DISCRIMINATION AS A SELF-FULFILLING PROPHECY: EVIDENCE FROM FRENCH GROCERY STORES*

DYLAN GLOVER
AMANDA PALLAIS
WILLIAM PARIENTE

Examining the performance of cashiers in a French grocery store chain, we find that manager bias negatively affects minority job performance. In the stores studied, cashiers work with different managers on different days and their schedules are determined quasi-randomly. When minority cashiers, but not majority cashiers, are scheduled to work with managers who are biased (as determined by an implicit association test), they are absent more often, spend less time at work, scan items more slowly, and take more time between customers. This appears to be because biased managers interact less with minorities, leading minorities to exert less effort. Manager bias has consequences for the average performance of minority workers: while on average minority and majority workers perform equivalently, on days where managers are unbiased, minorities perform significantly better than do majority workers. The findings are consistent with statistical discrimination in hiring whereby because minorities underperform when assigned to biased managers, the firm sets a higher hiring standard for minorities to get similar average performance from minority and nonminority workers. *JEL Codes:* D84, J24, J71, M50.

I. INTRODUCTION

A vast economic literature tests for the presence of labor market discrimination, defined as the differential treatment of equally productive minority and nonminority workers in terms of hiring, pay, or promotion. Becker's pioneering work,

*We thank Yann Algan, David Autor, Thomas Le Barbanchon, Eric Cediey, Raj Chetty, Bruno Crépon, Muriel Dejemeppe, Eleanor Dillon, Esther Duflo, Erin Fletcher, Roland Fryer, Ed Glaeser, Lisa Kahn, Lawrence Katz, Danielle Li, Florian Mayneris, four anonymous referees, and seminar participants at Berkeley Haas, London School of Economics, MIT, NBER Labor Studies, Nova Business School of Economics, Paris School of Economics, Sciences Po, University of California Santa Barbara, University of Illinois at Urbana-Champaign, University of Namur, and UC Louvain for their helpful comments. We also thank Lisa Abraham, Jenna Anders, Frédéric Cochinard, Elizabeth Mishkin, and Adrien Pawlik for superb research assistance. We gratefully acknowledge funding from the Fonds d'Expérimentation pour la Jeunesse (France).

The Quarterly Journal of Economics (2017), 1219–1260. doi:10.1093/qje/qjx006.
Advance Access publication on February 28, 2017.

The Economics of Discrimination (1971), introduced the notion of taste-based discrimination: employers experience disutility when employing minority workers and compensate by paying minorities less or requiring them to be more productive for the same wage. A subsequent body of work, starting with Phelps (1972) and Arrow (1973), conceived of discrimination not as a matter of animus but one of imperfect information. Unfavorable priors about minority workers' productivity or imperfect screening precision causes employers to treat equally skilled minority and majority workers unequally. Building on these insights, Lundberg and Startz (1983) and Coate and Loury (1993) showed how statistical discrimination could potentially depress minorities' skill investments by leading minorities to correctly believe that these investments would not be fully rewarded. As a result, statistical discrimination may lead to a self-fulfilling prophecy whereby employers' adverse prior beliefs about minorities' skill levels are self-confirming in equilibrium.

Something that unites these strands of literature is the implicit assumption that employers' tastes and beliefs do not directly affect worker productivity. Although statistical discrimination might inhibit skill investment, it does not directly affect the performance of workers with given skill levels. However, a strand of literature beginning with Steele and Aronson (1995) documents that adverse stereotypes about minority groups' abilities can directly impair group members' performance. This body of work finds that in some circumstances when stereotypes are made salient prior to performance (e.g., test-takers are asked to report their race or gender), blacks, Hispanics, and women perform worse than in settings where group membership is not made salient. This line of research implies that adverse employer beliefs about minorities—whether stemming from animus or statistical discrimination—could be self-fulfilling, not because they inhibit minority skill investment but because they induce poorer performance. Related research shows that individuals' own stereotypes can negatively impact their performance (Coffman 2014) and that these stereotypes need not be fully accurate (Bordalo et al. 2016).

This article presents a novel test of whether discriminatory beliefs directly affect minority workers' job performance in

a real-world workplace.[1] We study 34 outlets of a French grocery store chain. These stores employ a sizable proportion of minority workers that, based on their names, we identify as having a North African or Sub-Saharan African origin.[2] We study new cashiers hired on six-month contracts since these workers are assigned quasi-randomly to managers. These cashiers, like all cashiers in the stores, work with different managers on different days. Unlike more senior workers, however, they are not allowed to submit schedule preferences. Their schedules are determined by a computer program which assigns shifts to meet predicted demand, taking into account the preferences of more senior workers. Thus, the minority and majority workers in our sample do not choose the managers they work with and they work with the same managers under similar conditions. Workers know which managers they will be working with beforehand as both worker and manager schedules are publicly posted several weeks in advance.

We measured managers' bias toward minorities with an Implicit Association Test (IAT), which is widely used to measure bias, particularly in psychology (see Lane et al. 2007; Nosek, Greenwald, and Banaji 2007; and Greenwald et al. 2009 for summaries of the literature). IAT scores have been correlated with many real-world decisions and are difficult for subjects to manipulate.[3] The test uses the speed with which subjects categorize prompts to determine their implicit association between two concepts: here (i) traditionally French- or North African–sounding names and (ii) words indicating worker competence or incompetence. Our manager bias score thus measures the extent to which managers associate North African names with poor worker performance. This

1. This article is related to the literature showing that workers and students benefit from interacting with co-ethnics. See, for example, Dee (2004, 2005), Stoll, Raphael, and Holzer (2004), Stauffer and Buckley (2005), Giuliano, Levine, and Leonard (2009, 2011), Price and Wolfers (2010), and Hjort (2014). It is also related to the literature started by Rosenthal and Jacobson (1968) showing that teachers' expectations about student performance can directly affect student outcomes.

2. Workers are categorized into minority and nonminority status based on their names because in France it is illegal to ask workers their ethnicity. ISM CORUM, an expert in discrimination testing in France, did the categorization. We gave ISM CORUM separate lists of first and last names, so that it would not be able to identify any individual in the study.

3. See, for example, Kim (2003), Friese, Bluemke, and Wänke (2007), Green et al. (2007), Greenwald et al. (2009), and Rooth (2010).

concept is correlated with, but distinct from, managers' distaste for minorities (Agerström, Carlsson, and Rooth 2007).

Each of the stores in our sample tracks individual performance at a daily level. The stores provided us with data on absences and time worked (determined by time clock data), scanning speed, and time taken between customers. The firm considers absences particularly important: being absent three times is one of the few ways a worker can be fired during her initial six-month contract. The firm also prioritizes scanning speed, posting a list of workers' articles scanned per minute in the break room each week. The firm uses these performance metrics along with the managers' observations about workers' performance and customer relations to determine whether workers will be offered a longer contract at the end of their six-month contract. Approximately 30–40% of workers are offered a longer contract.

We assess whether minority cashiers perform worse on the days they work with managers who are biased against their minority group. Because there may be other differences between more and less biased managers—biased managers may simply be less skilled, for example—we do not want to simply attribute any change in minority performance when working with more biased managers to manager bias. Instead, we use a difference-in-difference methodology, comparing the change in minority workers' performance under more and less biased managers to the change in nonminority performance.

We find that manager bias leads minorities to perform worse. Minorities are more likely to be absent when scheduled to work with more biased managers. When they do come to work, they spend less time at the store: specifically, they are much less likely to stay after their scheduled shift ends. While workers are allowed to leave when their shift ends, managers can ask them to work late. Because workers are paid based on time worked, we estimate that minorities earn 2.5% less as a result of manager bias.

Minorities also scan items more slowly and take more time between customers when working with biased managers. Throughout our analyses, none of the differential effects of working with more biased managers are explained by the other manager characteristics we have, including the managers' own minority status. The effect of manager bias is concentrated in stores with fewer minority workers and appears to grow during the contract (though this latter difference is not statistically significant).

We combine data from a worker survey conducted after contract expiration with our administrative data to distinguish between theories of discrimination that can explain our results.[4] First, we find little evidence that animus—or biased managers treating minorities poorly—can explain our results. Minority workers do not report that biased managers disliked them or that they disliked biased managers. They report that biased managers were less likely to assign them to unpleasant tasks (cleaning) and no more likely to assign them unpleasant registers or breaks.

Our evidence is most consistent with a theory in which biased managers interact less with minority workers. Research in psychology on "aversive racism" has found that individuals with implicit biases toward minority groups are less likely to speak to, more hesitant in speaking to, and less friendly toward members of those groups.[5] They may feel less comfortable interacting with minorities, or they may be concerned about appearing biased. Using whether a worker remembered each manager as an indicator for worker-manager interaction, we find minorities were less likely to remember biased managers. Worker-manager interaction appears to be a key determinant of performance: workers performed substantially better when working with managers they later remembered.[6] Biased managers interacting less with minorities is consistent with their being less likely to ask minorities to do cleaning duties. It may also explain why minorities are less likely to stay after the end of their shifts when working with biased managers: the managers might simply not ask them to.

An alternative explanation concerns self-stereotyping or stereotype threat. Minorities may hold negative stereotypes about their suitability for the job or be aware of existing stereotypes about their group, and biased managers may activate these negative stereotypes. To test whether this occurred, we asked workers which managers gave them the most confidence in their abilities.

4. One explanation that cannot drive the day-to-day differences in performance we find is that biased managers depress minority human capital accumulation. Minorities may accumulate fewer skills under biased managers, but we would not detect this because minorities would have any skills they learned working with unbiased managers on the days when they work with biased managers.

5. See, for example, McConnell and Leibold (2001), Dovidio, Kawakami, and Gaertner (2002), and Hebl et al. (2002). Dovidio and Gaertner (2008) summarize this literature.

6. This is consistent with Mas and Moretti (2009) which finds that monitoring improves cashiers' performance.

Minorities do not report that biased managers gave them less confidence in their abilities. Nevertheless, this does not rule out an explanation whereby biased managers subconsciously activated minorities' negative stereotypes.

Finally, we find that the negative impact of manager bias on minority performance may lead to statistical discrimination in hiring. Under statistical discrimination, the firm infers worker productivity from workers' observable characteristics and minority status. By depressing minority productivity, manager bias can lead the firm to act as if minority workers are less productive than majority workers with the same characteristics. Because the firm has to pay workers the same wage independent of their performance, it would set a higher hiring threshold for minorities. Thus, in the absence of manager bias, hired minorities would perform better than hired majority workers. Overall, we find that minority and majority workers perform equivalently. There is no difference in their average absence rates, time spent at work, articles scanned per minute, or time taken between customers.[7] However, consistent with statistical discrimination, when working with unbiased managers, minority workers perform substantially better than nonminorities. They are half as likely to be absent and scan significantly faster.

