0.473	0.472	0.715	0.714	0.296	0.300

Table 7: Shareholder value implications of discrimination lawsuits. This table reports percentage cumulative abnormal returns (CARs) around employment discrimination lawsuit filings. The sample consists of 38,055 employment discrimination lawsuits against 2,952 U.S. public companies with data available in Compustat and CRSP between 1992 and 2018. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Appendix 1 lists variable definitions.

	3-day CARs		**5-day CARs**	
	Market Adjusted	Fama-French 3 Factor	Market Adjusted	Fama-French 3 Factor
Mean CAR (%)	-0.031*	-0.036**	-0.048**	-0.064***
Median CAR (%)	-0.100***	-0.093***	-0.085***	-0.092***
Observations	38,055	38,055	38,055	38,055

Table 8: Discrimination lawsuits and CEO turnover. This table presents estimates from OLS regressions that relate CEO dismissals to lagged employment discrimination lawsuits and firm characteristics. The sample consists of 35,391 firm-years from 3,050 U.S. public companies with data available in Compustat and Execucomp between 1993 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.

	CEO Turnover		**Involuntary CEO Turnover**	
	(1)	(2)	(3)	(4)
Discrimination Lawsuit	0.004		-0.002	
	(0.006)		(0.003)	
Discrimination Lawsuit Frequency		0.001		-0.002
		(0.006)		(0.003)
Number of Employees	0.016**	0.016**	0.007*	0.007*
	(0.007)	(0.007)	(0.004)	(0.004)
Size	-0.002	-0.002	0.007*	0.007*
	(0.006)	(0.006)	(0.004)	(0.004)
Age	0.078***	0.078***	0.002	0.002
	(0.012)	(0.012)	(0.006)	(0.006)
Stock Return	-0.018***	-0.018***	-0.008***	-0.008***
	(0.003)	(0.003)	(0.002)	(0.002)
ROA	-0.082***	-0.082***	-0.043***	-0.043***
	(0.027)	(0.027)	(0.013)	(0.013)
Market-to-Book Ratio	0.002	0.002	0.001	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Leverage	0.029	0.029	0.027**	0.027**
	(0.022)	(0.022)	(0.011)	(0.011)
Investment Grade Rating	-0.013	-0.013	-0.001	-0.001
	(0.011)	(0.011)	(0.005)	(0.005)
Speculative Grade Rating	0.003	0.003	-0.007	-0.007
	(0.010)	(0.010)	(0.005)	(0.005)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	35,391	35,391	35,391	35,391
Adjusted R-squared	0.085	0.085	0.032	0.032

Appendix 1: Variable definitions. This table lists variable definitions and data sources. FJC denotes the Federal Judicial Center Civil Integrated Database. CCM denotes the CRSP-Compustat Merged Database. LLR refers to Levine, Levkov, and Rubinstein (2008). CGP refers to Charles, Guryam, and Pan (2018). KLD denotes the MSCI ESG KLD STATS database. MIT refers to the MIT Election Data and Science Lab. ECOMP refers to the ExecuComp Annual Compensation database. RM refers to the RiskMetrics Governance database. KLM refers to Kaviani, Li, and Maleki (2021). NBER refers to the NBER-CES Manufacturing Industry Database. Romalis denotes John Romalis' website. GHQB refers to Gentry, Harrison, Quigley, and Boivie (2021).

Variable	Source	Description
Discrimination Lawsuit	FJC	Indicator that equals one if an employment discrimination lawsuit is filed in Federal Court against the firm (Nature of Suit 442-Civil Rights Job), and zero otherwise
Discrimination Lawsuit Frequency	FJC	Log one plus the number of employment discrimination lawsuits filed in Federal Court against the firm (Nature of Suit 442-Civil Rights Job)
Number of Employees	CCM	Log number of employees (in thousands)
Size	CCM	Log total assets
Age	CCM	Log number of years since firm was first listed in Compustat
Stock Return	CCM	Buy-and-hold abnormal return (BHAR) over the fiscal year using the CRSP value-weighted index as market proxy
ROA	CCM	Operating income before depreciation divided by total assets
Market Value of Assets	CCM	Market value of equity minus book value of equity plus total assets
Market Value of Equity	CCM	Common shares outstanding times the fiscal year closing price
Market-to-Book Ratio	CCM	Ratio of market value of assets to book value of assets
Leverage	CCM	Long-term debt plus debt in current liabilities divided by total assets
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
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
HHI	CCM	Sales based Herfindahl-Hirschman Index, calculated at the 6-digit NAICS industry-year level
Racial Bias Index	LLR	Difference between predicted and actual interracial marriage rates during 1970 in the firm's headquarter state
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
Republican Vote Share	MIT	Fraction of county-level votes cast for the Republican candidate in the most recent U.S. Presidential Election
Black Population	Census	County-level percentage of population that is Black
Rural	NCHS	Indicator that equals one if the 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 for Counties, and zero otherwise

Appendix 1: Variable definitions (Continued).

