Prejudice in the Workplace and Firm Revenue

Isaac Hacamo*

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ABSTRACT

What is the impact on firm revenue after prospective employees learn about incidents of racial prejudice in the workplace? Using job reviews for major U.S. stores and exploiting variation on reviews published during busy days, this paper shows that reporting one incident of workplace racial prejudice on a popular job-search website reduces store foot-traffic by 3 to 4 percentage points. An analysis combining a randomized survey experiment and observational data suggests that a decline in consumption of prospective employees, rather than an adjustment in labor supply, explains this effect. A simple general equilibrium model of production with monitoring costs for workplace safety rationalizes these results. The paper highlights the value of online job-search platforms in diffusing valuable information among rank-and-file employees, and providing a potential disciplining mechanism for firms.

*Assistant Professor of Finance at Kelley School of Business, Indiana University. Email: ihacamo@iu.edu. Corresponding address: 1309 E 10th St, 47405, Bloomington, IN. This paper benefited from comments from David Autor, Carlos Avenancio-Leon, Spencer Barnes, Alex Edmans, Geraldo Cerqueiro, David Cicero, Peter Haslag, Camile Hebert, Kristoph Kleiner, Ray Fisman, Jim Goldman, Nandini Gupta, Craig Lewis, Runjing Lu, Alessandro Previtero, Veronika Pool, Francisco Queiro (discussant), Danny Qin, Sahil Raina, Merih Sevilir, Barry Scholnick, Mo Shen, Joshua White, Sheng-Jun Xu, Xiaoyun Yu, and Luigi Zingales. I also thank comments and suggestions from seminar and conference participants of Auburn University, Indiana University, University of Alberta, University of Toronto, Vanderbilt University, and the 2021 Lubrafin Conference.

Prejudice in the workplace may lead to large negative implications to employees's productivity, health, and well-being.[1] Considering these detrimental effects, information on past incidents should be easily accessible to prospective employees. But beyond high-profile cases covered in major news outlets, readily available data on prejudice regarding rank-and-file employees has been mostly inexistent. If information on employer prejudice becomes public, prospective employees may adjust their labor supply and/or consumption of goods for some firms. Introducing an online job review platform may then have significant implications on firm performance as it may allow job seekers to easily learn about incidents of workplace prejudice.

Analyzing 7 million job reviews from a job-search website that receives 270 million monthly visits, this paper identifies over 10,000 instances of workplace racial prejudice. The paper uses two empirical strategies to document the impact on firm revenue after prospective employees learn about prejudice incidents. The first strategy compares the impact of prejudice reviews published in weeks of the month when website traffic is high with those published in weeks when traffic is low. The second design compares prejudice reviews that marginally stay on a website's first page with those marginally pushed out due to unrelated activity on a job-search platform around the time of publication. The results show that when prospective employees learn about one instance of racial prejudice in a specific firm-city, foot-traffic drops by 3 to 4 percentage points in all stores located in the city. An event-plot analysis suggests that the effect is persistent.

A classical production model generates the paper's hypothesis and sheds light on the importance of different economic channels. The model considers that firms incur in monitoring costs to maintain a level of workplace safety, and consumers care about this safety level.[2] When workplace safety is initially low (high), making public a prejudice review has a large (small) negative impact on foot-traffic. Intuitively, a prejudice complain is seen as an isolated accident when companies have a safe workplace, but a systematic issue when workplace safety is low. The model also shows that, when workplace safety is low, consumer demand, instead of a labor supply adjustment, ex-

[1] An extensive literature in health economics, psychology, and public health has shown the individual perils of racial and sexual harassment in the workplace. Harassment leads to worse physical health (Krieger et al., 2008; Okechukwu et al., 2014), job satisfaction (Antecol and Cobb-Clark, 2009; Shields and Price, 2002), and mental health outcomes (Richman et al., 1999). It also leads to depression (Buchanan and Fitzgerald, 2008), and drug addiction (Chavez et al., 2015).

[2] A workplace is safe when the probability of a prejudice incident is low.

plains a larger fraction of the impact on foot-traffic. When workplace safety is low, firms spend less in monitoring costs and can adjust wages more aggressively.

Combining a randomized survey experiment with observational data, I examine the importance of each economic mechanism. Consistent with the model, the analysis shows that consumer demand explains a large fraction of the main findings of the paper, while an adjustment in labor supply has limited explanatory power. For example, a simple industry heterogeneity test shows that the main result of paper is prevalent in most industries (e.g., restaurants, clothing stores, and groceries) but not in the hotel industry. We should obverse an effect in hotels if the underlying mechanism is an adjustment of labor supply. However, a weak effect for hotels is consistent with a reduction in consumer demand of prospective employees, since most hotel customers are out-of-town visitors who do not consult job-search websites. When potential employees learn about racial prejudice incidents they reduce both labor supply and product demand, but the elasticity of store foot-traffic with respect to labor supply is small.

The randomized survey results also show that negative non-racial reviews—i..e., bad management, favoritism, or long hours—have no impact on consumer demand. Observational data also shows no effect on foot-traffic when prospective employees learn about reviews related to bad management. These findings suggest that racial prejudice has very different implications on firm performance than other workplace dysfunctions.

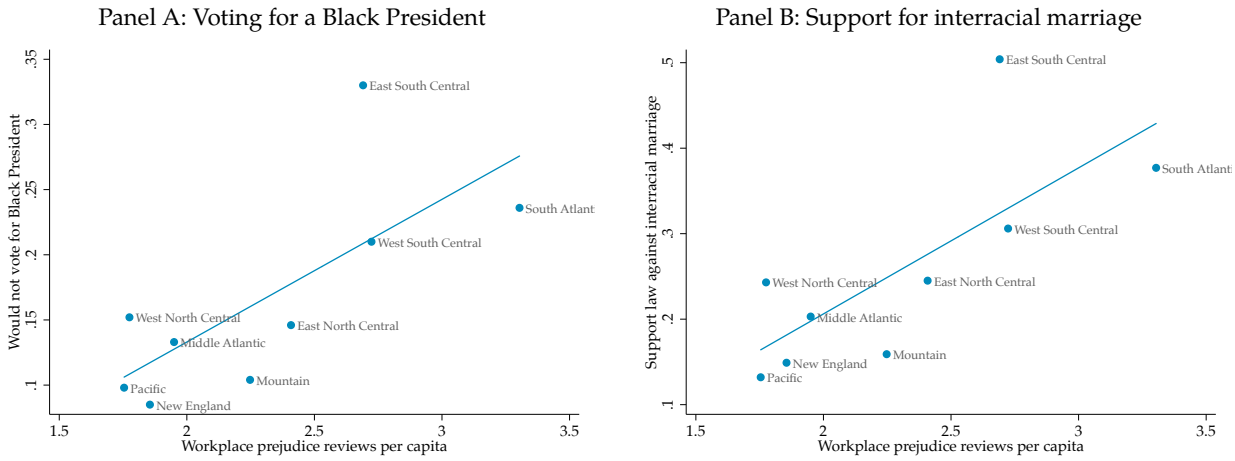
Workplace prejudice data is provided by Indeed.com. Among job search platforms, Indeed.com has the largest web traffic in the US since 2012. On October 2021, Indeed.com received over 270 million U.S. monthly visitors, a figure equivalent to Foxnews.com and NYtimes.com. The reports on racial prejudice are obtained through job reviews published on the website.[3] A review is published for a specific city where a company is located. I collect all reviews on Indeed.com for the 6,000 largest US firms with a storefront—i.e., McDonalds, Walmart, Homedepot, Chase, Marriott Hotel, AT&T, BP, Autozone, Walgreens, among others—and obtain a total of 7 million employer

[3]Indeed.com introduced an employer review system in 2012 to improve their user service. They ask job seekers to review their work experience at firms listed on their resumes. Each anonymous review contains a textual review, ratings on several aspects of a company, and some information about the reviewer. In the textual review, current and former employees may report any comment regarding their experience at the firm.

reviews from 2012 through 2021. Many companies have a large amount of reviews. For example, McDonald's and Walmart have each over 200,000 reviews. Firm performance is measured using foot traffic data from Safegraph from 2018 through 2021. These data uses GPS location of 18 million anonymized mobile phones representative of the US population, and is available for each U.S. store.

Prejudice reviews exist in every state, but there is a larger per capita incidence in southern U.S. states. I compare the per capita incidence of prejudice reviews with self-reported measures of prejudice introduced by Charles and Guryan (2008). Figure 1 shows that in locations where individuals are more likely to not vote for a Black President are also locations where we observe more incidents of workplace prejudice per capita. Furthermore, locations where more individuals support laws against interracial marriage are also locations with higher incident of workplace prejudice per capita. I also show in the data section that workplace prejudice reviews per capita is strongly negatively correlated with the Black-white wage gap. Census divisions with more workplace racial reviews per capita also have a large wage gap between black and white workers.

Figure 1: Workplace prejudice and self-reported measures of prejudice



Notes: The y-axis uses data from nine census divisions on the mean response among whites on two questions from the General Social Survey (GSS) about racial prejudice. The x-axis in both panels uses data on workplace prejudice reviews per 100,000 residents. Section 2 details the data on workplace prejudice reviews.

It is challenging to identify the hypothesis of the paper. For example, firms under distress might hire lower quality managers, which in turn, might lead to higher rates of workplace prejudice. Or firms with lower quality managers might have more instances of racial prejudice and simultaneously worse performance. To address these identification issues, I build on the conjecture that some reviews may receive more attention depending on when and where they are published.

I use two empirical designs to identify the paper's hypothesis. The first identification strategy exploits variation in weekly web traffic on Indeed.com by U.S. state. If a prejudice review is published on a week when web traffic on Indeed.com is above the monthly median traffic, it is likely that the review receives more attention. If differences in weekly web traffic are driven, for example, by TV advertisement by Indeed.com, a prejudice review might get more attention due to reasons that are exogenous to the outcomes of a firm-establishment. I use weekly google searches for Indeed.com by U.S. state to measure web traffic, and consider high-traffic weeks when the number of google searches is above the median monthly traffic across all U.S. states.

The second identification strategy focuses on a sharper discontinuity in visibility. Each firm in the sample has a page profile on Indeed.com. This profile page contains several company key facts, including the most recent 20 reviews for any location. I assume that these reviews receive the highest attention. Since several reviews (for any city/town) are posted every day, I use variation in the number of reviews that are published following the 24 hours after the racial prejudice report. I then compare performance of establishments in cities where racial reports marginally remain on the first page with those that are marginally pushed to the second page. The identification assumption is that individuals do not predict how many additional reviews from around the country are posted in the 24 hours following their workplace prejudice report.

The first analysis using weekly web traffic shows that firms experience a reduction of 3.1 to 3.5 percentage points in monthly visitors in all establishments in a city where a prejudice report is published. The second identification strategy documents a reduction of 2.8 to 3.6 percentage points in monthly visitors in all establishments in a city where a prejudice report is published.[4] The second

[4] A placebo test for the second empirical design with a false front page threshold bears no economic or statistically significant results.

design focuses on a smaller set of firms, while the first design uses all firms in the sample, yet both designs lead to remarkably similar point estimates. An event-plot analysis shows that (i) there are no pre-trends in foot-traffic between treatment and control firms, (ii) the reduction in foot-traffic occurs almost immediately after the publication, and (iii) the effect is likely permanent.