With some assumptions, we can combine our performance metrics to estimate the number of customers each worker serves per day. On average, minorities serve an insignificant 2% more customers than do majorities. However, when they work with unbiased managers, minorities serve 9% more customers than majorities. While the average minority is at the 53rd percentile of average worker performance, on days with unbiased managers she is at the 79th percentile. This suggests there are substantive consequences of manager bias on minority workers' performance and thus, in theory, workers' subsequent labor market outcomes.[8]

7. The similar performance of minority and majority workers is (weakly) inconsistent with a model of taste-based discrimination in which the firm faces a utility cost of employing minorities. In this model, the firm requires higher average productivity from minorities to hire them at the same wage. Although minorities may perform slightly better, we can reject that hired minorities perform more than 4% better on average than hired majority workers.

8. Unfortunately, we do not have data on which workers were offered a second contract. Our identification strategy also does not lend itself well to determining the effect of manager bias on workers' subsequent labor market outcomes because over the six-month contract, there is little variation in the average bias workers are exposed to.

The article proceeds as follows. Section II explains the empirical context. Section III describes the data, provides descriptive statistics, and tests the identifying assumption that minority and majority cashiers work with more and less biased managers under similar conditions. Section IV presents the main results, showing that across several outcomes, minorities perform worse when working with more biased managers. Section V presents and tests predictions of discrimination theories that can explain (i) why minorities perform worse under biased managers and (ii) the firm's hiring decisions. Section VI concludes. All supplementary material is in the Online Appendix.

II. Setting

We study entry-level cashiers in a large French grocery store chain. These cashiers are hired on a specific contract called *Contrat de Professionnalisation* (CP): a six-month contract subsidized by the government. In return for the subsidy, the firm trains CP workers (or CPs) to be cashiers and on the retail sector in general. Apart from the direct subsidy, these contracts are advantageous to firms because they include a week-long trial period before the official contract start date in which workers are trained without pay. During this week, either party can walk away from the contract without penalty.

CP cashiers perform the same job (running a cash register) as other workers. However, there are two special aspects of their employment. First, one day each week CPs attend training, during which they are not on the store floor. (Training days are not included in our data.) Second, CPs have no control over their schedules. All other cashiers are allowed to submit schedule preferences. A computer system assigns shifts by matching predicted demand to the available workforce, taking the preferences of non-CP workers into account. The computer system is constrained to ensure that workers have the requisite number of days off and that no worker may have more than two split shifts a week, open the store more than twice a week, or close the store more than twice a week.[9] Schedules are determined three weeks at a time and publicly posted. Manager schedules are also publicly posted in advance, so workers know ahead of time which managers they

9. A split shift occurs when a worker is scheduled to work for two separate periods in the same day (for example, from 9 am to 12 pm and from 3 pm to 6 pm).

will be working with. The chief cashier (the managers' boss) can, in theory, revise the schedules assigned by the computer system. However, this happens very rarely.

The stores typically have around five cashier managers (henceforth "managers") and 100 to 250 cashiers. There are 30 to 80 registers in each store, though it is rare that all the registers are open at once. The manager on duty sits in a special station in the middle of the registers. When a cashier arrives for her shift, she "badges" (clocks) in near the manager station. She typically has a brief conversation with the manager, who gives her the day's news and assigns her to a register. Some workers are assigned to special cash registers, such as the 10-items-or-less line or the self-checkout, though this is rare for CPs. The worker then gets her till (cash box) from the safe, sets it up at her station, and starts receiving customers. There are no baggers in these stores; customers bag the items themselves.

The manager roams the store, talking with cashiers and monitoring them at their stations. She manages the lines, opening and closing new ones and directing customers to short ones. Cashiers whose lines are closed are assigned to other tasks such as aisle arrangement, the welcome desk, or assisting managers. The manager also decides when workers can go on break, though the amount of break time is specified in workers' contracts. Workers are allowed to leave at the end of their shifts, but the manager can ask them to work late. Before leaving, cashiers confer with the manager, return their tills to the safe, and badge out near the manager station.

CP workers are hired in waves: approximately twice a year each store has a "promotion," in which new CPs are hired. The managers we study are rarely involved in the hiring process, which is conducted by the chain's central office and the store's chief cashier.

The most important performance metrics for workers are showing up to work, arriving on time, and having the correct amount of money in the till. During their initial contract, workers can be fired only for misconduct, which includes having more than three absences, being late more than three times, having more than three warnings for misbehavior, or having even one report of violent conduct or one large till deviation. If misconduct occurs, the chief cashier decides whether to fire the CP, relying on the advice of the managers. Aside from misconduct, the most important indicator of cashier performance is the number of articles

scanned per minute. Each week, a list of workers' average articles per minute is posted in the employee break room.

CPs are not paid based on performance; they are paid solely based on time worked. In particular, CPs are not paid for days they are absent, though after three sick days and a doctor's authorization, the government pays 70% of workers' pay during their sick leave.[10] CPs' paychecks are also adjusted if they work more or less than scheduled.

CPs' main incentive to perform well is the opportunity to receive another contract. After their initial six-month contract, about 30% to 40% of workers are offered another contract. The chief cashier decides whether to offer subsequent contracts to each worker based on the worker's performance, manager evaluations, and the number of available positions at the store. These subsequent contracts are of longer duration and pay higher salaries.

Managers are on indefinite-term contracts; their pay is fixed, not dependent on their performance. Managers' performance is assessed annually based on customers' checkout experience, which is determined by how quickly the lines move, and to a lesser extent register cleanliness, the stocking of the small shelves at each register, and effective handling of customer problems. Although managers are graded on their support of inexperienced cashiers, they do not have the primary responsibility for cashier training.

III. Data and Descriptive Statistics

III.A. Data Sources

We use three sources of data: store administrative data, manager survey data, and worker survey data. The store administrative data provide information on worker and manager schedules and worker performance. The manager survey data provide our measure of manager bias. We use the worker survey data to learn about the mechanism for the effects of manager bias.

1. Administrative Store Data. We collected daily data for each CP in a given promotion over a six-week period between July 2011 and August 2012. We have schedule data: the precise times at which workers and managers were supposed to begin and end

10. CPs earn vacation time but cannot use it for days off: they are paid for their vacation days after the end of the contract.

their shifts, allowing us to determine which manager(s) a worker was scheduled to work with on a given day.

We also have badge data: the precise times workers and managers badged in and out of the stores. Managers and CPs must badge in and out at the beginning and end of their shifts and for breaks, so we have actual working times to the minute. Combining these data with the schedule data provides our first two metrics of worker performance: absence and the number of minutes worked relative to the number of minutes the worker was scheduled to work.[11] Time spent at the store can differ from the schedule for three reasons: (i) workers arrive earlier or later than scheduled, (ii) workers leave earlier or later than scheduled, and (iii) workers take breaks. Although workers are entitled to breaks, breaks are not scheduled by the computer program.

We also have daily worker performance data, most important, articles scanned per minute. The time over which articles per minute is calculated starts when a worker scans a customer's first item and stops when a worker scans a customer's last item, so it is not affected by time between customers. We also have two other determinants of line speed. The first is inter-customer time: the time between finishing one customer's transaction and starting to scan the next customer's items. The second is payment time: the time between the scanning of a customer's last item and the completion of the customer's transaction, during which time she is paying. While the firm tracks both of these metrics, it does not emphasize them as key performance measures.

Our final sample has 34 stores, 204 workers, and 4,371 worker-day observations.[12] Although we asked for data from all of the chain's stores in France, we received the necessary administrative data from 45 of them. From these 45 stores, we eliminate 11 in which managers did not take the IAT. (The process of getting managers to take the IAT is explained more below.) Because most stores had multiple promotions during the year, we have data on 51 promotions from the 34 stores in our sample. Two stores did

11. We analyze time worked relative to time scheduled instead of simply time worked to gain precision.

12. Throughout the article, we cluster standard errors at the store level to allow for correlation in performance both within and across days in a store. While we have more than 30 clusters, we show that p-values are similar when we use a wild cluster bootstrap procedure that is robust to having a small number of clusters (Cameron, Gelbach, and Miller 2008; Garthwaite, Gross, and Notowidigdo 2014).

not provide data on inter-customer time, and four did not provide data on payment time, so we have slightly smaller samples for these outcomes.[13]

Because we wanted variation in the timing of the observations during the contract, we asked for data on weeks 3 through 8 of the contract for some promotions and weeks 18 to 23 for others. We have data on weeks 18 to 23 for promotions that occurred chronologically earlier and data on weeks 3 to 8 for promotions that occurred chronologically later because stores kept data for only one year.

In addition to these data, the stores provided a few other worker and manager characteristics, most important, their names. In France, it is illegal to ask people about their ethnicity. Thus, we use workers' names as an indicator of their minority status. ISM CORUM (Inter Service Migrants, Centre d'Observation et de Recherche sur L'Urbain et ses Mutations), a leading specialist in discrimination testing in France, performed the categorization. We provided ISM CORUM with separate lists of first and last names, so that it did not know the name of any individual in our study, much less any information about the workers it classified. Each first and last name was categorized into one of five possible origin types: (i) European, (ii) North African, (iii) Sub-Saharan African, (iv) mixed or undetermined, and (v) other (including names of Turkish and Asian origins). We consider workers with a North African and Sub-Saharan sounding first or last name as the minorities in this context. In the Online Appendix we show results are robust to using other definitions of minority status.

We also classified workers' and managers' genders using their names. The chain also provided managers' ranks (positions) within the store and managers' dates of birth.

2. Manager Survey and IAT. We measure managers' bias toward minority workers using an IAT (Greenwald, McGhee, and Schwartz 1998; Nosek, Greenwald, and Banaji 2007). The IAT is widely used, particularly in psychology, to measure unconscious bias. The test involves categorizing two sets of words to the left and right sides of a computer screen. In our case, subjects were

13. One store did not provide data on either of these outcomes. The remaining stores provided data on the total amount of inter-customer time or payment time during the worker's shift, not scaled by the number of customers served.

presented with (i) names typically indicating a French origin (e.g., Jean) or names traditionally indicating a North African origin (e.g., Ahmed) and (ii) adjectives that describe good employees (e.g., reliable) or bad employees (e.g., incompetent).

