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More than two decades after employment discrimination was outlawed by the Civil Rights Act of 1964, the CEO of Shoney's restaurant chain entered one of its restaurants that had lagging sales and noticed many black employees in visible positions. Seeing that the customers were largely white, he sent a memo to the restaurant manager directing him to employ more whites up front. In 1993, this attempt to accommodate the CEO's perceptions of customers' discriminatory preferences resulted in a settlement for $132 million (Watkins, 1997).

Proponents of workplace diversity, in contrast to the CEO at Shoney's, have frequently claimed that demographic diversity is good for business (Cox, 1993; Bantel and Jackson, 1989). As did Shoney's CEO, they often claim that customers prefer to deal with employees who have similar demographics. The difference between these two sets of advocates of accommodating customer discrimination is that Shoney's CEO saw his potential customers as white, while diversity proponents assume the customer base is typically diverse. If customers are diverse and many customers prefer to deal with a demographically similar salesperson, then employee diversity can increase sales.

Diversity proponents and opponents also make conflicting claims about how employees' similarity with each other affects performance. For example, some claim diversity can improve creativity and increase information (e.g., Bantel and Jackson 1989; Jehn, Northcraft, and Neale, 1999; Watson, Kumar, and Michaelson 1993). When creativity and the presence of diverse information sources are important, diversity can improve performance whenever workgroups make decisions, regardless of the contact with or composition of customers. At the same time, other theories (reviewed below) emphasize how workforce diversity can reduce cohesiveness and communication among employees.

Given these conflicting hypotheses, the fundamental question about how these conflicting forces affect the performance of actual work-groups is unanswered. One reason for the continued lack of clarity is that no large-scale studies speak directly to these conflicting hypotheses. In this study, we use longitudinal evidence from more than 800 similar business establishments within a single very large employer to examine how the demographic match between customers and employees affects workplace performance. (Due to confidentiality restrictions, we are unable to mention the name or industry of the employer.) We also examine how employees' racial, ethnic, gender and age diversity affect workplace performance.

Following establishments over time, we can also see how changes in workplace demographics affect performance within a workplace. Our measure of workplace performance is an objective one of central importance to business: sales.

If economists could run a controlled experiment on diversity, we would want to replicate the same workplace, experimentally varying only employee demographics. Although demographics have not been randomized, the workplaces are members of national chains that by design attempt to hold fixed many confounding factors that might affect sales. The chains have attempted to replicate these workplaces in every significant U.S. market.

This paper establishes the distinction between diversity itself and the main effects of race, gender, and age. (Due to data limitations described below, we refer to the categories white, black, Asian, and Hispanic as “race,” although Hispanic is more accurately described as an ethnicity.) We use rich measures of diversity along multiple dimensions. Importantly, we identify diversity as a nonlinear effect of employee demographic shares. Because we examine a broad demographic span, with stores that have both female and male majorities as well as stores with both white and nonwhite majorities, we can identify diversity effects distinct from the main demographic effects.

To examine employee-customer matching, we use Census data on the demographics of the community (that is, potential customers). Because we often have multiple workplaces in one community, we are also able to control for the fixed features of a community. We separately analyze Hispanics and Asians who speak English versus those who do not, as employee-customer similarity can be more important when language is a potential barrier.

Our goal here is to show how sales are affected by workplace diversity and by the demographic match between workplace and community.

Theory

We first discuss theories that examine whether sales of a service business depend on the diversity of its employees because customers care about the demographics of those who serve them. We then turn to theories on how diversity may affect productivity by affecting the internal dynamics of the workgroup.

Employee-Customer Matches

Most theories of the employee-customer match are based on the importance of similarity. After discussing these theories, we then discuss several alternatives.

Similarity theories

Several related theories suggest that the match between employee and customer demographics can improve store performance. Important examples include social identity theory (Tajfel and Turner, 1986), similarity-attraction theory (Jackson et al., 1991; Tsui, Egan, and O'Reilly, 1992), social-categorization theory (Tajfel and Turner, 1986), and Becker's theory of customer discrimination (1957). In these theories, familiarity, the desire to consider similar people as holding desirable traits, and preferences to be near those one considers the "in group" lead to preferences for doing business with similar others.

A close match in demographic characteristics may also improve employees' understanding of customers' preferences (Jackson and Alvarez, 1992; Cox, 1993). Additionally, employees who are demographically similar to customers may have an easier time understanding how customer preferences change over time. Finally, some studies indicate that employees can also attract customers using connections within the community (Cox, 1993; Ibarra, 1992, 1995). That is, in many sectors (including the one we study), an employee's social ties often help bring customers to the workplace and increase sales to them.

Jennifer Lee (2001) has identified two additional motives for storeowners to hire employees who match customers' demographics in her study of retail stores in largely black neighborhoods. She has found that white and Korean shopkeepers face disputes (for example, about a returned item) that can quickly escalate and gain a racial tinge. Thus, storeowners in her inner-city sample prefer to have at least one black employee in the store to have someone who can defuse a tense situation without overtones of race. In addition, owners prefer that at least one black employee be visible at all times so that customers feel the store is "giving back" to the community where it is located.