The model suggests that the effects are larger in firms in areas where workplace safety is low. I test this prediction using the self-reported measures of prejudice from Charles and Guryan (2008). I assume that workplace safety is low in states where level of self-reported prejudice is high. The results show that, after prospective employees learn about workplace prejudice, the effect on foot-traffic is almost as twice as large in states where the ex-ante self-reported level of prejudice is high.

To shed light on the underlying mechanisms, I first introduce a simple test based on industry heterogeneity. If the main finding stems from an adjustment in labor supply, we ought to observe the paper's hypothesis across all industries. Yet, I show that the effect of prejudice reviews on foot-traffic is prevalent in most industries (e.g., restaurants, clothing stores, and groceries) but not in the hotel industry. An adjustment in labor supply should lead to an effect in hotels. But since hotel customers are mainly out-of-town visitors who are unlikely to consult job-search websites, this evidence is instead consistent with a reduction in consumer demand of prospective employees.

I then combine a randomized survey experiment on Mturk with an analysis using observational data to provide further evidence of the underlying mechanism.[5] The experiment consists on showing job ads accompanied by job reviews to a representative sample of individuals. I select a sample of job ads from a sample of firms in my dataset and use real job reviews for those companies, but randomly select the reviews shown. All participants are shown two positive reviews, and a third review randomly picked from the following set: (i) review associated with an episode of racial prejudice, (ii) non-racial but very negative review, or (iii) another positive review about the company. I then ask several questions, including whether participants are interested in submitting a job application, and whether they would change their consumer behavior and boycott the firm.

The randomized survey results show several interesting patterns. First, awareness of a very negative non-racial review has no impact on consumer behavior. Second, awareness of a prejudice

[5] I confirm that the majority of survey participants are users of Indeed.com.

review increases the likelihood of consumer boycotting by 7.5 percentage points. This is an increase of over 90% relative to the mean. Third, this effect is largest for non-whites and females. Fourth, a racial review decreases the likelihood that a job seeker applies for a job by 12 percentage points. This is a decline of only 14% relative to the mean. Lastly, a negative non-racial review leads to a decline of 6 percentage points in job applications.

Supplemental results using observational data show that publishing job reviews on very negative management practices has no effect on foot-traffic. Combining this with survey results allow us to understand the importance of consumer demand and labor supply adjustments. Survey findings show that bad management reviews affect labor supply, but observational data shows that these reviews do not affect foot-traffic. It follows that the foot-traffic elasticity of labor supply must be small. While a reduction in labor supply is costly for firms, it produces no effect on foot-traffic in this time sample. This is also consistent with the almost immediate effect we observe in event plot analysis. A labor supply adjustment is likely to take longer to eventually affect store foot-traffic. Taken together, and consistent with the theory, the main results observed in the paper are driven by a reduction in consumer demand rather than an adjustment of labor supply.

This paper contributes to several literatures. First, it contributes to the literature that studies racial prejudice in labor markets (Charles and Guryan, 2008; Glover et al., 2017; Hedegaard and Tyran, 2018; Hjort, 2014; Rubinstein and Brenner, 2014). Charles and Guryan (2008) suggest that one-quarter of the racial wage gap is due to prejudice. Glover et al. (2017) shows that when minority cashiers are scheduled to work with managers who are biased, they are less productive. Hjort (2014) show that interethnic rivalries lower allocative efficiency. Barnes (2022) shows that employee morale declines after EEOC announcements of major discrimination cases. Others have shown the implications of prejudice in different markets (Alan et al., 2020; Card et al., 2008; Cutler et al., 1999; Fisman et al., 2017; Lowe, 2021; Schindler and Westcott, 2021). This paper shows that firms incur in a significant revenue loss when outsiders learn about prejudice practices in the workplace.

Second, an extensive literature in health economics, psychology, and public health has shown the individual perils of racial prejudice and harassment in the workplace. Harassment leads to

worse physical health (Krieger et al., 2008; Okechukwu et al., 2014), job satisfaction (Antecol and Cobb-Clark, 2009; Shields and Price, 2002), and mental health outcomes (Richman et al., 1999). Consistent with these findings, this paper shows that workers adjust their labor supply when they learn about prejudice in the workplace. More surprisingly, prospective employees exhibit sympathy to workers in harassment environments as they reduce their consumption of goods and services from these firms.

Third, the paper contributes to the literature on job culture and firm value (Gorton and Zentefis, 2020; Graham et al., 2017; Grennan, 2020; Guiso et al., 2015; Li et al., 2020; Lins et al., 2017; Martinez et al., 2015; Song and Thakor, 2019). This literature aims to understand how job culture impacts firm value. For example, Guiso et al. (2015) study which dimensions of corporate culture are related to a firm's performance. Martinez et al. (2015) examine the effect of management on culture and the effect of culture on performance. But corporate culture entails many attributes. I focus on racial prejudice and show that the effects on firm performance differ substantially from other job culture factors—-i.e., bad management, favoritism, or long-hours.

Fourth, the paper also relates to a newer literature on consumer boycotting (Besley and Ghatak, 2007; Broccardo et al., 2020; Gurun et al., 2020). Broccardo et al. (2020) considers voice (engagement) strategies in promoting socially desirable outcomes in companies. This paper adds to this literature by showing that prospective employees may boycott a firm when they learn about prejudice incidents. It also contributes to this literature by highlighting the value of online job-search platforms in diffusing information among prospective employees.

Lastly, the paper highlights the importance of (former) rank-and-file employees to firm value. A nascent literature in finance shows the impact of rank-and-file workers on firm value (Agrawal et al., 2021; Belo et al., 2017; Donangelo, 2014; Edmans, 2011; Hacamo and Kleiner, 2022). Since the majority of job reviews are posted by former employees, this paper shows that former employees might affect firm value by alleviating asymmetric information about workplace practices.

1 THEORY

When incidents of workplace prejudice become public information, existing and prospective employees learn that a firm might not be exerting enough effort to eliminate prejudice in the workplace. Potential employees might be discouraged to work for a firm with racial prejudice in the workplace, while existing employees may experience a disutility from working in such environment. Firms might then experience a smaller pool of applicants and a decline in productivity. Foot-traffic is affected when lower productivity affects quality, quantity, or price of products to final consumers. This mechanism is referred as the *labor supply channel* in the paper.

Job seekers and employees are also consumers themselves. Their consumption choices may affect demand and consequently firm outcomes. Workers may also disseminate information they learn on job-search websites to their family and friends. If preferences of these consumers are sensitive to discriminatory behavior in the workplace, firms may experience a sufficiently large drop in demand, which in turn, may affect firm revenues. This mechanism is referred as the *consumption demand channel* in the paper.

I introduce in the remainder of this section a model that embeds these two channels and generates the hypothesis of the paper. The framework augments a classical equilibrium production model with two simple features. First, firms incur in workplace monitoring costs to maintain a level of safety in the workplace that prevents racial prejudice. Second, consumers internalize firm's level of safety in the workplace in their consumption behavior. More concretely, consumers derive more utility value from one unit of consumption when goods are produced in low racial prejudice workplace environment. The model not only generates the hypothesis of the paper but also shows the parameter conditions in which the consumption demand channel dominates the labor supply channel, and vice versa.

1.1 Theoretical framework

Firms. Consider an economy with only one factor of production: labor l. Households supply labor and firms pay a competitive wage rate w per unit of labor. Racial prejudice might happen

in the workplace, but the incidence depends on the level of safety in the workplace. Consider a workplace safety parameter δ. The higher the safety parameter, the lower is the incidence of racial prejudice. To guarantee a safety level δ, firms must spend $v(\delta)$ for every dollar of the wage bill, where v is a convex function ($v' > 0$ and $v'' > 0$).[6] I assume that δ is arbitrarily determined by a regulatory government agency. In such environment, firms solve the following profit maximization problem:

$$\max_{\{l\}} \ \pi(l) = f(l) - wl - v(\delta)wl,$$

where f is increasing but exhibits diminishing returns to scale, that is $f' > 0$ and $f'' < 0$. The first order condition is given by: $f'(l^*) = [1 + v(\delta)]\, w$. Considering a Cobb-Douglas production function, $f(l) = Al^{\beta}$, yields the following optimal labor demand:

$$l^* = \left[\frac{A\beta}{w(1+v)}\right]^{\frac{1}{1-\beta}} \tag{1}$$

Consumers. There is one representative household that derives utility from consumption and leisure. This household must allocate T units of time between labor (L) and leisure (R). They also own any firm in the economy and hence receive all profits (π) derived by firms. In this setting, the representative household solves the following utility maximization problem:

$$\begin{aligned} &\max_{\{C,R\}} \ u(R, C, \delta) \\ &\text{s.t. } C \leq w \times (T - R) + \pi, \end{aligned}$$

where u is concave and strictly increasing in C and R, and accounts for the level of workplace safety. Consumers may "boycott" a firm with high likelihood of prejudice practices in the workplace. As such, the marginal utility of consumption is larger when goods are produced in a low workplace prejudice environment. Since the likelihood of a prejudice incident is strictly increasing

[6] The predictions of the model remain constant if we consider v per unit of labor (l). The model is however substantially easier to solve if we consider v per unit of the wage bill (wl).

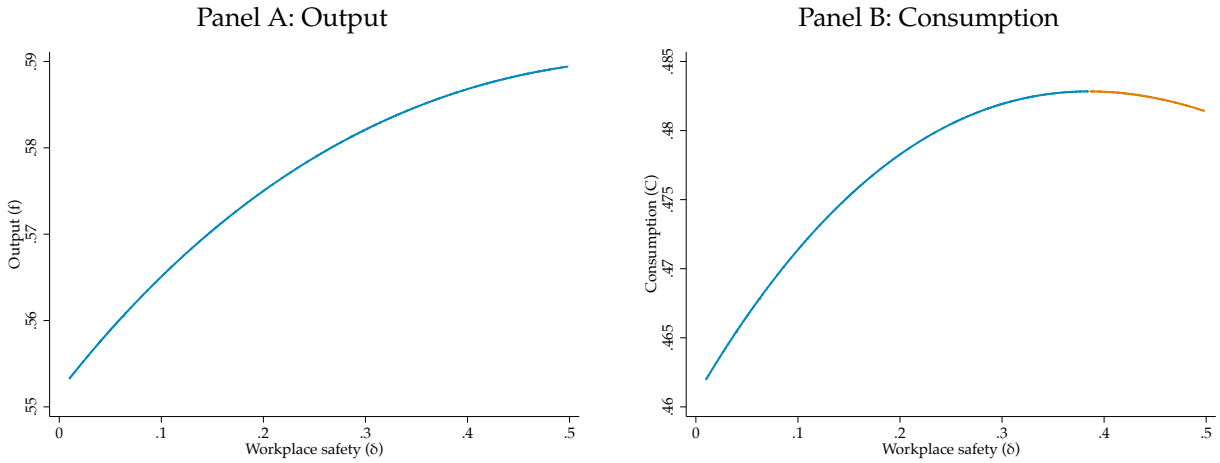
in δ, u is defined as follows:

$$u(C, R) = g(\delta)log(C) + log(R).$$

where g is concave and strictly increasing in δ ($g' > 0$ and $g'' < 0$). This utility function guarantees that households may "boycott" a firm (reduce consumption and labor supply) when the likelihood of prejudice increases. The concavity assumption ensures that increases in δ have a larger positive impact on consumer preferences and labor supply when safety in the workplace is initially low. Under these conditions, the first order condition yields the following optimal labor demand:

$$L^* = \frac{gTw - \pi}{w(1+g)} \tag{2}$$

Figure 2: Equilibrium dynamics as a function of workplace safety (δ)



Equilibrium. An equilibrium is defined by a wage rate w that clears the labor market ($L^* = l^*$):

$$w = \frac{A\beta}{1+v}\left[\frac{(1-\beta)(1+v)+\beta(1+g)}{\beta Tg}\right]^{1-\beta}. \tag{3}$$

With the equilibrium wage, we can derive optimal output, consumption, and labor. I examine an equilibrium when $\beta = 1/3$, $A = 1$, and $T = 1$. Figure 2 plots equilibrium output and consumption for different values of δ when $g(\delta) = log(\delta+1)+1$ and $v(\delta) = \delta^2+1$. I choose two simple functional forms for g and v, but the results are robust to more complex choices of g and v. Figure 2 shows that, due to the concavity of g, the rate of increase in consumption and output is larger when δ is small. In contrast, when δ is large, consumption and output may decline if v is too convex. Thus, the trade-off between monitoring costs and consumer preferences dictates whether a change in δ has a small or large impact on output.