In all rounds, one word at a time (either a name or adjective) comes onto the screen and subjects are told how to categorize it (for example, adjectives describing good employees to the left, adjectives describing bad employees to the right). Subjects are instructed to categorize the words as quickly as possible. In the rounds used for scoring, the names and the adjectives are interspersed. In one of these rounds, subjects are told to categorize French-sounding names and negative adjectives to the same side of the screen, whereas in the other, they are tasked with categorizing North African–sounding names and negative adjectives to the same side. The idea behind the test is that if a subject has an implicit association between two concepts (e.g., workers of North African origin and bad employees), it should be easier and quicker to do the categorization when they are placing those words on the same side of the screen. The test produces a measure of bias that compares the time taken to categorize items when North African–sounding names and negative adjectives are categorized on the same side of the screen, relative to when French-sounding names and negative adjectives are categorized on the same side.[14]

IAT scores have been found to be correlated with judgments, choices, and psychological responses (Bertrand, Chugh, and Mullainathan 2005). For example, IAT scores are correlated with voting behavior (Friese, Bluemke, and Wänke 2007), callback rates of minority job applicants (Rooth 2010), and doctors' provision of differential medical treatments by race (Green et al. 2007). Moreover, research suggests that it is very difficult to fake an IAT score.[15]

The chain's human resources office contacted the chief cashier in each store, asking her to get the managers to take the IAT as part of a study. While managers could likely tell from the IAT

14. We randomized the order in which subjects completed these rounds. We also included practice rounds to mitigate order effects (Nosek, Greenwald, and Banaji 2007). We used the computer software Inquisit to administer the IAT.

15. See Banse, Seise, and Zerbes (2001), Egloff and Schmukle (2002), Kim (2003), Greenwald et al. (2009), and Hu, Rosenfeld, and Bodenhausen (2012). Faking a score on an IAT requires a specific strategy of slightly speeding up or slowing down in certain blocks, a strategy that few participants spontaneously discover (Greenwald et al. 2009).

that the study concerned their beliefs about minorities, they did not know the exact purpose of the study. Managers were allowed to take the test during work hours, but did not receive any payment for doing so. Initially, managers received an email with a link to the IAT so that they could take it at their convenience. We sent email reminders and periodically called the chief cashiers to induce more managers to take the test. We also visited stores that had technical difficulty accessing the IAT website, administering the test in person to these managers.

The managers took the IAT on average 17 months after the administrative data in our sample. Thus, neither taking the IAT nor knowledge of our study could have affected managers' treatment of minority workers in our data. Managers' experience in the store could have affected their implicit beliefs, but it seems very unlikely that interaction with the CPs in our study would have led to variation in those beliefs. In particular, our identification strategy ensures that the more and less biased managers we compare worked with the same CPs. The vast majority of our managers (85%) had been at the store for over 10 years, so would have seen at least 20 different CP promotions, several more recent than the ones we study. These managers have managed 100 to 250 workers at a time for many years (relative to an average of six CPs per manager in our study), most of whom they work with much longer than with CPs. Moreover, the effects do not change with the length of time between the administrative data and when managers took the IAT. Finally, we use male names in the IAT (over 90% of our CPs are female) so managers are not prompted by the names of specific workers.

Although it is unlikely that interacting with the CPs in our study affected managers' IAT scores, interacting with minorities in general might have. For example, if minorities disliked some managers and, as a result, performed badly for them, these managers might have developed negative beliefs about minority workers' performance. We think this is unlikely: minorities do not report disliking biased managers and the negative impacts of manager bias on minority performance appear to be driven by manager actions, not solely worker actions. Nevertheless, we cannot fully rule out this alternative explanation.

We have IAT scores for 77% of the managers in the 34 stores. On most dimensions we have, managers who did and did not take the IAT look similar. They were the same average age and were equally likely to be a minority and to have a high position

in the store. Calculating manager fixed effects for all of our performance outcomes produces no significant differences between managers who did and did not take the IAT. (These differences are also inconsistently signed.) There is no correlation between the number of days it took managers to take the IAT after we requested it and their IAT scores, so it does not necessarily appear that more biased managers were more reluctant to take the test. Male managers were less likely than were female managers to take the IAT. We show, however, that our results are robust to including controls for manager gender and manager gender interacted with worker minority status.

3. Worker Survey. We conducted a telephone questionnaire from May 2013 to September 2013, surveying former CPs about their relationship with each of their managers. The heart of the questionnaire comprised CPs ranking their managers on a variety of dimensions. Respondents rated the extent to which they remembered each manager, which we use as a measure of worker-manager interaction. We also described manager traits or actions (e.g., the manager who liked the worker best) and asked workers to rate in order the top and bottom three managers on each trait.[16] We provided workers with a list of managers, but did not tell them managers' IAT scores, nor did we ask whether they thought the managers were biased.

Half of surveyed workers responded. The main cause of nonresponse was that CPs no longer had the same contact information and their phone numbers had been disconnected. (Only 2% of workers answered the phone but refused to answer the survey.) We have survey responses for 94 workers in our main sample. Because we did not know which stores would provide performance data when we conducted the survey, we surveyed a larger sample. We also have survey data for 74 workers for whom we have manager IAT scores but not performance data and 10 workers for whom we have performance data but not manager IAT scores.

Controlling for store fixed effects, minorities were 7.4 percentage points less likely to respond to the survey (off a base of 52.6% for majorities). Although this is not a statistically significant difference, it is not a small one. Online Appendix Table A.1 compares

16. Most workers had six or fewer managers. In a pilot, we asked workers to rate all of their managers, however, workers found this difficult. There was substantial nonresponse and a few workers asked to stop the survey.

the characteristics and performance of workers who responded and workers who did not, for the whole sample and separately for minority and majority workers. There are few differences between respondents and nonrespondents. Consistent with chance differences, of the 30 comparisons in the table, one is significant at the 5% level and two are significant at the 10% level.[17]

III.B. Descriptive Statistics

Table I reports descriptive statistics. We know only two things about all CPs: their minority status and gender, both based on their names. Whereas 28% of workers are minorities, only 7% are male. The worker survey paints a slightly richer picture of workers in these stores. Despite this being an entry-level job, the average worker is 30 years old and has had four previous jobs. Only 11% of the sample has had no prior employment. Most workers (58%) do not have a high school degree, while relatively few (7%) have more than a high school education.

Managers tend to be older, averaging 41 years of age. Relatively few of the managers are minorities themselves (6%) and few are male (10%). Managers' IAT scores suggest that most are biased against minorities. For ease of interpretation, throughout the article, we divide managers' raw IAT scores by the standard deviation in our sample (0.36). Positive scores indicate a preference for majorities and negative scores indicate a preference for minorities. The average (scaled) manager IAT score is thus 1.35, which means that the average manager is 1.35 standard deviations away from being completely unbiased. Using the typical thresholds in the literature,[18] 9% of managers show little to no bias against minorities, 20% show a slight bias against minorities, and 66% of our sample shows moderate to severe bias against minorities. Only 4% of our sample shows a preference for minorities. The managers seem approximately as biased as U.S. undergraduates are against African Americans though more

17. Respondents worked more minutes per day than did nonrespondents both among majority workers and in the overall sample. Respondents in the overall sample were less likely to be from the Paris region.

18. See, for example, Greenwald, Nosek, and Banaji (2003), Rooth (2010), Haider et al. (2011), and Hahn et al. (2014). Raw IAT scores below −0.15 indicate some preference for minorities; scores between −0.15 and 0.15 indicate little to no bias; scores between 0.15 and 0.35 indicate a slight bias against minorities; and scores above 0.35 show moderate to severe bias against minorities.

TABLE I

DESCRIPTIVE STATISTICS

	Administrative data sample		Worker survey sample	
	All observations from included stores	Regression sample (observations with manager IAT scores)	All survey respondents	Survey respondent regression sample
Panel A: Worker characteristics				
Minority	28%	28%	29%	25%
Male	6.9%	7.4%	7.7%	7.3%
Age			29.9	30.1
Number of previous jobs			3.9	4.0
Less than high school education			58%	61%
High school degree			35%	32%
More than high school education			7%	7%
Number of workers	218	204	310	178
Panel B: Manager characteristics				
Minority	6%	8%		
Male	10%	7%		
Level 4 (high position)	18%	18%		
Age	41.1	41.1		
Average IAT score (in standard devs)		1.35		
Moderate to severe bias		66%		
Slight bias		20%		
Little to no bias		9%		
Preference for minorities		4%		
Number of managers	154	119		
Panel C: Shift characteristics				
Scheduled days per week	4.2	4.2		
Scheduled hours per day	7.2	7.2		
Absent	1.8%	1.6%		
Minutes worked in excess of schedule	−0.31	−0.06		
Articles scanned per minute	18.5	18.5		
Inter-customer time (seconds)	29.2	28.7		
Payment time (seconds)	50.7	50.8		
Number of shifts	5,099	4,371		
Number of stores	34	34	70	51

Note. The first two columns of data provide descriptive statistics for the sample for whom we have administrative data. The first includes all observations from the 34 included stores, while the second includes only observations for which we have the manager's IAT score. The final two columns provide descriptive statistics for the worker survey sample. The first includes all survey respondents, while the second includes only those workers for whom we either have managers' IAT scores or performance data and are thus included in the analysis. Level 4 managers have a higher position in the store than the remaining managers. Manager age is reported as of January 1, 2012. *Moderate to severe bias* is defined as having a raw IAT score above 0.35, *Slight bias* is defined as having a raw IAT score between 0.15 and 0.35, *Little to no bias* is defined as having a raw IAT score between −0.15 and 0.15, and *Preference for minorities* is defined as having a raw IAT score below −0.15.

biased than Americans who choose to take an IAT online (Amodio and Devine 2006; Smith-McLallen et al. 2006; Mooney 2014).

Online Appendix Table A.2 shows the results of regressing manager IAT score on manager characteristics. The point estimates suggest that older managers tend to be more biased, and minority managers are less biased against their own group. However, none of these coefficients are significant, partially because we have so few minority managers.[19] Minority and majority CPs work in stores where managers are equally biased.

Workers are scheduled to work just over four days a week on average (in addition to the training day). Working days are distributed relatively evenly Monday through Saturday. We have relatively few observations on Sundays because the stores open on Sundays only during December. Workers are scheduled to work just over seven hours a day on average. The median shift starts at 10:15 am and ends at 8:15 pm.

Table I provides the means of the dependent variables. First, CPs are absent less than 2% of working days, an absence rate that leads to an average of two absences over the six-month contract. Second, CPs work almost exactly the number of minutes they are scheduled. Workers badge out of the store during breaks, but they tend to arrive earlier and stay later than scheduled. On average, CPs scan approximately 18.5 articles per minute, take just under 30 seconds between finishing one customer's transaction and starting the next,[20] and spend approximately 50 seconds per customer in payment time.

III.C. Exogeneity

Throughout the article we want to interpret any change in performance when minorities worked with biased

19. The coefficients suggest that on average, minority managers are 0.44 standard deviations less biased against minorities and a manager 10 years older is 0.08 standard deviations more biased. Controlling for other manager characteristics and store fixed effects, these effects decrease to 0.14 and 0.03 standard deviations, respectively.

20. We eliminate 25 observations where workers spent more than two minutes on average between customers throughout the day. We think these are likely data errors or they indicate that something else was going on in the store outside the CP's control. (For example, one observation indicates that a worker spent 49 minutes on average between customers.) Spending more than two minutes on average between customers is unrelated to manager bias or the interaction of manager bias and worker minority status.

managers—relative to when majorities worked with biased managers—as a causal effect of working with those managers. The key assumption is that minority workers were not systematically scheduled to work with biased managers on days or times when their performance would have been particularly high or low for other reasons. We first assess whether minority and majority workers were scheduled to work at similar times under similar conditions. We then analyze whether minority and majority workers were scheduled to work with more and less biased managers at similar times under similar conditions. Throughout the article, we use the CPs' and managers' schedules to construct CPs' exposure to bias since CPs' actual working times respond to the managers they are paired with.