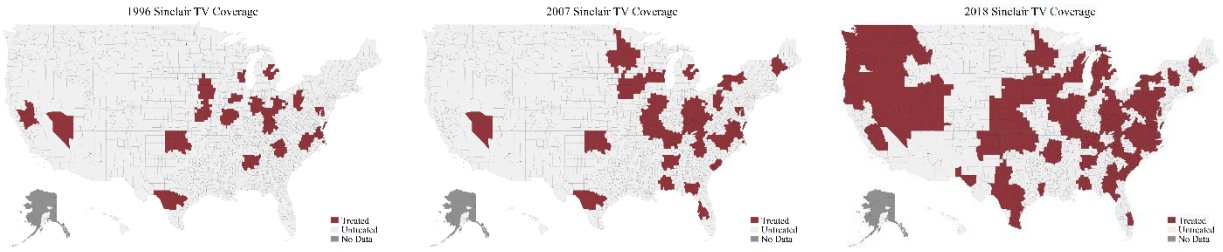
Variable	Source	Description
Unemployment Rate	Census	County-level unemployment rate
Income Per Capita	Census	County-level log income per capita
CSR Concerns	KLD	The number of community, diversity, employee relations, environment, and human rights concerns
Female CEO	ECOMP	Indicator that equals one if the CEO is female, and zero otherwise
Entrenchment Index	RM	Entrenchment index, developed by Bebchuk, Cohen, and Ferrell (2009), that counts the presence of six shareholder rights provisions: staggered boards, poison pills, golden parachutes, supermajority requirements for charter amendments, supermajority requirements for bylaw amendments, and supermajority requirements for mergers
Sinclair TV Exposure	KLM	Indicator that equals one if Sinclair broadcasts local television news in the firm's headquarter county, and zero otherwise
China Trade Shock Exposure	Romalis	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
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
Unskilled Labor Percentage	NBER	The fraction of employees in the firm's industry in 1999 that were production workers
AJCA Exposure	CCM	Indicator that equals one if the firm's cumulative reported foreign profits from 2001 to 2003 are positive, and zero otherwise
Shareholder Payouts	CCM	Cash dividends plus purchase of common and preferred stock scaled by lagged market value of equity
CEO Turnover	GHQB	Indicator that equals one if the firm experienced a CEO turnover, and zero otherwise
Involuntary CEO Turnover	GHQB	Indicator that equals one if the CEO was dismissed for cause, and zero otherwise
CAR	CCM	Market model (or Fama-French 3 Factor) cumulative abnormal return estimated using CRSP value-weighted index returns and a one-year estimation window (252 trading days) ending one month (20 trading days) before the [-1, +1] (or -2,+2) event window

Appendix 2: Sample description. The full sample consists of 95,015 firm-year observations from 10,229 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.