Racial prejudice and foot-traffic. The publication of a prejudice review may be construed by workers as a marginal decline in δ, a reduction in the safety standards in the workplace. Further, for firms with a store front, output equates to foot-traffic. Thus, when safety in the workplace is initially low, the model shows that a publication of a prejudice review might cause to a large decline in foot-traffic. And when safety in the workplace is initially high, the impact on foot-traffic is small. Empirically, it is unclear whether foot-traffic increases or decreases after the publication of workplace prejudice review. This leads us to the main hypothesis of the paper:

Hypothesis 1: *The publication of a racial prejudice review on the job-search website leads to a significant decline in foot-traffic.*

Economic channels. Next, I analyze the importance of different economic channels. To this end, one may examine a deviation from an existing equilibrium after a marginal increase in labor supply or consumer demand. I follow this strategy. For a given equilibrium, I first estimate the marginal change in labor supply and consumer demand after a marginal change in δ. I then estimate the impact on output.

A marginal change in labor supply after a marginal change in δ, changes labor demand to $l^+ =$

$l^* + \frac{\partial L^*}{\partial \delta}$. And the implied percentage change in output equals:

$$\Delta f(\text{Labor supply}) = \frac{f(l^+) - f(l^*)}{f(l^*)} = \frac{f\left(l^* + \frac{\partial L^*}{\partial \delta}\right) - f(l^*)}{f(l^*)}. \tag{4}$$

On the other hand, since in equilibrium $C^* = wl^*$, a marginal change in consumer demand leads to an adjustment of labor demand of $l^{\#} = l^* + \frac{1}{w}\frac{\partial C^*}{\partial \delta}$. The percentage change in output is then equal to:

$$\Delta f(\text{Consumer Demand}) = \frac{f(l^{\#}) - f(l^*)}{f(l^*)} = \frac{f\left(l^* + \frac{1}{w}\frac{\partial C^*}{\partial \delta}\right) - f(l^*)}{f(l^*)} \tag{5}$$

Figure 3: Labor supply versus consumer demand

Notes: This graph reports the importance of the labor supply relative to the consumer demand channel. Each component is derived in (5).

Figure 3 depicts the ratio of $\Delta f(\text{Labor supply})$ to $\Delta f(\text{Consumer Demand})$, when $g(\delta) = log(\delta + 1) + 1$ and $v(\delta) = d^2 + 1$. The figure suggests that when workplace safety level is low, the impact on total output of consumer demand is significantly larger than that of labor supply. Labor supply only has a larger impact on output when workplace safety is relatively high. Intuitively, when workplace level is low, firms save in monitoring costs and can adjust wages more aggressively to incentivize workers to work despite their change in preferences. This leads

us to the main second hypothesis of the paper:

Hypothesis 2: *When workplace safety is low, consumer demand drives the effect on foot-traffic after prospective employees learn about a racial prejudice review.*

I test hypotheses 1 and 2 using the data and empirical design described in the next two sections.

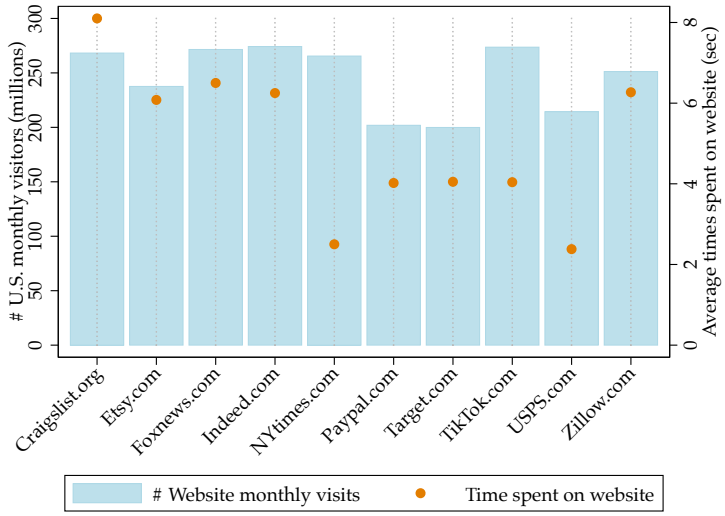
2 DATA

2.1 Racial prejudice data

Data on incidences of racial prejudice is scarce. Beyond large lawsuits and cases covered in news media, data available to prospective employees is limited. Researchers may measure workplace prejudice through anonymous complains (i.e., EEOC), but these data is not readily available to other market participants. Furthermore, since it is costly to sue employers or even to report complains to governmental institutions, prejudice and racial harassment is often underreported (Aguilar and Baek, 2020; Cheng and Hsiaw, forthcoming; Dahl and Knepper, 2021). These limitations prevent information regarding prejudice incidents to be quickly incorporated in firm value. But Indeed.com might have changed this landscape since it has started to provide these data for free to all market participants.

Indeed.com aggregates job listings from thousands of websites and offers a resume repository service that allows job seekers to easily apply for jobs. Indeed.com is the highest-traffic job-search website in the United States since 2010. Figure 4 compares the web-traffic on Indeed.com with other prominent websites. Web-traffic on Indeed.com is similar to Foxnews.com, NYtimes.com, or Craiglist.org; and is larger than Paypal.com, Zillow.com, or Target.com. In October 2021, Indeed.com received over 270 million monthly U.S. visitors. Indeed.com is not only the most important job-search platform in the U.S., but also generates a substantial amount of web-traffic. Information published on their website is likely to receive a large amount of attention.

Figure 4: Web traffic on Indeed.com and other prominent websites



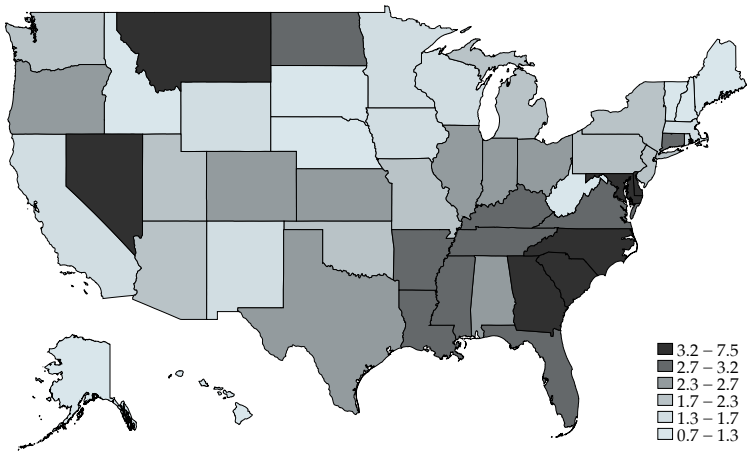
Notes: The bar column depicts the number of website visits originated from the US. The dot plot shows the average time spent on each website. Both data charts report data for October 2021. Data is from from Similarweb.com.

Indeed.com introduced an employer review system in 2012. Each anonymous review contains a textual review, ratings on several aspects of a company, and some information about the reviewer. A review contains a reviewer's occupation, location, and employment status, and a date. Reviewers may also rate a company with an overall rating, and five additional dimensions: job work/life balance; compensation/benefits; job security/advancement; management; and job culture. Ratings may vary from one to five stars. In the textual review, current and former employees report any comment regarding their experience at the firm.

I collected all reviews on Indeed.com for the 6,000 largest US firms with a storefront—i.e., McDonalds, Walmart, Home Depot, Chase, Bank of America, Marriott Hotel, AT&T, BP, AutoZone, Walgreens, among others—and obtained a total of 7 million employer reviews. Most companies have a large amount of reviews. For example, McDonald's and Walmart have each over 200,000 reviews. Approximately 70% of reviewers are former employees, and 50% of the reviews were posted after 2018. Most reviews are positive, but over 20% are negative ones.

Identifying racial prejudice. I parse all negative reviews (two-stars or less) to find those associated with racial prejudice. The process involves three steps. First, I search for words associated with racial prejudice in the textual review content (i.e., racist, racism, harassment, discrimination, slur, prejudice, etc). The full list of words is reported in the Internet Appendix. More than 80% of tagged reviews contain one of four words: racist, racism, discrimination, or racial. Reading some of the tagged reviews shows that some are clearly associated with racial prejudice while others are not. For example, the word "racist" almost always pins down a racial prejudice complain, while the word "discrimination" leads to several instances unrelated to prejudice.

Figure 5: Geographical distribution of racial prejudice reports



Notes: This figure maps the ratio of number of reviews per 100,000 residents for each state in the US.

Second, I search among tagged reviews in the first step for additional words (i.e. minority, race, Hispanic, African, etc). This second list of words is also reported in the Internet Appendix. In some cases, I impose the second word to be in same sentence. For example, "discrimination" and "black" need to be in same sentence. This criteria is specified in the internet appendix. The first and second step lead to almost 12,000 reviews associated with racial prejudice. Lastly, a team of trained research assistants verified every review in the final list and excluded misclassified reviews. This process identifies over 10,000 episodes of racial prejudice in the workplace between 2012 and 2021.

Figure 5 depicts the distribution across U.S. states of racial reviews per 100,000 residents. The

lowest states have around 0.7 prejudice incidents reviews per capita, while the highest states have over 4 or more racial incidents per capita during the sample period. The two states with the lowest number of racial of reviews per capita are Hawaii and Maine, and the ones with the highest number of prejudice reviews per capita are Georgia and Nevada.

Table A.1 reports the top 10 firms with the highest incidence of racial reviews in the sample. Walmart, MacDonald's, and Target have the highest number of incidents. Wendy's, Lowe's, and CVS have the lowest number of incidents among the top 10. There is a large variation across firms. For example, McDonald's has 2.2 reported prejudice incidents per 100 stores, while Burger King only has 1.2 reported incidents per 100 stores during the sample period.[7] Table 1 tests this conjecture. It regresses the incidence of prejudice in the workplace on a constant and different levels of fixed effects. The regression sample includes the sample of firms with a complain of workplace prejudice and all other firms in the same county and 6-digit NAICS industry. Firm fixed effects explain 28.43% of the variation of prejudice incidents, while county or industry fixed effects only explain 8.79% and 3.25% of the variation, respectively. This is consistent with recent evidence in the literature (Kline et al., 2021).