Table II, Panel A compares the shifts minority and majority workers were scheduled to work. Each column in the panel presents a separate regression of a characteristic of a scheduled working day on an indicator for the worker's minority status. We control for store fixed effects, as shift assignment is only quasi-random within a store. We cluster standard errors at the store level.

The first dependent variable is the bias (IAT score) of the CP's scheduled manager. For workers scheduled to work with multiple managers on a given day, this is a weighted average of the managers' IAT scores, where the weights are based on the amount of time each worker was scheduled to work with each manager. If we do not have a manager's bias score, we simply omit this manager from the calculation. We might have expected that if minority workers had control over their schedules or their schedules were assigned nonrandomly by managers, they would have been less likely than majority workers to work with biased managers. Instead, we see that the difference is not significant and the point estimate goes in the other direction. The next column investigates whether minority workers are more likely to work with managers who themselves are minorities. Again, we find no effect. Next, we consider minorities' likelihood of working with male managers and level 4 managers (who are higher in the store hierarchy than level 3 managers). We see no difference in the likelihood that minority and majority workers are scheduled to work with different types of managers. Nor do we see a difference in the number of managers they work with on a shift.

Workers may systematically scan articles faster on some days than others, for example because stores are busier. To construct

TABLE II

EXOGENEITY OF SCHEDULED SHIFTS

	Manager bias	Minority manager	Male manager	Level 4 manager	Total managers	Articles per minute in other stores on date	Shift includes early morning	Shift includes late evening	Total hours	Split shift
Panel A: Minority workers										
Minority worker	0.005	0.000	−0.011	−0.001	−0.025	0.045	0.014	0.021	0.017	−0.000
	(0.022)	(0.003)	(0.009)	(0.004)	(0.043)	(0.086)	(0.015)	(0.013)	(0.034)	(0.011)
Panel B: Minority workers and manager bias										
Minority worker ×		0.009	−0.002	0.008	−0.013	0.049	−0.004	0.007	0.038	0.002
manager bias		(0.007)	(0.008)	(0.010)	(0.024)	(0.044)	(0.008)	(0.007)	(0.037)	(0.012)
Minority worker		−0.009	−0.009	−0.010	−0.011	−0.007	0.019	0.013	−0.024	−0.002
		(0.009)	(0.007)	(0.012)	(0.034)	(0.074)	(0.017)	(0.013)	(0.049)	(0.020)
Manager bias		−0.032	0.015	0.049	0.066	−0.043	−0.002	−0.003	−0.058	−0.017
		(0.021)	(0.025)	(0.033)	(0.048)	(0.093)	(0.011)	(0.019)	(0.062)	(0.022)
Observations	4,371	4,371	4,371	4,371	4,371	4,238	4,371	4,371	4.368	4,371
Dependent variable mean	1.13	0.060	0.114	0.171	2.74	18.23	0.141	0.580	7.22	0.465
Store fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note. Each column in Panel A shows the result of regressing the dependent variable indicated by the column heading on an indicator for the worker being a minority. Each column in Panel B shows the results of regressing the same dependent variable on a dummy for the worker's minority status, the manager's IAT score (in standard deviation terms), and the interaction of the worker's minority status and the manager's IAT score. Both the dependent variables and the manager's IAT score are based on the store's schedule, not actual realizations. For example, *Shift includes early morning* is a dummy variable for the shift being scheduled to start at 9 am or earlier, regardless of whether the worker arrived by that time. *Shift includes late evening* is an indicator for the shift being scheduled to end at 8 pm or later. *Manager bias* is the manager's IAT score, while *Minority manager*, *Male manager*, and *Level 4 manager* are indicators for a manager being a minority, being male, and having a high-level management position, respectively. When workers are scheduled to work with more than one manager, manager variables are averages, weighted by the amount of time workers were scheduled to work with each manager. Observations are worker-days and standard errors are clustered at the store level.

a single measure of how productive workers are on a given date, we calculate the average articles scanned per minute in all other stores (excluding the store itself) on that date. We see no evidence that minority workers work on particularly productive or unproductive days, nor that minority workers are any more or less likely to work in the early morning or late evening. Minority and majority workers work the same number of hours per day and are equally likely to have split shifts. Online Appendix Table A.3, Panel A shows that minority and majority workers also work under similar conditions when we do not restrict the sample to days in which they are working with at least one manager who took the IAT.

Table II, Panel B assesses whether minority workers work with more and less biased managers under the same conditions as do nonminorities. It presents the results of estimating the equation

$$y_{ist} = \alpha + \beta_1(minority_i \times bias_{ist}) + \beta_2 bias_{ist} + \beta_3 minority_i + \delta_s + \varepsilon_{ist}. \quad (1)$$

Here, y_{ist} is a characteristic of the shift worker i in store s who was scheduled to work on day t. $minority_i$ is an indicator for worker i being a minority, and $bias_{ist}$ is the scaled IAT score of the manager the worker was scheduled to work with on day t in store s. Store fixed effects, δ_s, are included. The coefficient β_2 can be significantly different from 0 without violating our key assumption, though it never is. This term measures how the conditions under which more and less biased managers work with nonminorities differ. The coefficient β_3 measures how the working conditions of minority and majority CPs differ when working with unbiased managers. We see across the board that these coefficients are insignificant. The primary coefficient of interest, β_1, shows how the working conditions of minority CPs change relative to those of majority CPs when both work with a manager one standard deviation more biased. Again, all the estimated coefficients are insignificant. Panel B of Online Appendix Table A.3 shows that all the coefficients are also insignificant when we include worker instead of store fixed effects. Online Appendix Table A.4 shows that minority CPs are not differentially likely to be scheduled to work at the same times as other minority CPs.

IV. Effect of Manager Bias on Performance

We turn our attention to assessing whether minority workers perform worse when paired with biased managers. We first consider absence rates and the amount of time spent at work, which are important to the firm and directly affect workers' pay. Then, we consider measures of performance while at work, the most important of which is articles scanned per minute.

To determine the effect of manager bias on worker performance, we estimate the equation

$$y_{ist} = \alpha + \beta_1(minority_i \times bias_{ist}) + \beta_2 bias_{ist} + \delta_i + X_{ist}\beta_3 + \varepsilon_{ist}. \quad (2)$$

Here, y_{ist} is a performance metric for worker i in store s on day t. $minority_i$ and $bias_{ist}$ are defined as in the previous section. The regression controls for worker fixed effects, δ_i, and shift characteristics X_{ist}. Standard errors are clustered at the store level. The coefficient of interest, β_1, measures how minorities' performance changes (relative to the change in nonminority performance) when working with a manager one standard deviation more biased.

We expect the estimate of β_1 to be attenuated due to measurement error. Workers' names do not provide a perfect measure of minority status, and we do not have IAT scores for all managers. However, the largest source of measurement error is likely to be that managers' IAT scores are not a perfect measure of bias. Nosek, Greenwald, and Banaji (2007) summarize studies measuring the IAT's reliability over time and finds that individuals' scores on different IAT administrations have a correlation of approximately 0.56, an effect that doesn't change with the length of time between testing. If the IAT is a combination of managers' true implicit bias and noise that is uncorrelated across test administrations, the coefficients of interest will be attenuated by a factor of approximately 1.8 due to measurement error in the IAT score.

IV.A. Time Spent at Work

CPs are absent less than 2% of days. Absence rates increase throughout the week, starting at a low of 0.7% on Monday and reaching 2.3% on Saturday; absence rates are even higher (2.8%) on the rare occasion that workers work on Sunday. However, absences are not significantly different on days with morning or evening shifts.

TABLE III
EFFECT OF MANAGER BIAS ON TIME SPENT AT WORK

Panel A: Dependent variable: absence indicator				
Minority worker ×	0.0098**	0.0095**	0.0117***	0.0118***
manager bias	(0.0039)	(0.0040)	(0.0042)	(0.0043)
Manager bias	−0.0021	−0.0021	−0.0050	−0.0052
	(0.0031)	(0.0032)	(0.0040)	(0.0042)
Minority worker ×				0.0081
minority manager				(0.0972)
Minority manager				−0.0057
				(0.0153)
Observations	4,371	4,371	4,371	4,371
Dependent variable mean	0.0162	0.0162	0.0162	0.0162
R-squared	0.0005	0.0031	0.0835	0.0835
Panel B: Dependent variable: minutes worked in excess of schedule				
Minority worker ×	−3.295**	−3.279**	−3.327*	−3.237*
manager bias	(1.550)	(1.588)	(1.687)	(1.678)
Manager bias	−0.002	−0.002	−0.005	−0.005
	(1.141)	(1.167)	(0.969)	(1.009)
Minority worker ×				0.349
minority nanager				(10.501)
Minority manager				−3.712
				(4.592)
Observations	4,163	4,163	4,163	4,163
Dependent variable mean	−0.068	−0.068	−0.068	−0.068
R-squared	0.001	0.008	0.129	0.129
Individual fixed effects	Yes	Yes	Yes	Yes
Day of the week fixed effects	No	Yes	No	No
Morning/evening fixed effects	No	Yes	Yes	Yes
Date fixed effects	No	No	Yes	Yes

Note. Each column in each panel shows the result of regressing the dependent variable on the interaction of the worker's minority status and the manager's IAT score (in standard deviation terms), controlling for the manager's IAT score and worker fixed effects. The dependent variable in Panel A is an indicator for the worker being absent. The dependent variable in Panel B is the number of minutes worked in excess of the number of minutes the worker was scheduled to work. The first column includes no additional controls. The second column adds day of the week fixed effects, an indicator for the shift starting at 9 am or earlier, and an indicator for the shift ending at 8 pm or later. The third column includes date fixed effects and drops the day of the week fixed effects. The last column adds a dummy for the manager being a minority and the interaction of the worker's and the manager's minority status. Observations are worker-days and standard errors are clustered at the store level. *,**, *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table III, Panel A shows the effect of manager bias on absence rates. The first column adds no controls, X_{ist}. It shows that working with a manager with an IAT score one standard deviation higher leads minorities to have an absence rate 1 percentage point higher. The effect is large (70% of the mean) even before measurement error corrections. The subsequent two columns add (i) day of the week fixed effects and controls for the shift starting in the early morning and ending in the late evening and (ii) date fixed effects,

respectively. Adding these controls does not change the coefficient of interest.

The final column includes as controls a dummy for the manager being a minority and an indicator for the worker and the manager both being minorities. Because there are so few minority managers, we do not estimate these terms precisely. However, including these terms does not change the coefficient of interest (nor does simply eliminating days with minority managers). Thus, the effect of working with a biased manager appears to result from the manager's bias, not the manager's group affiliation.