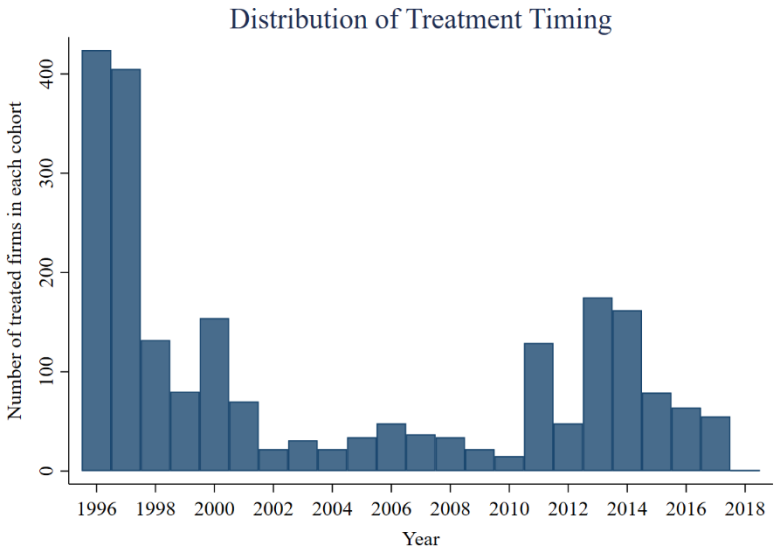
	Mean	**S.D.**	**P25**	**Median**	**P75**	**Obs**
Discrimination Lawsuit	0.134	0.341	0.000	0.000	0.000	95,015
Discrimination Lawsuit Frequency	0.150	0.444	0.000	0.000	0.000	95,015
Number of Employees	-0.130	2.089	-1.666	-0.201	1.352	95,015
Size	5.724	2.197	4.093	5.650	7.200	95,015
Age	2.555	0.838	1.946	2.565	3.178	95,015
Stock Return	0.015	0.610	-0.342	-0.070	0.215	95,015
ROA	0.051	0.200	0.019	0.091	0.153	95,015
Market-to-Book Ratio	1.977	1.647	1.042	1.376	2.200	95,015
Leverage	0.194	0.186	0.021	0.150	0.316	95,015
Investment Grade	0.119	0.324	0.000	0.000	0.000	95,015
Speculative Grade	0.100	0.300	0.000	0.000	0.000	95,015
HHI	0.345	0.270	0.146	0.248	0.473	95,015
Racial Bias Index	0.253	0.083	0.150	0.280	0.320	95,015
Least Sexist State	0.106	0.307	0.000	0.000	0.000	95,015
Republican Vote Share	0.364	0.142	0.261	0.364	0.444	95,015
Black Population	0.138	0.118	0.048	0.106	0.200	95,015
Rural	0.076	0.264	0.000	0.000	0.000	95,015
Unemployment Rate	0.298	0.245	0.251	0.348	0.424	95,015
Income Per Capita	10.617	0.409	10.313	10.590	10.866	95,015
CSR Concerns	1.205	1.448	0.000	1.000	2.000	9,500
Female CEO	0.017	0.128	0.000	0.000	0.000	9,500
Entrenchment Index	2.273	1.292	1.000	2.000	3.000	9,500
Sinclair TV Exposure	0.198	0.398	0.000	0.000	0.000	81,189
China Trade Shock Exposure	0.317	0.121	0.295	0.329	0.363	25,288
High Tech Industry	0.319	0.466	0.000	0.000	1.000	25,288
Unskilled Labor Percentage	0.598	0.147	0.500	0.584	0.723	25,288
ROA (China Trade Shock Sample)	0.018	0.275	-0.014	0.101	0.165	25,288
AJCA Exposure	0.523	0.500	0.000	1.000	1.000	4,522
Shldr. Payouts (AJCA Sample)	0.026	0.051	0.000	0.008	0.034	4,522
CEO Turnover	0.177	0.382	0.000	0.000	0.000	35,391
Involuntary CEO Turnover	0.028	0.166	0.000	0.000	0.000	35,391

Appendix 3: Sinclair's expansion into local TV markets. This figure depicts the geographic expansion of Sinclair Broadcast Inc. during the 1996 to 2018 sample period. Panel A displays county-level snapshots of Sinclair's local TV coverage in 1996, 2007, and 2018. Panel B plots the firm-level distribution of treatment timing. The first cohort contains firms treated in 1996 or earlier because 1996 is the first year with data on Sinclair's local TV presence; these firms are "already-treated" in our staggered DiD analysis.

Panel A: Snapshots of Sinclair's geographic expansion



Panel B: Firm-level treatment timing



Appendix 4: Discrimination case outcomes. This table reports estimates from OLS regressions that relate employment discrimination lawsuit outcomes to lagged firm, industry, and headquarter-area characteristics. The sample consists of 11,580 firm-year observations where at least one discrimination lawsuit is filed against the firm. Trial Rate is the fraction of cases filed during the firm-year that ultimately go to trial. The sample mean Trial Rate is 0.23. Heteroskedasticity-consistent standard errors clustered by firm are reported in parentheses. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

	Trial Rate		
	(1)	(2)	(3)
Number of Employees	-0.007*	-0.031**	-0.016
	(0.004)	(0.015)	(0.037)
Size	0.015***	0.015	0.023
	(0.004)	(0.014)	(0.031)
Age	0.002	0.010	0.050
	(0.005)	(0.020)	(0.086)
Stock Return	-0.000	0.002	0.016
	(0.008)	(0.008)	(0.017)
ROA	0.014	-0.043	0.501***
	(0.050)	(0.079)	(0.143)
Market-to-Book Ratio	0.002	0.001	-0.014
	(0.004)	(0.006)	(0.008)
Leverage	0.003	0.008	0.115
	(0.024)	(0.040)	(0.085)
Investment Grade	-0.007	-0.010	-0.049
	(0.011)	(0.018)	(0.041)
Speculative Grade	0.016	-0.031	-0.010
	(0.012)	(0.019)	(0.046)
HHI	-0.017	-0.036	-0.087
	(0.013)	(0.024)	(0.054)
Racial Bias Index	0.080		
	(0.055)		
Least Sexist State	-0.012		
	(0.016)		
Republican Vote Share	-0.036		
	(0.040)		
Black Population	0.003		
	(0.037)		
Rural	0.016		
	(0.021)		
Unemployment Rate	0.015		
	(0.022)		
Income Per Capita	-0.007		
	(0.021)		
CSR Concerns			0.001
			(0.007)
Female CEO			-0.130*
			(0.072)
Entrenchment Index			-0.009
			(0.012)
Firm Fixed Effects	No	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	11,580	11,580	3,396
Adjusted R-squared	0.008	0.073	0.102