Table 1: Variation of workplace prejudice

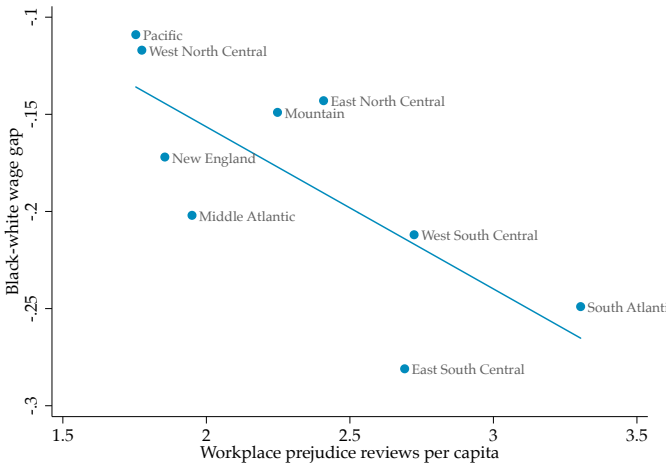
	Workplace Prejudice Report		
	Industry	Geography	Firm
R^2	8.79%	3.25%	28.43%
Industry FE	Yes	No	No
County FE	No	Yes	No
Firm FE	No	No	Yes

I confirm that the fraction of prejudice reviews per capita correlates with the measures of self-reported prejudice introduced by Charles and Guryan (2008). Figure 1, Panel A, shows that in locations where individuals are less likely to not vote for a Black President are also locations where we observe more incidents of workplace prejudice per capita. Figure 1, Panel B, shows that in

[7] These figures are based on the number of establishments estimated from ScrapeHero.com. McDonald's and Burger King have 13,237 and 7.257 locations, respectively.

locations where more individuals support laws against interracial marriage are also locations with higher incident of workplace prejudice per capita.

Figure 6: Workplace prejudice and Black-white wage gap



Notes: The y-axis uses data from nine census divisions on the Black-white wage gap. This data was directly obtained from Charles and Guryan (2008). The x-axis in both panels uses data on workplace prejudice reviews per 100,000 residents.

Figure 6 shows that racial reviews per capita correlate negatively with the Black-white wage gap. Census divisions where there are more racial reviews per capita are also those with high wage gap between blacks and whites.

2.2 Foot-traffic data

I measure firm performance with foot traffic data provided by Safegraph. These data uses GPS location of 18 million anonymized mobile phones representative of the US population, and is available for each store in the US. The dataset starts in January 2018 and provides the number of daily visitors, including stores visited before and after, and the census tract of the visitor's residence. Data is available for every single establishment for the 6,000 largest U.S. brands.

2.3 Summary statistics

Table 2 reports main summary statistics of the dataset. The average firm has almost 10 stores in a county, and each store receives on average 440 unique monthly visitors. The average monthly growth of visitors is 5.8%. Over 7% of the firms in the sample are in the hotel industry, 4% are clothing stores, almost 7% are groceries stores, and 27% are restaurants.

Table 2: Summary statistics of observational data

	N	Mean	Std	10th	50th	90th
Number establishments per county	154512	9.82	22.1	1	3	24
Monthly #visitors	154512	439.8	577.1	21.3	228.9	1197
Monthly growth #visitors	150491	0.058	0.34	-0.23	0.015	0.34
Google search index	154512	41.8	15.8	25	40	61
High self-reported prejudice	154512	0.48	0.50	0	0	1
Hotel	154512	0.075	0.26	0	0	0
Clothing store	154512	0.041	0.20	0	0	0
Grocery	154512	0.069	0.25	0	0	0
Restaurant	154512	0.27	0.45	0	0	1

Notes: Public racial report is equal to one for a firm-county where a prejudice report was filed on Indeed.com. Number establishments in county is the total number of establishments for a given firm in a given county. Monthly visitors per establishment is the total number of monthly unique visitors per establishment.

2.4 Survey data

Data for the randomized survey is obtained on Amazon Mechanical Turk (MTurk)—Section 3.3 describes the survey design. MTurk is an web-based platform that allows requesters to post small tasks (HITs) to be performed by humans. Potential workers browse through postings and choose whether to complete a task for the offered price. Mturk has been widely used among economists to conduct surveys and experiments (De Quidt et al., 2018; DellaVigna and Pope, 2018; Fisman et al., 2020; Kuziemko et al., 2015). I choose participants located in the US, with at least 1,000 completed jobs, and an approval rate higher than 95%.

Each participant was paid to answer 10 questions, including demographic queries. The sur-

vey took 3-4 minutes to complete. Regarding demographical data, I asked participants about their employment status, gender, race, ethnicity, age, and household income—the specific questions are available in the Internet Appendix. After filtering responses that did not complete successfully the re-captcha, the final sample contains 2217 unique participants. Table 3 summarizes the characteristics of the participants: 45% are female, 69% are white, 10% are black, and 14% are hispanic. The average participant age is 39.6. And over 34% of participants live in a household that makes more than $75,000 income.

Table 3: Summary statistics of survey data

	N	Mean	Std	10th	50th	90th
Female	2192	0.45	0.50	0	0	1
White (non-hispanic)	2217	0.69	0.46	0	1	1
Black	2217	0.098	0.30	0	0	0
Hispanic	2217	0.14	0.34	0	0	1
Age	2217	39.6	10.3	28	37	56
Household income $\geq$ 75k	2217	0.34	0.47	0	0	1
Consumer boycotting	2217	0.080	0.27	0	0	0
Apply for job	2217	0.87	0.34	0	1	1

Notes: This table reports the summary statistics of the survey data.

3 EMPIRICAL DESIGN

It is challenging to identify the causal effect of reporting instances of workplace prejudice on firm performance. Omitted variables and reverse causality can contaminate a naive regression of firm performance on a measure of reported prejudice incidents. This is because underperforming firms might have worse job culture, which in turn, might lead to higher number of reported episodes of racial prejudice. Or firms with low quality managers might have more instances of reported prejudice and simultaneously worse performance.

To address these identification issues, I introduce two empirical designs that exploit plausi-

ble exogenous variation on visibility. The first specification exploits variation on web traffic on Indeed.com in the week when a review for a firm is published. While the second specification exploits a discontinuity on whether reviews stay on the first or second page of the website. The first design uses the whole sample of firms, while the second design focuses on a specific set of firms that are at the margin of the discontinuity.

3.1 Design 1: Weekly web traffic on Indeed.com

Web traffic on Indeed.com varies by week of the month and location due to factors that are likely exogenous to a specific firm-establishment. For example, TV and online ad advertisement are large drivers of website traffic (Liaukonyte et al., 2015). Advertisement spending by Indeed.com is unlikely related to firm-establishment outcomes, except through the visibility that may provide to specific reviews. Leveraging on this variation, I estimate a model that compares the impact on establishment foot-traffic of reviews that are published on weeks that have web traffic above the monthly median with those below the median. More concretely, I estimate the following regression model firm i in county c and month t:

$$\begin{aligned} \text{Y}_{ict} &= \beta \times \text{Post}_t \times \text{High weekly web traffic}_{ic} \\ &+ \theta \times \text{Controls}_{ict} + A_{it} + B_{ic} + \varepsilon_{ict} \end{aligned} \tag{6}$$

where *High weekly web traffic*$_{ic}$ equals one when weekly web traffic for Indeed.com in the U.S. state of county c is above the monthly median across all U.S. states. *High weekly web traffic*$_{ic}$ is measured on the week when the racial prejudice review is published, and web traffic is proxied by the search volume index from Google Trends. Errors are clustered at the county level. The main hypothesis implies that $\beta > 0$.

3.2 Design 2: Discontinuity in attention

Each firm has a page profile on Indeed.com. The firm's profile contains a snapshot of the firm. A prospective employee may learn about job reviews of current and former employees for any

establishment in the *Reviews* tab. Figure A.1 provides an example for Walmart's reviews tab in November 2019. Here an employee can see the history of all Walmart's reviews for any location. Reviews are order chronologically, from the latest to the oldest. The first page shows the 20 most recent reviews.[8] An example of a review is provided in Figure A.2.

To identify the main hypothesis, the paper (i) builds on the conjecture that reviews on the first page receive disproportional attention, and (ii) exploits exogenous reasons why some reviews remain in the first page longer than others. Specifically, it uses variation in the number of reviews that are published for a firm in the following 24 hours after the workplace prejudice report in a firm-city. And compares the impact on firm performance of racial reports that marginally remain on a first page with those that are marginally pushed to a second page. Since there is a delay between submission and release of reviews—Indeed.com verifies reviews prior to posting—some reviews might stay for a significantly long period on the first page, and thus receive more attention. The identification assumption is that individuals do not predict how many additional reviews are posted country-wide for a firm in the 24 hours following their prejudice report.

The identification is best described with an example. Consider that today a workplace prejudice report was filled for McDonald's and Burger King in Bloomington. Supposedly, these two establishments have an equal likelihood of prejudice in the workplace. But if the post-publication flux of country-wide reviews is larger for McDonald's than Burger King, Burger King's prejudice review in Bloomington might then get more visibility than that for McDonald's. Let's assume that the total volume of reviews this month is equal for both establishments, but in the 24h following the racial review, 17 other reviews were submitted (country-wide) for Burger King and 23 for McDonald's. Then, McDonald's review might almost immediately move to the second page, while Burger King's incident is visible for several additional hours or days. For this visibility to have a bite it is important that the number of daily visitors to Indeed's website is large. As argued above, Indeed.com receives almost 10 million website visits per day.[9] It is then very plausible that these differences in visibility might have large consequences on the number of job seekers who see these reviews.

[8] Around late 2020, Indeed.com started to feature one review, and sub-selecting the reviews on the location where the job seeker's is accessing internet. Figure A.1 shows that in November 2019 none of these features was available.