Throughout the panel, the measured effect of working with a biased manager for majority workers is negative, suggesting that nonminorities are less likely to be absent when scheduled to work with biased managers. However, this effect is always insignificant and smaller than the effect for minority workers.

We next investigate the effect of working with biased managers on the amount of time spent at work. Table III, Panel B replicates Panel A where the dependent variable is the number of minutes the CP worked in excess of the number of minutes she was scheduled to work. These regressions exclude days the worker was absent.[21]

The panel shows that even when not absent, minorities work less when paired with a biased manager. When working with a manager one standard deviation more biased, they work about 3.3 fewer minutes (one twelfth of a standard deviation before correcting for measurement error). As in the prior panel, the result is robust to the addition of controls and is not driven by the manager's minority status. However, unlike in the previous panel, the point estimates do not suggest that majority workers spend more time at work when scheduled to work with biased managers.

Aside from being absent, there are three main ways that a worker could spend less time at work: she could arrive later, leave earlier, or take longer breaks. We find that working with a biased manager primarily leads minorities to leave work earlier.

21. Because we eliminate days workers were absent, these regressions could be biased. For this bias to be driving our results, it would have to be that minority workers would have chosen to work more than average (relative to their schedules) on the days that being scheduled to work with a biased manager led them to be absent. Instead, we believe that any bias likely attenuates our results. It seems reasonable that the days that minority workers were absent as a result of being paired with a biased manager are days that they would have worked relatively less had they arrived.

Online Appendix Table A.5 presents regressions similar to those in Table III, where the dependent variables are different aspects of time spent at work. There is no significant effect of manager bias on arrival time or break time. However, a minority worker paired with a manager one standard deviation more biased left the store 3.7 minutes earlier on average. Minorities were not more likely to leave before the end of their shift when working with biased managers. But they were substantially less likely to stay after.[22] Biased managers may be less likely to ask minority workers to stay late or minorities may be less likely to agree when working with biased managers.

Although we do not have information on workers' pay, we can use estimates of time spent at work to estimate how much more minorities would earn (relative to majorities) if they worked only with unbiased managers. Reestimating the minutes worked regression including days that workers were absent suggests that for each standard deviation increase in manager bias minorities spend eight fewer minutes per day at work. Eliminating manager bias would thus increase the time minorities spent at work and their pay by 2.5% (before correcting for measurement error).

IV.B. Performance while at Work

We examine the effect of manager bias on minority performance while at work, first considering the number of articles scanned per minute. This is one of the performance metrics over which workers have the most control and the firm cares most about. In each store, a list of workers' average articles scanned per minute is posted in the break room each week.

22. Consistent with this, when we include in our regressions the bias of the manager on duty at shift end instead of the day's average manager bias, the effect of manager bias on minutes worked more than doubles to −6.71 (without controls) and −7.18 (with our full controls). However, neither of these estimates is significant at conventional levels, with p-values of .132 and .114, respectively. Online Appendix Figure A.1 shows the measured effect of manager bias on minorities staying different lengths of time after the shift ends. Manager bias appears to decrease the number of minutes minorities stay after their shifts throughout the distribution. For example, it decreases the probability that a minority will stay at least an hour and a half after her shift ends by 1.5 percentage points. If these minorities who would have stayed at least 90 minutes after the end of their shifts instead leave at shift end, a decrease in these longer shifts would account for approximately half of the overtime effect.

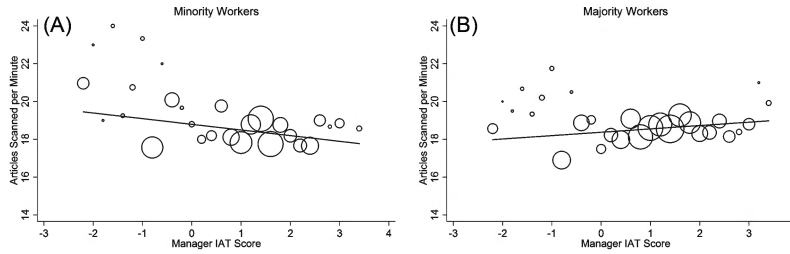

FIGURE I

Manager Bias and Worker Performance

The size of each marker indicates the number of observations in the bin.

On average, CPs scan 18.5 articles per minute. There are not large day-of-the-week effects in scanning speed, except that workers are exceptionally slow (1.2 articles per minute slower) on the few occasions when they have to work Sundays. Workers also scan articles more slowly on shifts that begin in the early morning.

Before we show our causal estimates, Figure I plots the relationship between manager bias and articles scanned per minute for minorities (Panel A) and majorities (Panel B). Each point plots the average articles scanned per minute on days that workers faced the indicated level of bias. Manager IAT scores are aggregated into bins of 0.2 and the size of the point indicates the number of observations in each bin. These graphs do not remove any individual or store fixed effects, so differences in scanning speed may result from cross-store differences in the types of workers hired or items purchased, instead of manager bias. Nonetheless, these graphs tell the same story as the regressions: minorities tend to scan slower when working with more biased managers. Majority workers appear to scan more quickly when working with more biased managers, but the effect for majorities is smaller than the effect for minorities.

Table IV, Panel A replicates the format of Table III, Panel A, showing the effect of manager bias on scanning speed. To the extent that cashiers' performance at work is affected by the bias of managers they actually work with (as opposed to the bias of the managers they were scheduled to work with), these regressions can be thought of as the reduced form for instrumental variables regressions in which the bias of the scheduled manager

TABLE IV
EFFECT OF MANAGER BIAS ON PERFORMANCE AT WORK

Panel A: Dependent variable: articles scanned per minute				
Minority worker ×	−0.276**	−0.279**	−0.233**	−0.249**
manager bias	(0.109)	(0.111)	(0.108)	(0.111)
Manager bias	0.140*	0.140	0.080	0.102
	(0.083)	(0.083)	(0.065)	(0.073)
Observations	3,601	3,601	3,601	3,601
Dependent variable mean	18.53	18.53	18.53	18.53
R-squared	0.001	0.013	0.195	0.195
Panel B: Dependent variable: inter-customer time (seconds)				
Minority worker ×	1.213**	1.228**	1.417**	1.360**
manager bias	(0.590)	(0.553)	(0.649)	(0.665)
Manager bias	−0.648	−0.571	−0.656	−0.580
	(0.386)	(0.376)	(0.521)	(0.534)
Observations	3,287	3,287	3,287	3,287
Dependent variable mean	28.70	28.70	28.70	28.70
R-squared	0.001	0.013	0.195	0.195
Individual fixed effects	Yes	Yes	Yes	Yes
Day of the week fixed effects	No	Yes	No	No
Morning/evening fixed effects	No	Yes	Yes	Yes
Date fixed effects	No	No	Yes	Yes
Manager minority variables	No	No	No	Yes

Note. Each regression shows the result of regressing the dependent variable on the interaction of the worker's minority status and the manager's IAT score (in standard deviation terms), controlling for the manager's IAT score and worker fixed effects. The dependent variables are the number of articles scanned per minute (Panel A) and the average number of seconds between finishing one customer's transaction and starting to scan the next customer's items (Panel B). The first column includes no controls. The second column adds day of the week fixed effects, an indicator for the shift starting at 9 am or earlier, and an indicator for the shift ending at 8 pm or later. The third column includes date fixed effects and drops the day of the week fixed effects. The last column adds a dummy for the manager being a minority and the interaction of the worker's and the manager's minority status. Observations are worker-days and standard errors are clustered at the store level. *, ** denote significance at the 10% and 5% levels, respectively.

instruments for the bias of the manager on duty. These two bias measures have a high correlation (0.93).

Being scheduled to work with a manager one standard deviation more biased leads the average minority worker to scan 0.28 fewer items per minute (Table IV, Panel A). (The standard deviation of articles per minute is 2.9.) Unreported regressions show that manager bias does not appear to induce minorities to perform extremely poorly (in the bottom 15%), but otherwise it affects the entire distribution of performance: from making minorities more likely to perform poorly (in the bottom 25%) to making them less likely to perform extremely well. The coefficients indicate that biased managers may cause majority workers to scan more quickly,

though this effect is only significant at the 10% level and in only one of the specifications.

Table IV, Panel B investigates the effect of manager bias on inter-customer time: the amount of time that a cashier spends between finishing one customer's transaction and starting to scan the next customer's items. Although this is not an often-discussed performance metric in the store, it directly affects the speed at which the lines move. On average, workers spend just under 30 seconds between customers. Working with a manager one standard deviation more biased leads minority workers to spend about 1.2 more seconds (one tenth of a standard deviation or 4% longer) between customers.

In contrast, there is no effect of manager bias on payment time: the time between the scanning of a customer's final item and the end of the transaction, during which the customer is paying (Online Appendix Table A.6). Payment time may largely depend on the customer.

Our results on the effect of manager bias are not driven by the other manager characteristics in our data: manager position in the firm, age, or gender (Online Appendix Table A.7). Online Appendix Table A.8 shows the results are robust to using different definitions of minority status. Panel A considers as minorities only workers with either a first or last name of North African origin (and eliminates remaining workers with names of Sub-Saharan African origin), whereas Panel B does the reverse. The effects of manager bias on workers of North and Sub-Saharan African origins are similar. Panel C uses the original definition of minority workers, but considers as majority workers only workers who have both a first and last name of European origin (eliminating workers of indeterminate, mixed, or other origins). The results are virtually unchanged. The results are also robust to eliminating managers who are unbiased or biased in favor of minorities and to using a wild cluster bootstrap, a method suggested for small numbers of clusters (Cameron, Gelbach, and Miller 2008).[23]

An alternative explanation for our results is that CPs' performance does not respond to managers but responds instead to senior cashiers who tend to work disproportionately with certain

23. Using a wild cluster bootstrap, the p-values for the coefficients on the minority worker $\times$ manager bias term in the first columns of Tables III and IV are .016 (absences), .009 (minutes worked), .049 (articles scanned per minute), and .093 (inter-customer time).

managers. More biased workers may sort toward more biased managers or senior minority workers may sort away from more biased managers.[24] It is difficult to test this theory directly because we do not have data on senior cashiers (just managers and CPs). However, we think this explanation is unlikely. Although senior cashiers have some control over their schedules, they can only submit preferences over the times they work, not the people they work with. A worker could attempt to work with a particular manager by requesting certain shifts that the manager prefers. For example, if a biased manager tended to work Monday mornings, but not Tuesday afternoons, biased senior cashiers who wanted to work with this manager could request these times. There is a limit to how much workers can control their schedules: because the firm values everyone doing their "fair share" of different kinds of shifts, it has included this as a constraint in the assignment algorithm.