When employee and customer demographics are similar, communication costs may fall. Jargon, slang, and speech patterns all vary by demographic group. Even among native English speakers, racial (Lang, 1986) and gender (Tannen, 1990) differences often make communication difficult.

These concerns about communication costs grow in importance when a large number of potential customers do not speak English well. Although most immigrants learn English rapidly (Friedman and DiTomaso, 1996), in many cities, large immigrant enclaves contain a substantial number of people who cannot or prefer not to speak English.

These motivations can all lead profit-maximizing employers to desire a workforce that is demographically similar to its customers. When search is costly for customers, they lead to the hypothesis that sales are higher when the workforce demographics are similar to customer demographics, notwithstanding the legal risk incurred by discriminating in employment.

Alternative Theories

The standard economic model of discrimination due to Becker does not distinguish between liking whites and disliking blacks: preferences are relative and the effects of similarity should be broadly proportional to the match of customers and employees. We go beyond this standard model to theoretically and empirically distinguish positive from negative discrimination. With "negative discrimination" customers of one race avoid stores with employees of other races (no matter how few). For example, if negative discrimination against blacks holds true, employing even a small number of blacks would reduce sales. Negative discrimination is tightly linked to theories of status and power. Demographic traits such as race and gender are tacit reflections of status in organizations (Kanter 1977; Nkomo, 1992; Ely, 1994). Racial and gender-based inequities in organizations are reinforced and justified by stereotypes and biases that ascribe positive characteristics and therefore a higher status to whites and males (Nkomo, 1992; Heilman et al., 1989).

In contrast, with "positive discrimination" customers are attracted to stores with at least a few employees of their own race (no matter how many). For example, a customer who speaks on Spanish primarily wants at least one employee to be working in the store who speaks Spanish. There are diminishing returns to having multiple Spanish-speaking salespeople. When customers have positive discrimination, stores maximize profits by having a few employees of every race. If these cases are common, we should see sales increasing as each nonwhite race's share rises above zero and then leveling off. We test these variants below.

Evidence that Customers Prefer Similar Employees

To sum up, hypotheses drawn from a number of social sciences imply profit-maximizing employers may desire a workforce that is demographically similar to its potential customers. In spite of the many theories supporting this idea, the evidence for this effect is generally weak, with one important exception.

For example, the literature on marketing contains several small-scale studies that offer a mixture of results with no clear pattern that sales are higher when customer and employee demographics are similar (e.g., contrast Churchill, Collins, and Strang (1975) with Dwyer, Richard, and Shepherd (1998).

Some evidence from other spheres indicates that "customers"--when broadly defined in non-retail settings-- do better with demographically similar service providers. One randomized experiment indicates that students learn more when teachers are of the same race (Dee, 2001). A nonrandomized study suggests patients are more involved in their care when their doctors are of the same race (Cooper-Patrick et al., 1999).

Other studies examine employee-customer similarity but do not look at actual sales performance. For example, one important study indicates that newly hired low-wage workers who have direct contact with customers are more likely to match the demographics of those customers than are new hires who have no customer contact (Holzer and Ihlanfeldt, 1988). Similarly, employers as different as federal agencies (Borjas, 1982) and restaurants (Neumark, 1996) have been shown to hire workforces that approximate that of their clients. Employers here are acting as if customers discriminate.

The evidence for customer discrimination is strongest for professional sports. For example, studies find that white players' baseball memorabilia sells for more than the memorabilia of similarly accomplished black players (e.g., Andersen and La Croix, 1991; Nardinelli and Simon, 1990; and Gabriel, Johnson, and Stanton, 1999, but not 1995). In addition, white basketball players have been shown to attract more fans than do black players of similar quality, which presumably contributes to whites' higher pay (Kahn and Sherer, 1988). Also, professional basketball teams in cities with a high proportion of white residents typically employ a high proportion of white players (Burdekin and Idson, 1991). In football, there is no racial wage gap, but white players earn more in cities with a high proportion of whites, and nonwhites earn more in cities with a high proportion of nonwhites (Kahn, 1991).

The evidence above documents two important points. First, academics have only little evidence, and what we have is mixed, evidence as to whether customers prefer to be served by similar others in retail and service occupations, although the evidence is more consistent in other spheres. Second, employers often act as if customers have this preference.

Despite the lack of consistent evidence, proponents of diversity routinely advocate that employers must hire a diverse workforce to attract diverse customers. Examples can be found in trade publications including those serving marketing departments (Bertagnoli, 2001), stock brokerages (Lee, 2000)**,** voluntary associations (Baker, 1999), restaurants (Lieberman, 1998), real estate (Liparulo, 1998), healthcare providers (Chyna, 2001), and many others.