[9] The latest web traffic information may be consulted here: https://www.similarweb.com/website/indeed.com/

Firms that receive between $19-\Delta$ and 19 reviews country-wide in the 24 hours after the prejudice review are considered treated. Those that receive between 21 and $21+\Delta$ reviews country-wide post publication are considered control firms. Δ varies between 3 and 7. That is, in the most restrictive specification, treatment firms receive between 16 and 19 reviews, and controls firms receive between 21 and 24 reviews in the 24 hours post publication of the prejudice report. I use a range of values for Δ since there is a trade-off. A low Δ guarantees more comparability, while a high Δ leads treated reviews to stay longer on the first page. A large Δ also increases the sample size. Under these conditions, I estimate the following regression specification (firm i, county c, month t):

$$\begin{aligned} \text{Y}_{ict} &= \beta_1 \times \text{Post}_t \times \text{First Page}_{ic}^{\Delta} \\ &+ \theta \times \text{Controls}_{ict} + A_{it} + B_{ic} + \varepsilon_{ict} \end{aligned} \tag{7}$$

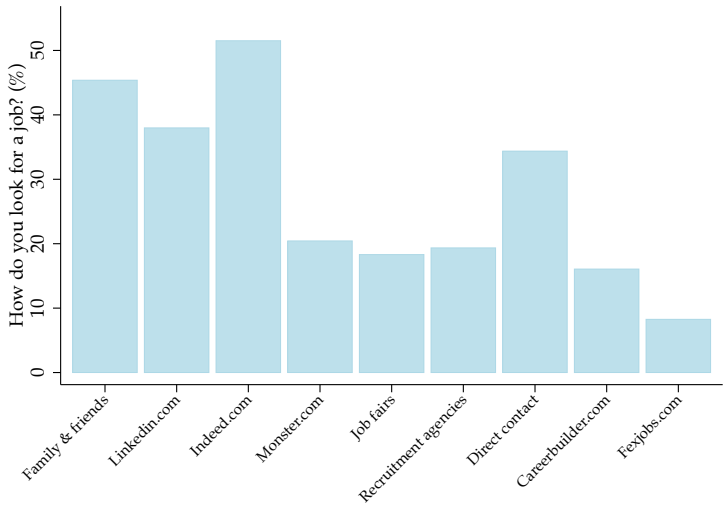
where *First page*$^{\Delta}$ is a binary variable equal to one if only $19-\Delta$ to 19 reviews were published on the same or next day, and zero if 21 to $21+\Delta$ reviews were published in the following 24 hours post publication of the racial prejudice review. This flow of reviews stems from other establishments of the firm. Δ varies between 3 and 7. This regression is estimated on the sample of firms for which at least one prejudice review was published. I only consider the first racial prejudice review that was published for a firm. Errors are clustered at county level but robust to other specifications as described in the Internet appendix. The main hypothesis of the paper implies that $\beta_1 > 0$.

3.3 Randomized survey experiment

While the above regression specifications allow the estimation of a causal effect, they do not pin down the economic mechanism that links reporting a prejudice episode to store foot-traffic. When workplace prejudice episodes become public information among job seekers, firm performance might be affected through two different channels. First, potential employees might lower the supply of labor to targeted firms. Second, job seekers, who are consumers themselves, may reduce consumer demand, and may also disseminate information to other consumers in their personal

networks. If awareness of prejudice in the workplace affects preferences of these individuals, consumer demand may also be affected. To identify these mechanisms, I conduct a randomized survey experiment with participants that are likely users of Indeed.com. The experiment consists on showing job ads accompanied by job reviews to a representative sample of individuals to understand their job and consumer preferences.

Figure 7: Most popular platforms to find a job



Notes: This chart summarizes the survey response to the question "How do you usually look for a job?". Respondents were allowed to choose multiple choices.

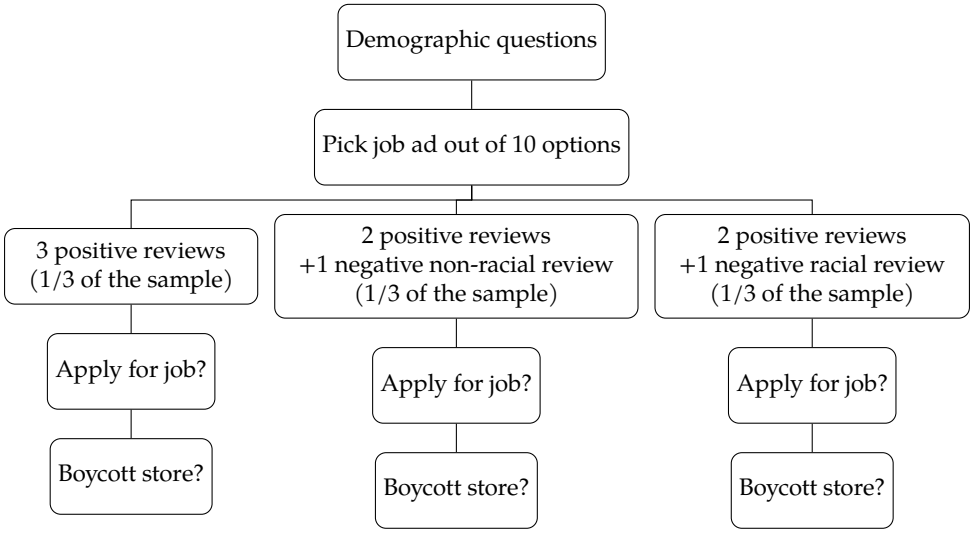
The randomize survey is structured as follows. First, I ask a few demographic questions, which complement the information provided by Mturk. Second, I confirm if participants use Indeed.com. Whether users are familiar with Indeed.com is not critical for this part of the study, since we only want to understand the behavior of job seeker. Figure 7 shows that 50% of the survey participants are Indeed.com users. Indeed.com is the most popular job-search website among survey participants.[10]

I present participants with 10 different job ads titles and ask them to pick one ad. Job ads are selected for firms that are included in the dataset. I use real job reviews for these companies, but

[10]There are no differences between users and non-users of Indeed.com.

randomly select the reviews shown. All participants are shown two positive reviews, and a third review randomly picked from the following set: (i) review associated with an episode of prejudice, (ii) non-racial negative review, or (iii) positive review about the company. Figures A.3 and A.4 in the Internet Appendix provide examples of racial and negative non-racial reviews. I then ask several questions. Including, whether they are interested in submitting an application to the job post. And whether they think they would change their consumer behavior. The Internet appendix details all survey questions. Figure 8 summarizes the survey dynamics.

Figure 8: Survey design



Notes: This chart summarizes the survey design.

4 RESULTS

4.1 The effect of reporting racial prejudice episodes

Design 1: Weekly web traffic on Indeed.com. I first present the estimates of the model detailed in section 3.1. The model relies on the assumption that web traffic on Indeed.com varies by week of the month and location due to factors that are likely exogenous to outcomes of a specific firm-establishment. The empirical specification compares the impact on foot-traffic per establishment of reviews published on weeks with web traffic above the monthly median with those published on weeks with web traffic below the monthly median. Table 4 reports the estimated coefficients of this regression model. The first column only includes firm-year-month and firm-county fixed effects. The second column adds controls for the number of establishments per county and growth in number of visitors in the past month. And the third column includes industry-year fixed effects.

Table 4: Racial prejudice and foot-traffic: Design 1

	Monthly Visitors		
	(1)	(2)	(3)
High web-traffic week × Post	-15.508***	-13.594**	-13.559***
	(-2.80)	(-2.53)	(-2.88)
Firm × County FE	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes
Industry × Year	No	No	Yes
Controls	No	Yes	Yes
N	154,512	154,356	154,355
R-squared	0.925	0.927	0.936

Notes: This table reports the estimates of regression model (6). The outcome variable is the number of monthly visitors. Post is a variable equal to one after the racial prejudice report becomes public. *High weekly web traffic*$_{ic}$ equals one when weekly web traffic for Indeed.com in the U.S. state of county c is above the monthly median across all U.S. states. *High weekly web traffic*$_{ic}$ is measured on the week when the racial prejudice review is published, and web traffic is proxied by the search volume index from Google Trends. Number establishments in county is the total number of establishments for a given firm in a given county. Errors are clustered at the county level. t-statistics are in parentheses. Statistic significance: ***=1%; **=5%; and *=10%.

The point estimates vary from -13.6 to -15.5. The full model shows that, relative to the mean, reporting one episode of prejudice leads to a decline of 3.1 percentage points (=13.6/440) in monthly visitors for all establishments in the city where the review was published. This effect is significant at the 1% percent level. These findings suggest a significant drop in revenue after prospective

employees learn about a racial prejudice incident.

Design 2: Discontinuity in visibility Section 3.2 introduces a second empirical design that exploits plausible exogenous variation in the degree in which prospective employees learn about a racial prejudice incident. The design uses variation in the number of reviews that are published in the following 24 hours after the racial prejudice report. It compares the impact on firm performance of racial reports that marginally remain on a firm profile webpage with those that are marginally pushed to a second webpage.

Table 5: Racial prejudice and foot-traffic: Design 2

	Monthly Visitors				
	(1) $\Delta = 3$	(2) $\Delta = 4$	(3) $\Delta = 5$	(4) $\Delta = 6$	(5) $\Delta = 7$
Post × First Page$^{\Delta}$	-15.225*	-12.495	-13.366*	-15.991**	-14.246**
	(-1.67)	(-1.40)	(-1.73)	(-2.32)	(-2.28)
Firm × County FE	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes
Industry × Year	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
$\bar{Y}$	436.1	452.2	438.3	443.3	446.5
N	14,255	17,241	20,487	24,599	28,206
R-squared	0.936	0.935	0.933	0.934	0.936

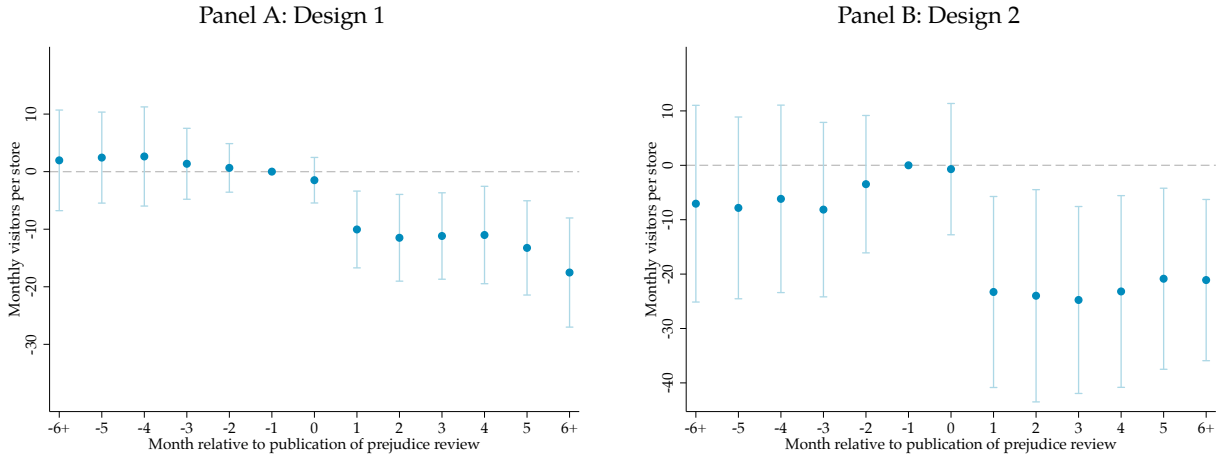
Notes: This table reports the estimates of regression model (7). Firms that receive between $19 - \Delta$ and 19 reviews in the 24 hours after the racial prejudice review are considered treated. Those that receive between 21 and $21 + \Delta$ are considered control firms. Δ varies between 3 and 7. *First Page* equals 1 for treatment firms and zero otherwise. *Post* is a variable equal to one after the racial prejudice report becomes public. The outcome variable is the number of monthly visitors. The sample is restricted to firm-counties for which a racial prejudice report was filed on Indeed.com. Controls include: (i) *Number establishments in county*, (ii) *Number of monthly reviews at the firm level*, and (iii) *Annual monthly visitors at the firm-city level*. Errors are clustered at the county level. t-statistics are in parentheses. Statistic significance: ***=1%; **=5%; and *=10%.