Online Appendix Table A.9 shows that the measured effects of manager bias on minority performance are virtually identical if we control for shift (day of the week × morning or evening) within store and thus the likely sorting of senior cashiers. While senior workers could sort toward managers in a more sophisticated way, the fact that this most likely method explains none of the effect of manager bias suggests that senior cashier sorting does not drive our results.

Finally, in Online Appendix A, we examine heterogeneity in the impact of biased managers on minority workers' performance. We find that manager bias has larger impacts in the latter part of the contract. Its effects also appear to be concentrated in less diverse stores (Online Appendix Table A.10).

Why do firms employ biased managers given that biased managers negatively impact minority performance? One explanation is that biased managers do not decrease average performance. Regressing worker performance on manager IAT scores indicates that biased managers don't generate worse average worker performance. While biased managers depress the performance of minority workers, minorities are a small share (28%) of the entire

24. Even though we do not think this describes what is happening in the stores, this alternative explanation might have similar implications to manager bias directly depressing minority performance. If minority performance is negatively affected by more biased senior workers, then the bias of more senior store personnel would still be harming minority workers. If senior minority workers prefer not to work with biased managers, then this explanation would still include manager bias affecting minorities' work patterns.

workforce. For three out of the four main outcomes, point estimates suggest that biased managers (insignificantly) improve majority worker performance. Because managers can choose when they work, the estimates of manager bias on overall worker performance may include the effects of different store conditions and do not necessarily isolate managers' effects on worker performance. Nevertheless, adding controls for shift characteristics does not change the estimates.

V. Evidence on Mechanisms

In this section, we discuss several theories of discrimination and their predictions for our context. We then use worker survey and administrative data to test these predictions. We break these theories into two types: (i) theories that explain why minorities perform worse when working with biased managers and (ii) theories that explain the firm's hiring decisions, and discuss them in turn.

V.A. Effects of Manager Bias on Minority Performance

We consider only theories that can explain why minorities are absent more often, leave work earlier, and have worse on-the-job performance on days when they work with more biased managers. For example, although manager bias may impede minority skill development (see, for example, Lundberg and Startz 1983 and Coate and Loury 1993), our study cannot assess whether this occurs. Even if it did occur, minorities would have similar skills on days when they worked with both more and less biased managers.

1. Theories.

Animus. Biased managers may simply dislike minorities. Animus could lead biased managers to treat minorities badly and give them unpleasant tasks, thereby causing minorities to dislike coming to work. Minorities would be absent more often and leave work earlier. Animus could also impede minority on-the-job performance. To test whether manager animus drives our results, we assess whether, in the worker survey, minorities report that biased managers liked them less and assigned them to unpleasant tasks.

Less Interaction. Individuals with higher implicit biases toward a minority group have been found to spend less time talking

to, have more hesitation in speaking to, and act less friendly towards minority group members (McConnell and Leibold 2001; Dovidio, Kawakami, and Gaertner 2002; Hebl et al. 2002). Biased individuals may be uncomfortable interacting with minorities or their actions can be driven by a desire to seem unprejudiced (see Dovidio and Gaertner 2008 for a summary of the literature on aversive racism). Alternatively, biased managers may believe minorities are so unproductive that there are low returns to expending managerial effort on them. As a result, biased managers may spend less time at minority workers' stations. When they need a task accomplished—even an unpleasant one—they may be less likely to ask a minority. Minorities may realize which managers are paying less attention to them and exert less effort when these managers are on duty. We use the worker survey linked to administrative data to test whether (i) biased managers interact less with minority workers, (ii) in our context, more worker-manager interaction correlates with better worker performance and (iii) biased managers are less likely to assign minorities to tasks whose assignment requires interaction with workers.[25]

Self-Stereotyping or Stereotype Threat. Under self-stereotyping (Coffman 2014), workers' expectations about their group's suitability for a given task affect their performance. Here, minorities might think that workplace environments (even relatively low-skill ones) are not environments where minorities thrive. Biased managers may activate these negative stereotypes. Relatedly, under stereotype threat (Steele and Aronson 1995), the risk of confirming negative group stereotypes leads minorities to become anxious and perform worse. To assess the extent to which managers differentially activate self-stereotypes or trigger stereotype threat, we asked workers the extent to which different managers made them feel confident in their abilities. While this tests for conscious activation of stereotypes, it would not capture stereotypes activated subconsciously.

2. Evidence. The evidence seems inconsistent with manager animus. Minorities do not perceive biased managers as disliking them. In the worker survey, we asked workers to rank their

25. Both the animus and less interaction theories are potentially consistent with Becker's theory of taste-based discrimination. That is, both can arise from personal prejudice (the foundation of Becker's model) leading biased managers to treat minorities differently.

TABLE V

WORKER-MANAGER AFFECTION AND TASK ASSIGNMENT

Panel A: Worker-manager affection

	Manager liked you best	Manager most likely to recommend you for promotion	You enjoyed working with manager best	Manager initially made you feel most confident
Minority worker × manager bias	0.019	0.078	0.243	0.194
	(0.246)	(0.212)	(0.234)	(0.196)
Manager bias	0.152	0.251*	−0.061	0.134
	(0.131)	(0.148)	(0.162)	(0.127)
Observations	3,036	2,862	3,209	3,189
Dependent variable mean	3.991	4.053	4.062	4.073
R-squared	0.015	0.042	0.010	0.026

Panel B: Task assignment

	Manager assigned to preferred register type	Manager assigned best breaks	Management of lines and customer flows encouraged performance	Manager assigned to fewest cleaning duties
Minority worker × manager bias	−0.035	0.146	−0.153	0.673***
	(0.391)	(0.469)	(0.308)	(0.189)
Manager bias	0.021	−0.083	0.129	−0.276
	(0.157)	(0.146)	(0.137)	(0.182)
Observations	2,288	2,553	2,864	2,235
Dependent variable mean	4.010	3.922	4.215	3.373
R-squared	0.002	0.008	0.018	0.045

Note. Each column in each panel shows the result of regressing the dependent variable on the interaction of the worker's minority status and the manager's IAT score (in standard deviation terms), controlling for the manager's IAT score and worker fixed effects. The dependent variable is the worker's ranking of the manager on the question indicated by the column heading. This ranking ranges from 1 (the lowest ranked manager) to N (the highest ranked manager), where N is the number of managers the worker had. Observations are worker-days and standard errors are clustered at the store level. *, *** denote significance at the 10% and 1% levels, respectively.

managers on the extent to which the manager liked the worker and the manager was likely to recommend the worker for promotion. We use workers' answers to order managers from 1 (the lowest ranked) to N (the highest ranked).[26] The first two columns of Table V, Panel A, show the results of estimating equation (2) with workers' rankings of their managers on these dimensions as the dependent variables. While neither coefficient is significant, both are positive, suggesting that if anything minorities

26. We eliminate from this ranking managers workers indicated they did not remember since workers almost never ranked these managers. We analyze whether workers remembered their managers separately.

perceived biased managers as liking them better. We also asked workers to rate how much they enjoyed working with each manager. Minorities again rated biased managers insignificantly more positively.

To assess whether biased managers activated minorities' negative self-stereotypes or triggered stereotype threat, we asked workers which managers initially made the worker feel most confident in their abilities. There is no evidence that biased managers made minorities anxious about confirming stereotypes or activated self-stereotypes of poor performance: minorities rated biased managers as making them feel insignificantly more confident about their abilities (Table V, Panel A). As discussed already, this does not rule out subconscious stereotype activation.

Panel B analyzes task assignment. Animus would lead biased managers to assign minorities to unpleasant tasks more often, while if biased managers avoid interacting with minorities, they would assign minorities to additional tasks less often, regardless of task pleasantness. The first two columns of Table V, Panel B, examine workers' register assignments and the desirability of their break times.[27] Since all cashiers need to be assigned to a register and given breaks, these two assignments test for animus, but not whether biased managers avoid interacting with minorities. We find that biased managers do not assign minorities unpleasant registers or break times.

The final column considers assignment to cleaning duties. Telling a worker to shut down her register and start cleaning requires interaction. Moreover, cleaning is typically considered to be cashiers' least pleasant task. Consistent with biased managers avoiding minorities but inconsistent with animus, biased managers are significantly less likely to assign minorities to cleaning duties.[28] The interaction theory can also explain why biased managers may be less likely to ask minorities to stay late as well as why the effect of manager bias is larger in stores with less diversity: it may be more difficult for managers to avoid minority workers when a larger share of the workforce is minority.

To further test the interaction theory, we asked CPs to rate the extent to which they remembered each manager. We utilize

27. Managers choose when workers can take their breaks, but not how much break time workers receive, which is stipulated in workers' contracts.

28. Managers worried about appearing biased might be particularly concerned with the visuals of assigning minorities to clean.

remembering a manager (ranking the manager at least 2 out of 10) as an indicator of the amount of interaction the worker and manager had. Workers performed much better when working with managers with whom they interacted more. The first column of Table VI, Panel A shows that workers scanned 1.5 more articles per minute when working with a manager they later remembered. However, workers did not perform better when working with managers they had been scheduled to work with more often (second column), nor does the effect of remembering the manager on worker performance decrease when we control for the amount of time spent working together (third column) or manager fixed effects (fourth column). Taken together, this suggests that worker-manager interaction within a shift leads workers to perform substantially better. Mas and Moretti (2009) similarly find that cashiers exert more effort when their performance is being noticed by coworkers they value.

Minorities report interacting less with biased managers: they were about 1.5 percentage points less likely to remember a manager one standard deviation more biased (Table VI, Panel B).[29] The final columns in Panel B suggest how worker-manager interaction affects the measured impact of manager bias on minority performance. While the results are imprecise, when we limit the sample to days the worker remembered the manager, the effect of manager bias on minority performance falls by 25%.

Summarizing our results, we find the strongest evidence for the theory that biased managers interact less with minority workers and assign them to new tasks—even unpleasant ones—less often. This may be because they feel less comfortable around minorities, they are concerned with appearing biased, or they believe there is a low return to expending effort managing minorities. We find no evidence of animus: minorities do not report that biased managers disliked them or assigned them to unpleasant tasks. We have less clear evidence on whether self-stereotyping or stereotype threat plays a role: we do not find that biased managers made minorities consciously anxious about their abilities, but this does not rule out that they activated minority stereotypes on a subconscious level.