Treated firms are those that receive between $19 - \Delta$ and 19 reviews in the 24 hours after the racial prejudice review is published, while control firms are those receiving between 21 and $21 + \Delta$ reviews post publication. Δ varies between 3 and 7. That is, in the most restrictive specification, treatment firms receive between 16 and 19 reviews, and controls firms receive between 21 and 24 reviews in the 24 hours post publication of the prejudice report. Table 5 reports the estimates of this model. All regression models include year-month, firm-county, and industry-year fixed effects, and control variables for number of establishments per county, monthly number of reviews that a

firm receives, and annual level of foot-traffic per firm-county.

The point estimates vary between -12.495 and -15.991. The effects are statistically significant at the 10% percent level when Δ equals 3 and 5, and 5% percent level when Δ equals 6 and 7. The estimates imply that awareness of one prejudice report reduces number of visitors by 2.8 (=12.495/452.2) to 3.6 (=15.225/436.1) percentage points in the months following publication.[11] These point estimates are remarkably close to those estimated in the previous section. These findings highlight the importance that online job-search platforms have on disseminating information about racial prejudice practices in the workplace.

Figure 9: Event plot analysis



Notes: This figure provides a test to the parallel trends assumption. It reports the point estimates β_m coefficients from model (10), including the 95% confidence intervals.

Event-study plot and pre-trends. I then test the parallel trends assumption with an event plot analysis for both empirical designs. Figure 9 reports these tests. Panel A depicts the estimates and 95% confidence intervals of the following model:

$$\begin{aligned} Y_{ict} &= \sum_{m=-6}^{6} \beta_m \times \text{Post}_{t,m} \times \text{High weekly web traffic}_{ic} \\ &+ \theta \times \text{Controls}_{ict} + \Gamma_{it} + \Psi_{ic} + \varepsilon_{ict} \end{aligned} \tag{8}$$

[11] The average number of visitors per store varies with Δ because the sample of firms varies with Δ.

and Panel B reports the estimates of the following model:

$$\begin{aligned} \text{Y}_{ict} &= \sum_{m=-6}^{6} \beta_m \times \text{Post}_{t,m} \times \text{First Page}_{ic}^{\Delta} \\ &+ \theta \times \text{Controls}_{ict} + \Gamma_{it} + \Psi_{ic} + \varepsilon_{ict} \end{aligned} \tag{9}$$

where m refers to the month relative to the month when the racial prejudice becomes public. $\text{Post}_{t,m=-6}$ and $\text{Post}_{t,m=6}$ are binary dummies that equal one for all months prior and after to $m = -6$ and $m = 6$, respectively. All other $\text{Post}_{t,m}$ equal one for month m and zero otherwise. The outcome variable is the level of foot-traffic instead of the growth rate. The second model is estimated for $\Delta = 7$.

The event plots show several results. First, there are no pre-trends prior to the month when racial prejudice reviews are published. The Wald test fails to reject that the coefficients prior to publication are equal to zero. Second, the even-plot rejects a time trend confounding factor. Third, the decline in foot-traffic is concentrated in first month post-publication. Fourth, the effect does not revert back, suggesting that this drop in foot traffic is permanent. Lastly, the estimates are more precisely estimated in model 1 since it uses a larger sample. Taken together, these findings suggest that there are large long-run negative implications for firms that tolerate prejudice behavior in the workplace.

Placebo test for Design 2. The second empirical design hinges on exploiting the differences in foot-traffic between firms that receive around 20 reviews after the publication of the racial prejudice report. One could potentially argue that this threshold still captures a systematic difference between firms. To alleviate these concerns, I introduce a placebo test. Instead of using the threshold between the first and second page, I use threshold of 10 and 30 reviews, which are located in the middle of the first and second page, respectively. The first placebo test defines treatment firms as those that receive between $9 - \Delta$ and 9, and control firms as those that receive between 11 and $11 + \Delta$ reviews within one day after the reported prejudice review. The second placebo test switches the treatment cutoff to 29 and the control cutoff to 31.

These estimation should not produce significant results since it is unlikely that there is a meaningful difference in visibility between racial reports that are in the middle of the first or second page.

Table 6: Placebo test

	Monthly Visitors					
	Threshold at 10			Threshold at 30		
	(1) $\Delta = 3$	(2) $\Delta = 4$	(3) $\Delta = 5$	(4) $\Delta = 3$	(5) $\Delta = 4$	(6) $\Delta = 5$
Post × Treatment $^{\Delta}$	4.817	5.865	4.502	-7.798	-2.489	-8.943
	(0.80)	(1.12)	(0.92)	(-0.83)	(-0.27)	(-1.03)
Firm × County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	33,061	42,292	54,208	6,814	8,391	10,217
R-squared	0.935	0.932	0.930	0.932	0.934	0.933

Notes: This table reports a placebo test based on the regression model (7). Instead of using a threshold at 20 reviews, the model uses 10 and 30. Δ varies between 3 and 5. *Post* is a variable equal to one after the racial prejudice report becomes public. The outcome variable is the number of monthly visitors. The sample is restricted to firm-counties for which a racial prejudice report was filed on Indeed.com. Controls include: (i) *Number establishments in county*, (ii) *Number of monthly reviews at the firm level*, and (iii) *Annual monthly visitors at the firm-city level*. Errors are clustered at the county level. t-statistics are in parentheses. Statistic significance: ***=1%; **=5%; and *=10%.

Table 6 reports the point estimates of this placebo tests for Δ between 3 and 5. The results show that there are no differences around the 10 and 30 review cutoff. For all Δ, the effect is economically small and statistically insignificant. Some point estimates are actually positive. These results confirm that there is a significant difference between being in the first versus second page. They also provide a validation to the empirical design.

Are effects larger in states with more prejudice? The model suggests that the effects are larger in firms or areas where workplace safety is low. I test this prediction using data on the level of prejudice by U.S. state. I assume that in states where level of prejudice is high the level of workplace safety is low. Level of prejudice is based on two questions from General Social Survey (GSS) about racial prejudice that were used in Charles and Guryan (2008). The first question asks survey participants whether they would not vote for a Black President, and the second question asks participants whether they support a law against interracial marriage. I define high prejudice states as

those in census divisions with the highest scores in these questions. This set of states includes Alabama, District of Columbia, Florida, Georgia, Kentucky, Maryland, Mississippi, North Carolina, South Carolina, Tennessee, Virginia, and West Virginia.

Table 7: Low vs high prejudice areas

	Monthly Visitors Low Prejudice Areas			Monthly Visitors High Prejudice Areas		
	(1) $\Delta = 5$	(2) $\Delta = 6$	(3) $\Delta = 7$	(4) $\Delta = 5$	(5) $\Delta = 6$	(6) $\Delta = 7$
Post × Non-busy day	-7.755	-11.924	-13.202	-21.277*	-22.663**	-16.099*
	(-0.77)	(-1.28)	(-1.58)	(-1.83)	(-2.24)	(-1.73)
Firm × County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	10,265	12,700	14,622	10,222	11,899	13,584
R-squared	0.939	0.937	0.935	0.929	0.932	0.937

Notes: This table reports the estimates of regression model (7). High prejudice areas are census divisions with the highest scores in the prejudice questions administered by the GSS. Firms that receive between $19 - \Delta$ and 19 reviews in the 24 hours after the racial prejudice review are considered treated. Those that receive between 21 and $21 + \Delta$ are considered control firms. Δ varies between 5 and 7. *First Page* equals 1 for treatment firms and zero otherwise. *Post* is a variable equal to one after the racial prejudice report becomes public. The outcome variable is the number of monthly visitors. The sample is restricted to firm-counties for which a racial prejudice report was filed on Indeed.com. Controls include: (i) *Number establishments in county*, (ii) *Number of monthly reviews at the firm level*, and (iii) *Annual monthly visitors at the firm-city level*. Errors are clustered at the county level. t-statistics are in parentheses. Statistic significance: ***=1%; **=5%; and *=10%.

I estimate model (7) in these two set of states and report results in Table 7. The estimates show that, after prospective employees learn about workplace prejudice incidents, the effect on foot-traffic is larger in states where the ex-ante level of prejudice is high. The point estimates are almost as twice as large in high prejudice states. This is consistent with the premise of the model that effects on total output ought to be larger in firms in which the level workplace safety is low.

4.2 Are results similar for bad management reviews?

Are the effects of racial reviews on foot-traffic similar to those of other workplace dysfunctions, such as bad management, long hours, or favoritism? Understanding the difference between these effects helps assess the importance of prejudice in workplace versus other bad culture issues in the workplace. To shed light on these differences, I re-estimate model (7) when prospective employees

learn about very bad management practices in the workplace. I identify reviews associated with bad management practices as those that receive a rating of 1 or 2 stars and mention the phrase 'bad manager'. Table 8 reports the estimates of this estimation. The results show that there is no effect on foot-traffic when prospective employees learn about bad management reviews. The point estimates are statistically and economically zero. These findings show that prejudice in the workplace has an impact on firm performance that is different than other workplace culture issues.

Table 8: Effect of bad management

	Monthly Visitors		
	(1)	(2)	(3)
	$\Delta = 3$	$\Delta = 4$	$\Delta = 5$
Post $\times$ First Page$^{\Delta}$	-1.694	-5.315	-4.144
	(-0.23)	(-0.80)	(-0.74)
Firm $\times$ County FE	Yes	Yes	Yes
Year $\times$ Month FE	Yes	Yes	Yes
Industry $\times$ Year	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	24,401	31,351	39,206
R-squared	0.931	0.932	0.931

Notes: This table reports the estimates of regression model (7). Firms that receive between $19 - \Delta$ and 19 reviews in the 24 hours after the bad management review are considered treated. Those that receive between 21 and $21 + \Delta$ are considered control firms. Δ varies between 3 and 5. *First Page* equals 1 for treatment firms and zero otherwise. *Post* is a variable equal to one after the bad management report becomes public. The outcome variable is the number of monthly visitors. The sample is restricted to firm-counties for which a bad management report was filed on Indeed.com. Controls include: (i) *Number establishments in county*, (ii) *Number of monthly reviews at the firm level*, and (iii) *Annual monthly visitors at the firm-city level*. Errors are clustered at the county level. t-statistics are in parentheses. Statistic significance: ***=1%; **=5%; and *=10%.

4.3 Identifying the mechanism

The findings in prior sections are consistent with the first hypothesis of the paper. The publication of a prejudice review has a large effect on foot-traffic. It follows from the model that the level safety in the workplace must be low. Under this hypothesis, the effect should be driven by consumer demand instead of an adjustment in labor supply. This section tests this prediction. I first introduce a simple test based on industry heterogeneity and then evidence from a randomized survey experiment.

Industry heterogeneity evidence I start with the premise that if the main finding in the paper stems from an adjustment in labor supply, we ought to observe an effect on all firms regardless if the costumer base if local or out-of-town. I assume that hotel customers are mainly out of town visitors that are unlikely to consult job search websites for local jobs. Thus, if we observe an effect in hotels, it indicates that an adjustment of labor supply is a likely mechanism, but on the contrary, if we do not observe an effect for hotels, it provides evidence against this mechanism.

Table 9: Effect across industries

	Monthly Visitors			
	(1) Hotels	(2) Clothing Stores	(3) Groceries	(4) Restaurants
High web-traffic week × Post	-6.864	-38.399*	-29.666**	-19.702**
	(-0.31)	(-1.90)	(-2.51)	(-2.51)
Firm × County FE	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes
N	11,706	6,371	10,646	42,488
R-squared	0.777	0.841	0.925	0.848

Notes: Errors are clustered at the county level. t-statistics are in parentheses. Statistic significance: ***=1%; **=5%; and *=10%.

Table 9 reports the estimates of the first empirical design broken down by five different industries: hotels, clothing stores, groceries, and restaurants. I only use the first empirical because the second empirical design focuses on a small sample of firms, and does not have enough power to estimate cross-sectional industry estimates. The effect of prejudice reviews on foot-traffic is prevalent in most industries but not in the hotel industry. An adjustment in labor supply should lead to an effect in hotels. But since hotel customers are mainly out-of-town visitors, this evidence is instead consistent with a reduction in consumer demand of prospective employees.

Survey evidence To further understand the importance of the underlying mechanisms in the paper, I conduct a randomized experiment on Mturk with individuals that use Indeed.com.

Table 10: Do racial prejudice reviews affect consumer demand?

	Consumer boycotting					
	(1)	(2)	(3)	(4)	(5)	(6)
	All	All	Non-white	Non-white	White F	White M
Negative racial review	0.075***		0.104***		0.075***	0.049**
	(5.33)		(3.80)		(3.33)	(2.08)
Negative non-racial review	0.021	0.021	0.031	0.031	0.023	0.013
	(1.54)	(1.54)	(1.09)	(1.10)	(1.05)	(0.56)
Neg. racial (Sr. IT Analyst)		0.097***		0.159***		
		(4.81)		(4.08)		
Neg. racial (Customer Assoc.)		0.022		0.035		
		(1.14)		(0.92)		
Neg. racial (Manager)		0.110***		0.121***		
		(5.45)		(3.09)		
N	2217	2217	678	678	724	800
R-squared	0.013	0.020	0.022	0.032	0.016	0.006

The experiment consists of showing job ads accompanied by job reviews to a representative sample of individuals to understand their job and consumer preferences. I select a sample of job ads from firms in the dataset, and then use real job reviews for those companies, but randomly select which ones are presented to each survey participant. I then ask several questions, including whether they are interested in submitting an application to the job post. And whether they would go back to the store if the company in the survey is a store where they usually shop. Section 3.3 details the design of the survey, Figure 8 provides a schematic diagram of the survey, and the Internet Appendix reports the all survey questions.

Table 10 reports the likelihood that respondents will boycott the store by estimating the following regression model:

$$\begin{aligned} \text{Consumer boycott}_i &= \beta_1 \times \text{Negative racial review}_i \\ &+ \beta_2 \times \text{Negative non-racial review}_i + \varepsilon_i, \end{aligned}$$

where *Consumer boycott* equals one if participants answer yes to the following question "", *Negative racial review* equals one if a participant was shown a negative racial review, and *Negative non-racial review* equals one if a participant was shown a negative non-racial review. Figures A.3 and A.4 in the Internet Appendix provide examples of racial and negative non-racial reviews. Since $1/3$ of the sample only views positive reviews, β_1 and β_2 measure the likelihood of consumer boycotting after a negative review is shown, relative to a positive review.

Table 11: Do racial prejudice reviews affect consumer demand?

	Non-white		Age	
	(1) Hispanic	(2) Black	(3) ≥40	(4) <40
Negative racial review	0.086**	0.172***	0.066***	0.079***
	(1.98)	(3.37)	(3.39)	(3.97)
Negative non-racial review	0.056	0.041	0.023	0.019
	(1.24)	(0.82)	(1.22)	(0.95)
N	305	217	956	1261
R-squared	0.013	0.053	0.012	0.014

Table 10 shows several interesting patterns. First, awareness of a negative non-racial review has no impact on consumer behavior. Second, a racial prejudice review increases the likelihood of consumer boycotting by 7.5 percentage points. Third, this effect is largest for non-whites and lowest for white males. Last, reviews from higher ranked employees are more likely to impact the likelihood of consumer boycotting. By and large, these findings align with the evidence shown with observational data. Prejudice reviews impact store foot-traffic, but reviews about other workplace dysfunctions do not affect the likelihood that prospective employees boycott a store. I also examine the heterogeneity of the results across different characteristics in Table 11. First, blacks are two times more likely to boycott a store than hispanics. Second, younger individuals are slightly more likely to boycott the store. Across the board, negative non-racial reviews have no effect on likelihood of boycotting.

Table 12 reports the survey results for the likelihood that respondents will submit a job appli-

cation. There are several conclusions that we can infer from these estimates. First, a racial review decreases the likelihood that a job seeker applies for a job by 12 percentage points, while a non-racial review only leads to a decline in 6 percentage points. Second, the effect of racial review on job applications in twice as large for Black job applicants. Third, the effect is larger for white females than white males. Table A.3 in the Internet Appendix shows that there is no discernible difference between young and older individuals.

Table 12: Do racial prejudice reviews affect labor supply?

	Apply for Job					
	(1) All	(2) All	(3) Non-white	(4) Non-white	(5) White F	(6) White M
Negative racial review	-0.123***		-0.140***		-0.143***	-0.091***
	(-7.14)		(-4.37)		(-4.86)	(-3.15)
Negative non-racial review	-0.065***	-0.065***	-0.093***	-0.093***	-0.083***	-0.029
	(-3.80)	(-3.81)	(-2.86)	(-2.86)	(-2.85)	(-1.01)
Neg. racial (Sr. IT Analyst)		-0.147***		-0.160***		
		(-5.94)		(-3.49)		
Neg. racial (Customer Assoc.)		-0.079***		-0.100**		
		(-3.28)		(-2.21)		
Neg. racial (Manager)		-0.147***		-0.163***		
		(-5.94)		(-3.54)		
N	2217	2217	678	678	724	800
R-squared	0.023	0.026	0.029	0.031	0.032	0.013

Which mechanism drives the results? To understand the importance of each mechanism, one needs to combine the results using observational data with those from the randomized survey. The survey findings show that bad management reviews affect labor supply, but observational data shows that these reviews do not affect foot-traffic. It follows that the foot-traffic elasticity of labor supply must be small. While a reduction in labor supply is costly for firms, it produces no effect on foot-traffic in this context. This is also consistent with the almost immediate effect observed in event plot analysis—a labor supply adjustment is likely to take longer to eventually affect store foot-traffic. Taken together, and consistent with the theory, the main results observed in the paper

are likely driven by a reduction in consumer demand rather than an adjustment of labor supply. The findings that individuals may boycott certain firms for their bad behavior is consistent with evidence showing that consumers are more likely to boycott businesses in recent years.[12]

5 CONCLUDING REMARKS

Racial prejudice in the workplace has large negative implications to employees's health and well-being. Eliminating prejudice in the workplace is critical to achieve an equal society, but developing policies almost always requires increasing public awareness of episodes of unequal treatment based on race. If job seeker's awareness of episodes of racial prejudice affects firm performance, perhaps organizations may feel sufficiently incentivized to eradicate prejudice and racial harassment in the workplace.

Analyzing 7 million job reviews from a job-search website that receives 270 million monthly visits, this paper identifies over 10,000 instances of workplace prejudice. It then examines the impact on firm performance when prospective employees learn about these incidents. To derive variation in the degree of learning, I introduce two empirical designs that draw on presumably exogenous variation. The results show that when prospective employees learn about one instance of prejudice in a specific firm-city, foot-traffic drops by 3 percentage points in all stores located in the city. An event-plot analysis suggests that this effect is likely permanent.

Combining a randomized survey experiment with observational data, I examine the importance of each economic mechanism. The analysis shows that consumer demand explains a large fraction of the main findings of the paper, while an adjustment in labor supply has limited explanatory power. When potential employees learn about racial prejudice incidents they reduce both labor supply and product demand, but the elasticity of store foot-traffic with respect to labor supply is small. Additional results show that negative non-racial reviews have no impact on consumer demand. These findings suggest that racial prejudice has very different implications on firm performance than other workplace dysfunctions.

[12] A recent survey suggests that 38% of Americans are currently boycotting at least one company, up from 26% only a year ago (LendingTree, 2020).

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INTERNET APPENDIX

FIGURE A.1: FIRM HOMEPAGE

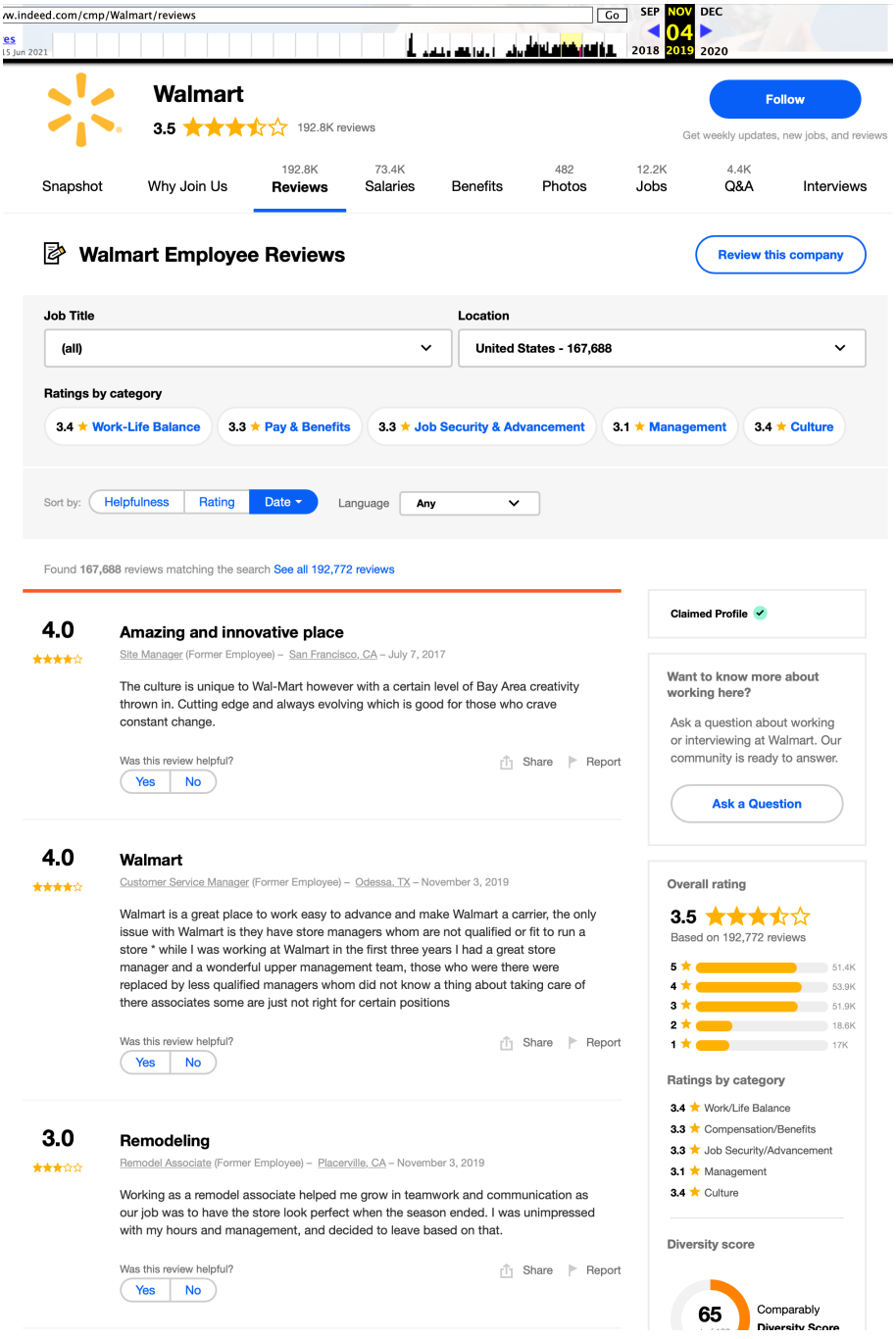
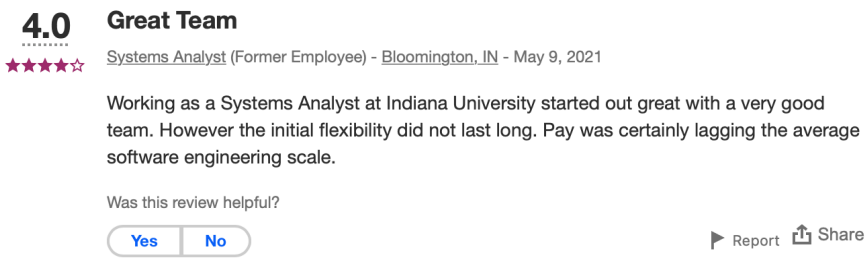