29. Unsurprisingly, workers were more likely to remember managers they were scheduled to work with more.

TABLE VI

WORKER-MANAGER INTERACTION

Panel A: Effect of worker-manager interaction on performance

	Dependent variable: articles scanned per minute			
Remembers manager (indicator)	1.510** (0.635)		1.587** (0.630)	2.053*** (0.744)
Fraction of time scheduled with manager		−1.172 (1.352)	1.724 (3.638)	4.021 (3.828)
(Fraction of time scheduled with manager)^2			−4.454 (4.886)	−6.603 (5.407)
Individual fixed effects	Yes	Yes	Yes	Yes
Manager fixed effects	No	No	No	Yes
Observations	1,885	1,885	1,885	1,885
Dependent variable mean	18.42	18.42	18.42	18.42
R-squared	0.008	0.001	0.010	0.095

Panel B: Minority status, manager bias, and worker-manager interaction

	Dependent variable: remembers manager (indicator)	Dependent variable: articles scanned per minute	
	All days	All days	Days where worker remembers manager
Minority worker × manager bias	−0.0152* (0.0086)	−0.415* (0.209)	−0.311 (0.314)
Manager bias	0.0190* (0.0097)	0.271** (0.114)	0.203** (0.095)
Fraction of time scheduled with manager	0.6362* (0.3351)	−1.932 (4.159)	−5.115 (4.161)
(Fraction of time scheduled with manager)^2	−0.5605 (0.3981)	1.017 (6.270)	4.114 (5.757)
Individual fixed effects	Yes	Yes	Yes
Observations	3,958	1,584	1,317
Dependent variable mean	0.932	18.52	18.66
R-squared	0.017	0.006	0.005

Note. Each regression in Panel A shows the results of regressing *articles scanned per minute* on the variables listed in the left-most column, controlling for worker fixed effects. *Remembers manager* is an indicator for the worker reporting in the worker survey that she remembered the manager she was scheduled to work with that day. *Fraction of time scheduled with manager* is a number between 0 and 1. It is the fraction of the worker's time in the administrative data that she was scheduled to work with the given manager, averaged over all working days. The first column of Panel B regresses an indicator for whether the worker remembered the manager on the interaction of the worker's minority status and the manager's IAT score (in standard deviation terms), controlling for the manager's IAT score, the fraction of time in the administrative data the worker spent with the manager, this fraction squared, and worker fixed effects. The next column in the panel regresses *articles scanned per minute* on these same variables. The final column in the panel replicates the previous column, but eliminates days where the worker did not remember the manager. Throughout, observations are worker-days and standard errors are clustered at the store level. *, **, *** denote significance at the 10%, 5%, and 1% levels respectively.

V.B. Hiring Decisions

Comparing the performance of minority and majority workers allows us to shed light on whether the firm engages in statistical or Beckerian (taste-based) discrimination in the hiring process.

1. Theories.

Statistical Discrimination. By making minorities less productive, manager bias may lead to statistical discrimination in hiring (Phelps 1972; Arrow 1973). Under statistical discrimination, the firm uses workers' observable characteristics and minority status to infer worker productivity. In this setting, wages are fixed and cannot depend on performance. Thus, the firm hires the workers with the highest expected productivities.[30] Even if minorities and majorities with the same observable characteristics are equally productive in the absence of manager bias, because manager bias depresses minority productivity, the firm infers that minority workers are less productive than majority workers with the same characteristics. To be hired, minorities would need better qualifications than hired majority workers and hired minorities would be more productive than hired majorities when not exposed to manager bias.

Beckerian Discrimination. Under taste-based discrimination, the firm doesn't necessarily have uncertainty about worker productivity. Instead, the firm (or its employees) faces a utility cost of employing minority workers. Because all hired workers must be paid the same wage, hired minorities need to have higher average productivity than hired majorities to compensate the firm for hiring them.

2. Evidence. Our evidence is most consistent with statistical discrimination, though we cannot fully rule out that the firm engages in taste-based discrimination.

We compare minority and majority workers on a summary measure of worker performance, the number of customers served, as well as on the individual performance metrics. To construct this summary measure, we combine the time spent at work with a worker's average articles scanned per minute, inter-customer time, and payment time. We assume that the average customer has 25 items, though our results are not very sensitive to this assumption. We also assume that cashiers spend all day at their

30. We assume the firm is risk neutral.

TABLE VII

COMPARISON OF MINORITY AND NONMINORITY PERFORMANCE

	Absence indicator	Minutes worked in excess of schedule	Articles scanned per minute	Inter-customer time (seconds)	Estimated customers served
Panel A: All days					
Minority worker	−0.0041	0.522	0.282	0.504	2.80
	(0.0072)	(2.213)	(0.329)	(0.719)	(2.02)
Nonminority mean	0.0187	−1.186	18.55	28.21	162
Observations	4,371	4,163	3,601	3,287	3,086
Store fixed effects	Yes	Yes	Yes	Yes	Yes
Panel B: Days with unbiased managers					
Minority worker	−0.0127*	2.572	0.745**	−2.075*	13.94**
	(0.0067)	(2.331)	(0.323)	(1.113)	(4.84)
Nonminority mean	0.0267	−4.268	18.65	26.59	162
Observations	482	444	367	330	301
Store fixed effects	Yes	Yes	Yes	Yes	Yes
Panel C: Days with biased managers					
Minority worker	−0.0047	0.271	0.006	0.936	2.21
	(0.0094)	(2.872)	(0.383)	(0.935)	(2.68)
Nonminority mean	0.0194	−1.106	18.65	27.94	162
Observations	3,474	3,319	2,832	2,555	2,395
Store fixed effects	Yes	Yes	Yes	Yes	Yes
Panel D: All other days					
Minority worker	0.0026	0.379	1.292**	0.178	−0.25
	(0.0027)	(1.625)	(0.325)	(0.186)	(2.05)
Nonminority mean	0.0036	0.928	17.69	31.88	156
Observations	445	429	422	421	410
Store fixed effects	Yes	Yes	Yes	Yes	Yes

Note. Each column in each panel shows the result of a separate regression of the dependent variable indicated by the column on an indicator for the worker being a minority, controlling for store fixed effects. *Estimated customers served* is calculated under the assumptions that customers average 25 items. Standard errors are clustered at the store level. *Days with unbiased managers* are days where the worker spent at least 50% of the day with managers with a raw (unscaled) IAT score between −0.15 and 0.15. *Days with biased managers* are days where the worker spent more than 50% of the day with managers whose raw IAT score exceeds 0.15. Days where a worker spent more than 50% of her time with managers biased in favor of minorities (managers with raw IAT scores below −0.15) and days where a worker did not spend more than 50% of her time with managers in any bias category are included in Panel D as *All other days*. *, ** denote significance at the 10% and 5% levels, respectively.

registers receiving customers. To the extent that this is not true, but relative performance differences are similar on other tasks, this can still be thought of as a summary measure of performance.

Table VII compares minority and majority performance by presenting estimates from the regression

$$y_{ist} = \alpha + \beta \text{minority}_i + \delta_s + \varepsilon_{ist}, \tag{3}$$

where y_{ist} is a metric of performance of worker i in store s on date t. As before, δ_s are store fixed effects. The coefficient of interest,

β, shows how minority workers' performance compares to the performance of majority workers in the same store. Panel A includes all days. It shows that minority workers' average performance is statistically indistinguishable from that of majority workers. We estimate that the average majority worker serves 162 customers a day. The average minority serves an additional 2.8 customers, a difference that is far from significant, and which places the average minority worker at the 53rd percentile of average worker performance. The similar average performance of minority and majority workers is (weak) evidence against taste-based discrimination. We do not find that hired minorities perform substantially better than hired majority workers and can rule out that they perform more than 4% better on average.

Panel B compares minority and majority workers' performance on days when they work with unbiased managers. (Because most managers are biased, minorities only work with unbiased managers on a small fraction of days.) On days when workers spend at least half their time with unbiased managers, minority workers perform substantially better than nonminority workers. They are approximately half as likely to be absent, they scan 0.75 more articles per minute, and they take two fewer seconds between customers. On days with unbiased managers, the average minority cashier serves 14 customers more a day than does the average majority. This 9% better performance places the average minority working with an unbiased manager at the 79th percentile of worker performance. That overall minority and majority workers perform similarly, but minorities perform substantially better when not exposed to manager bias is consistent with statistical discrimination.[31]

These facts would not be evidence of statistical discrimination if minority workers were simply intertemporally substituting effort toward days with unbiased managers. If they were fully intertemporally substituting, manager bias would not affect average minority performance, it would just lead minorities to

31. Statistical and taste-based discrimination predict that hired minorities should have better observable characteristics than hired majorities. We have only one pre-employment characteristic that should have a clear relationship with productivity: educational attainment. Minorities are more than three times as likely to have above high school education as majority workers (16% versus 5%) and this difference remains nearly as large (9.1 percentage points) when store fixed effects are added.

TABLE VIII

EFFECT OF MANAGER BIAS ON PERFORMANCE IN REST OF THE WEEK

Dependent variable: average performance in rest of the week				
	Absence indicator	Minutes worked in excess of schedule	Articles scanned per minute	Inter-customer time (seconds)
Minority worker × manager bias	0.0023	−0.628	−0.099**	−0.336
	(0.0020)	(0.940)	(0.041)	(0.274)
Manager bias	−0.0029	1.185*	0.063	−0.030
	(0.0017)	(0.638)	(0.046)	(0.203)
Observations	4,271	4,174	3,935	3,610
Dependent variable mean	0.0153	−0.053	18.43	29.04
R-squared	0.0010	0.0058	0.0061	0.0026

Notes. Each column shows the result of regressing the workers' average performance in the rest of the week on the interaction of the worker's minority status and the manager's IAT score (in standard deviation terms) on a given day. Regressions control for the manager's IAT score on that day and worker fixed effects. They also control for the average manager IAT score in the rest of the week and this score interacted with the worker's minority status. Standard errors are clustered at the store level. *, ** denote significance at the 10% and 5% levels, respectively.

perform worse on some days and better on others. That minorities performed better on days without bias would not indicate their higher general productivity. However, we provide two tests that suggest that minorities are not simply intertemporally substituting effort. Table VIII shows the effect of working with a more biased manager on one day on the worker's performance in the rest of the week.[32] Under intertemporal substitution, working with a more biased manager on one day should lead minorities to perform better in the rest of the week. We find no evidence that this is the case. In fact, minorities scan items significantly slower in the rest of the week when they spend one day with a more biased manager.

We can also look at workers' response to manager bias aggregated over longer periods (Online Appendix Table A.12). If a worker is intertemporally substituting her effort within a given period (e.g., at the week or two-week level), performance should be uncorrelated with manager bias at that level of aggregation. However, we find no evidence that the impact of manager bias is attenuated when performance is aggregated over longer periods. For absences, the measured effect of manager bias is relatively constant with the level of aggregation, though it is no longer statistically significant once the data is aggregated. For scanning speed, the measured effect increases with the level of

32. We control for manager bias in the rest of the week and its interaction with the worker's minority status to eliminate the effects of intra-week correlation in manager bias.

aggregation. Consistent with the results in Table VIII, this may indicate that there are some cumulative effects of manager bias on scanning speed.

VI. Conclusion

Working with biased managers leads minorities to perform more poorly. When scheduled to work with more biased managers, minority cashiers are absent more often and they leave work earlier. This depresses minority wages because workers are paid for time worked. Minorities also scan items more slowly and take more time between customers when working with biased managers. Biased managers do not appear to treat minorities poorly. Instead, they seem to simply interact less with minorities, leading these workers to exert less effort. By making minorities less productive, manager bias appears to generate statistical discrimination in hiring.