Figure A.2: Example of a review

Panel A: Example of a review



Panel B: Detailed ratings

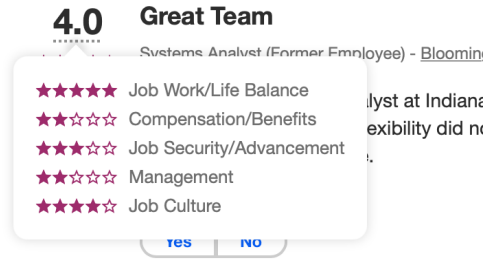
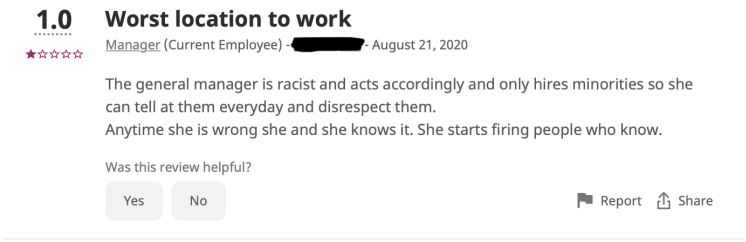
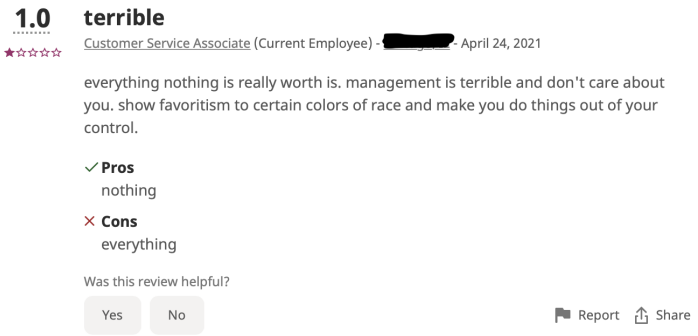


FIGURE A.3: EXAMPLE OF RACIAL HARASSMENT REVIEWS

Panel A: First example



Panel B: Second example



Panel C: Third example

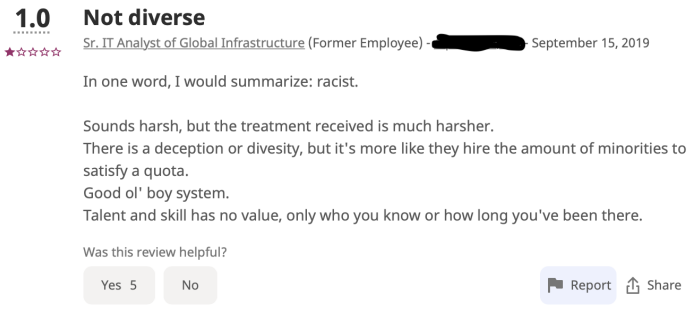
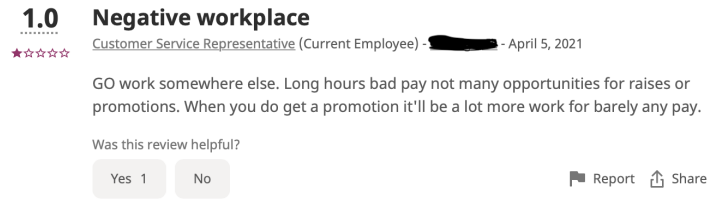
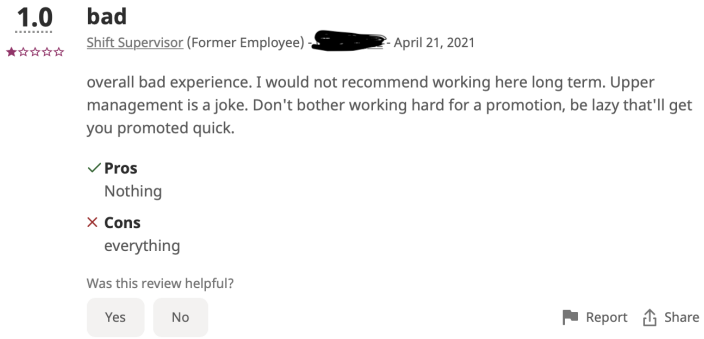


FIGURE A.4: EXAMPLE OF BAD MANAGEMENT REVIEWS

Panel A: First example



Panel B: Second example



Panel C: Third example

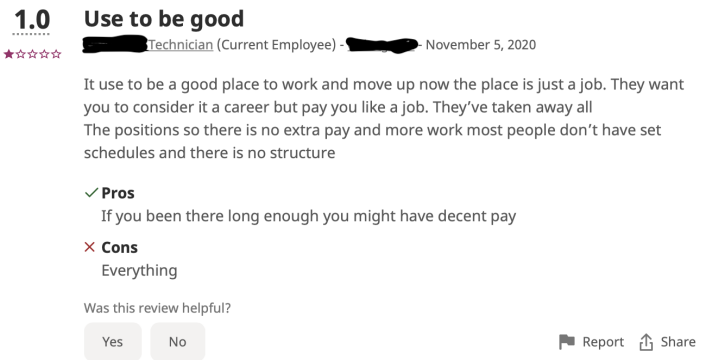


Table A.1: Companies with largest incidence of workplace prejudice

Firm	Total # Racial Reviews	Sample Fraction
Walmart	364	3.07%
MacDonald's	293	2.47%
Target	109	0.92%
Home Depot	105	0.89%
Burger King	91	0.77%
Walgreens	79	0.67%
Amazon	77	0.65%
Wendy's	76	0.64%
Lowe's	74	0.62%
CVS	71	0.60%

Notes: This table reports the firms in the sample with the largest incidence of workplace prejudice. The second column reports the total count, and the third column reports the fraction of prejudice reviews in the sample that are assigned to the company.

Table A.2: Prejudice and foot-traffic prior to March 2020

	Growth in Monthly #Visitors		
	(1)	(2)	(3)
	$\Delta = 3$	$\Delta = 4$	$\Delta = 5$
Post × Non-busy day	-15.769*	-11.276	-13.402*
	(-1.74)	(-1.29)	(-1.77)
Firm × County FE	Yes	Yes	Yes
Frim × Year × Month FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	9,569	11,557	13,741
R-squared	0.954	0.955	0.954

Notes: This table reports the estimates of regression model (7) prior to March 2020. Firms that receive between $19 - \Delta$ and 19 reviews in the 24 hours after the racial prejudice review are considered treated. Those that receive between 21 and $21 + \Delta$ are considered control firms. Δ varies between 3 and 5. *First Page* equals 1 for treatment firms and zero otherwise. *Post* is a variable equal to one after the racial prejudice report becomes public. The outcome variable is the number of monthly visitors. The sample is restricted to firm-counties for which a racial prejudice report was filed on Indeed.com. Controls include: (i) *Number establishments in county*, (ii) *Number of monthly reviews at the firm level*, and (iii) *Annual monthly visitors at the firm-city level*. Errors are clustered at the county level. t-statistics are in parentheses. Statistic significance: ***=1%; **=5%; and *=10%.

Table A.3: Do Racial prejudice reviews affect labor supply?

	Non-white		Age	
	(1)	(2)	(3)	(4)
	Black	Hispanic	$\geq$40	$<$40
Negative racial review	-0.091**	-0.234***	-0.128***	-0.121***
	(-2.33)	(-4.06)	(-4.87)	(-5.26)
Negative non-racial review	-0.077*	-0.115**	-0.056**	-0.073***
	(-1.87)	(-2.02)	(-2.16)	(-3.16)
N	305	217	956	1261
R-squared	0.020	0.072	0.024	0.022

RANDOMIZED SURVEY QUESTIONS

1. Are you currently employed?

    - Yes, full-time
    - Yes, part-time
    - No

2. How do you usually look for a job? (multiple choices allowed)

    - Monster.com
    - Indeed.com
    - Linkedin.com
    - CareerBuilder.com
    - Friends and Family
    - Contact companies directly
    - Job fairs
    - Recruitment/staffing agencies
    - Glassdoor.com
    - Flexjobs.com

3. We would like you to consider the job openings below. Please choose one job that you may be interested in applying for. Job details will be shown after you pick one option.

    - Restaurant General Manager
    - Cashier/Counter Service
    - Retail Sales Consultant
    - Supervisor Marketing Strategy
    - Store Manager
    - Associate Manager Marketing

- Bank Teller
- Assistant Manager - Room Operations
- Faculty Assistant
- Commercial Sales Manager

4. (A full job description based on a real job ad is shown)

5. We would like you to consider a few company reviews. Please read them carefully. Do not write anything in the text boxes below each review. Just click next at the bottom. These are real company reviews. A few details have been redacted to protect confidentiality.

   (Three random reviews are shown. There are three positive reviews, three bad non-racial reviews, and three racial reviews. The randomization scheme is described in the results section.)

6. Are you still interested in applying for this job?

   - Yes
   - No

7. Why aren't you interested in this job?

   - I am not qualified for this job
   - I did not like the company reviews
   - I am no longer interested in the job
   - Other, please list why.

8. If this employer is a store where you usually shop, what is the likelihood that you will go back to the store?

   - Extremely unlikely
   - Somewhat unlikely
   - Neither likely nor unlikely

- Somewhat likely
- Extremely likely

9. To which gender identity do you most identify?
    - Female
    - Male
    - Transgender
    - Non-binary/non-conforming
    - Not Listed: _____
    - Prefer not to respond

10. What is your race?
    - White
    - Black or African American
    - American Indian or Alaska Native
    - Asian Indian
    - Chinese
    - Other Asian
    - Native Hawaiian
    - Other Pacific Islander
    - Other: _____
    - Prefer not to respond

11. Are you hispanic?
    - Yes
    - No

12. How old are you?

13. What's your total annual household income?

    - Under $10k
    - $10k to $40k
    - $40k to $75k
    - $75k to $100k
    - $100k or more