These results come from one setting: entry-level cashiers in a large French grocery store chain. However, they may be applicable to many other settings. In our setting, biased managers' discomfort with minorities can lead them to monitor minorities less, assign minorities to new tasks less frequently, and not ask minorities to stay late. In other settings, interacting less with minority employees may have larger consequences if it leads biased managers to train, mentor, advise, or challenge minorities less.

Our results raise the question of the type of policy responses that could be used to ameliorate the impacts of manager bias on minority workers. One set of potential policies would aim to directly reduce implicit bias. Beaman et al. (2009) finds that having female leaders reduces implicit bias against women. Outside of the workplace, Rao (2014) and Boisjoly et al. (2006) find that exposure to a group can reduce bias against it. Another set of potential policies would attempt to mitigate the effect of manager bias by directly targeting manager actions. For example, these interventions could encourage managers to interact with all workers equally or provide more specific guidelines about how to manage workers. Investigating the effects of such policies is an interesting question for future research.

Sciences PO
Harvard University and NBER
IRES, Université Catholique de Louvain

Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

References

Agerström, Jens, Rickard Carlsson, and Dan-Olof Rooth "Ethnicity and Obesity: Evidence of Implicit Work Performance Stereotypes in Sweden," Working Paper, IFAU-Institute for Labour Market Policy Evaluation, 2007.

Amodio, David M., and Patricia G. Devine "Stereotyping and Evaluation in Implicit Race Bias: Evidence for Independent Constructs and Unique Effects on Behavior," *Journal of Personality and Social Psychology*, 91 (2006), 652–661.

Arrow, Kenneth "The Theory of Discrimination," *Discrimination in Labor Markets*, 3 (1973), 3–33.

Banse, Rainer, Jan Seise, and Nikola Zerbes "Implicit attitudes towards Homosexuality: Reliability, Validity, and Controllability of the IAT," *Zeitschrift für Experimentelle Psychologie*, 48 (2001), 145–160.

Beaman, Lori, Raghabendra Chattopadhyay, Esther Duflo, Rohini Pande, and Petia Topalova "Powerful Women: Does Exposure Reduce Bias?," *Quarterly Journal of Economics*, 124 (2009), 1497–1540.

Becker, Gary S. *The Economics of Discrimination* (Chicago: University of Chicago Press, 1971).

Bertrand, Marianne, Dolly Chugh, and Sendhil Mullainathan "Implicit Discrimination," *American Economic Review: Papers and Proceedings*, 95 (2005), 94–98.

Boisjoly, Johanne, Greg J. Duncan, Michael Kremer, Dan M. Levy, and Jacque Eccles "Empathy or Antipathy? The Impact of Diversity," *American Economic Review*, 96 (2006), 1890–1905.

Bordalo, Pedro, Katherine Coffman, Nicola Gennaioli, and Andrei Shleifer "Stereotypes," *Quarterly Journal of Economics*, 131 (2016), 1753–1794.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller "Bootstrap-based Improvements for Inference with Clustered Errors," *Review of Economics and Statistics*, 90 (2008), 414–427.

Coate, Stephen, and Glenn C. Loury "Will Affirmative-Action Policies Eliminate Negative Stereotypes?," *American Economic Review*, 83 (1993), 1220–1240.

Coffman, Katherine Baldiga "Evidence on Self-Stereotyping and the Contribution of Ideas," *Quarterly Journal of Economics*, 129 (2014), 1625–1660.

Dee, Thomas S. "Teachers, Race, and Student Achievement in a Randomized Experiment," *Review of Economics and Statistics*, 86 (2004), 195–210.

——, "A Teacher Like Me: Does Race, Ethnicity, or Gender Matter?," *American Economic Review*, 95 (2005), 158–165.

Dovidio, John F., and Samuel L. Gaertner "New Directions in Aversive Racism Research: Persistence and Pervasiveness," in *Motivational Aspects of Prejudice and Racism*, Cynthia Willis-Esqueda, ed. (New York: Springer, 2008), 43–67.

Dovidio, John F., Kerry Kawakami, and Samuel L. Gaertner "Implicit and Explicit Prejudice and Interracial Interaction," *Journal of Personality and Social Psychology*, 82 (2002), 62–68.

Egloff, Boris, and Stefan C. Schmukle "Predictive Validity of an Implicit Association Test for Assessing Anxiety," *Journal of Personality and Social Psychology*, 83 (2002), 1441–1455.

Friese, Malte, Matthias Bluemke, and Michaela Wänke "Predicting Voting Behavior with Implicit Attitude Measures: The 2002 German Parliamentary Election," *Experimental Psychology*, 54 (2007), 247–255.

Garthwaite, Craig, Tal Gross, and Matthew J. Notowidigdo "Public Health Insurance, Labor Supply, and Employment Lock," *Quarterly Journal of Economics*, 129 (2014), 653–696.

Giuliano, Laura, David I. Levine, and Jonathan Leonard "Manager Race and the Race of New Hires," *Journal of Labor Economics*, 27 (2009), 589–631.
——, "Racial Bias in the Manager-Employee Relationship: An Analysis of Quits, Dismissals, and Promotions at a Large Retail Firm," *Journal of Human Resources*, 46 (2011), 26–52.
Green, Alexander R., Dana R. Carney, Daniel J. Pallin, Long H. Ngo, Kristal L. Raymond, Lisa I. Iezzoni, and Mahzarin R. Banaji "Implicit Bias among Physicians and Its Prediction of Thrombolysis Decisions for Black and White Patients," *Journal of General Internal Medicine*, 22 (2007), 1231–1238.
Greenwald, Anthony G., Debbie E. McGhee, and Jordan L. K. Schwartz "Measuring Individual Differences in Implicit Cognition: The Implicit Association Test," *Journal of Personality and Social Psychology*, 74 (1998), 1464–1480.
Greenwald, Anthony G., Brian A. Nosek, and Mahzarin R. Banaji "Understanding and Using the Implicit Association Test: I. An Improved Scoring Algorithm," *Journal of Personality and Social Psychology*, 85 (2003), 197–216.
Greenwald, Anthony G., T. Andrew Poehlman, Eric Luis Uhlmann, and Mahzarin R. Banaji "Understanding and Using the Implicit Association Test: III. Meta-Analysis of Predictive Validity," *Journal of Personality and Social Psychology*, 97 (2009), 17–41.
Hahn, Adam, Charles M. Judd, Holen K. Hirsh, and Irene V. Blair "Awareness of Implicit Attitudes," *Journal of Experimental Psychology: General*, 143 (2014), 1369–1392.
Haider, Adil H., Janel Sexton, N. Sriram, Lisa A. Cooper, David T. Efron, Sandra Swoboda, Cassandra V. Villegas, Elliott R. Haut, Morgan Bonds, Peter J. Pronovost, Pamela A. Lipsett, Julie A. Freischlag, and Edward E. Cornwell III "Association of Unconscious Race and Social Class Bias with Vignette-Based Clinical Assessments by Medical Students," *Journal of the American Medical Association*, 306 (2011), 942–951.
Hebl, Michelle R., Jessica Bigazzi Foster, Laura M. Mannix, and John F. Dovidio "Formal and Interpersonal Discrimination: A Field Study of Bias toward Homosexual Applicants," *Personality and Social Psychology Bulletin*, 28 (2002), 815–825.
Hjort, Jonas "Ethnic Divisions and Production in Firms," *Quarterly Journal of Economics*, 129 (2014), 1899–1946.
Hu, Xiaoqing, J. Peter Rosenfeld, and Galen V. Bodenhausen "Combating Automatic Autobiagraphical Associations: The Effect of Instruction and Training in Strategically Concealing Information in the Autobiographical Implicit Association Test," *Psychological Science*, 23 (2012), 1079–1085.
Kim, Do-Yeong"Voluntary Controllability of the Implicit Association Test (IAT)," *Social Psychology Quarterly*, 66 (2003), 83–96.
Lane, Kristin A., Mahzarin R. Banaji, Brian A. Nosek, and Anthony G. Greenwald "Understanding and Using the Implicit Association Test: IV. What We Know (So Far) about the Method," in *Measures of Attitudes*, Bernd Wittenbrink and Norbert Schwarz, eds. (New York: Guilford Press, 2007), 59–102.
Lundberg, Shelly J., and Richard Startz "Private Discrimination and Social Intervention in Competitive Labor Markets," *American Economic Review*, 73 (1983), 340–347.
Mas, Alexandre, and Enrico Moretti "Peers at Work," *American Economic Review*, 99 (2009), 112–145.
McConnell, Allen R., and Jill M. Leibold "Relations among the Implicit Association Test, Discriminatory Behavior, and Explicit Measures of Racial Attitudes," *Journal of Experimental Social Psychology*, 37 (2001), 435–442.
Mooney, Chris "Across America, Whites Are Biased and They Don't Even Know It," *Washington Post: Wonkblog*, December 8, 2014, available at https://www.washingtonpost.com/news/wonk/wp/2014/12/08/across-america-whites-are-biased-and-they-dont-even-know-it/.

Nosek, Brian A., Anthony G. Greenwald, and Mahzarin R. Banaji "The Implicit Association Test at Age 7: A Methodological and Conceptual Review," in *Automatic Processes in Social Thinking and Behavior*, J. A. Bargh ed. (New York: Psychology Press, 2007), 265–292.
Phelps, Edmond S. "The Statistical Theory of Racism and Sexism," *American Economic Review*, 62 (1972), 659–661.
Price, Joseph, and Justin Wolfers "Racial Discrimination among NBA Referees," *Quarterly Journal of Economics*, 125 (2010), 1859–1887.
Rao, Gautam "Familiarity Does Not Breed Contempt: Diversity, Discrimination, and Generosity in Delhi Schools," Working paper, 2014.
Rooth, Dan-Olof "Automatic Associations and Discrimination in Hiring: Real World Evidence," *Labour Economics*, 17 (2010), 523–534.
Rosenthal, Robert, and Lenore Jacobson "Pygmalion in the Classroom," *Urban Review*, 3 (1968), 16–20.
Smith-McLallen, Aaron, Blair T. Johnson, John F. Dovidio, and Adam R. Pearson "Black and White: The Role of Color Bias in Implicit Race Bias," *Social Cognition*, 24 (2006), 46–73.
Stauffer, Joseph M., and M. Ronald Buckley "The Existence and Nature of Racial Bias in Supervisory Ratings," *Journal of Applied Psychology*, 90 (2005), 586–591.
Steele, Claude M., and Joshua Aronson "Stereotype Threat and the Intellectual Test Performance of African Americans," *Journal of Personality and Social Psychology*, 69 (1995), 797–811.
Stoll, Michael A., Steven Raphael, and Harry J. Holzer "Black Job Applicants and the Hiring Officer's Race," *Industrial & Labor Relations Review*, 57 (2004), 267–287.