Advocating discriminatory customer preferences as a rationale for hiring nonwhite workers is an ironic twist in the history of American race relations. For much of the last 300 years, proponents of segregation have proposed that customers prefer to be served by similar others. The foundation of the fight against discrimination has been the proposition that individuals be treated as individuals, rather than on the basis of their membership in a demographic group. The theories are the same, but the older proponents of segregation assumed most customers were white, while many modern proponents of diversity assume customers are racially diverse.

Effects of Diversity Within the Workplace

Even if diversity does not affect business performance through customer preferences, we need to ask if it still has direct productivity effects by affecting how employees work with each other in groups or teams.   In this section, we document that both the theory and evidence on how employees' similarity with each other affects performance show mixed results.[1]

First, theories of diversity emphasize that diversity can have both positive and negative effects. Studies indicate that diverse teams can help performance because they are more likely to have the information needed to solve any given problem (Lazear, 1998), come up with more creative solutions than do homogeneous groups (Thomas and Ely, 1996; Nemeth, 1985), and are more likely to have employees with insights into the needs of customers (Thomas and Ely, 1996). At the same time, diversity can increase the costs of communication within the workforce (Lang, 1986; Zenger and Lawrence, 1989), lower group cohesiveness (Pfeffer, 1983), increase

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[1] Williams and O'Reilly (1998) and Reskin et al. (1999) provide excellent recent reviews of demographic research in organizations.

employee turnover (O'Reilly et al., 1989; Jackson et al., 1991), and reduce incentives for cooperation (Greif, 1993).

Given the contradictory theories and the mixed evidence surrounding diversity's effects, it is crucial to examine directly how diversity affects retail store performance.

Data and Methods

In this study we examine over 800 workplaces and over 70,000 employees of a single large service-sector employer. To test the effect of employment demographics on performance, an ideal experiment would randomly vary demographics while holding all other possibly confounding factors fixed. Studies of employment are bedeviled by unmeasured differences in policies, practices, and the working conditions across different employers. Although we do not have random demographics, we come close to achieving most of the data needs for a study of employee diversity and employee-customer match. In particular, our design minimizes unmeasured differences across workplaces.

In most field studies, demographics are highly correlated with other features of the workplace or job; for example, female-dominated occupations and establishments typically involve quite different tasks than do those dominated by males. The workplaces in our study, however, exhibit almost none of this variation. Each workplace has minimal local discretion, as each must implement the detailed human resource policies disseminated from corporate headquarters. Wages, internal hierarchy, fringe benefits, job content, and product and service prices, are for the most part centrally set and uniformly implemented. As is common among national chains that promote a common brand image, the employer has purposefully attempted to replicate the same outlet characteristics in every U.S. market of significance. Advertising, product selection, pricing, and human resource policies are all centrally determined to promote uniformity. The employer’s goal is that customers and employees perceive workplaces in different locations as essentially interchangeable. The remaining variation is far less than would be observed across most other jobs, employers, or industries. This standardization limits possible confounds between demographics and omitted job, product, or establishment characteristics.

As the establishments we analyze are dispersed across the United States, location-specific factors may affect both demographics and sales. For example, inner-city establishments may have both low sales and a high percentage of minority employees without any direct causal link. We use specifications designed to capture fixed features, measured or not, of the workplace, labor market, and customers. A local labor market shock might affect both changes in demographics and changes in sales; thus, in some specifications, we include a community fixed effect when examining changes in sales.

Additionally, this study unpacks the concept of diversity into a number of theoretically and empirically distinct measures. Most previous studies have had no workplaces with female, black, or Hispanic majorities. The limited range of data implies that a single diversity measure conflates both a main effect (such as rising percent female) and gender diversity. The data used in this study are unique among studies of organizational demography in having a sufficiently large sample size and sufficiently varied workgroup compositions to examine both diversity and the main effect of percent female, percent black, and percent Hispanic. While field research usually involves trading a smaller number of observations for greater depth, this study examines over 800 workplaces. This figure is roughly the total number of natural work groups in *all* the field studies reviewed by Williams and O'Reilly (1998).

Against these virtues we must count the limitations of this study, detailed in the Discussion section. Most importantly, this is a case study of one large employer in the low-wage service sector. Although not representative of all employers, this case study provides a cleaner study design with results that are plausibly applicable to a large sector of the U.S. workforce.

Specification

We first model the match between a store and a community, and then enrich the model to account for within-store diversity. We assume that the current match between a store and its community determines the current level of sales in a store. Equation 1 presents a simple reduced-form empirical specification where sales at store *i* in community *c* at time *t* depend on store demographics ($demog_{ict}$) such as the proportion Hispanic, other store observable characteristics (X_{ict}), community demographics ($demog_c$) such as the proportion Hispanic in the community, other community observable characteristics such as the distribution of household income (Z_c), and time effects[2] (*time*):

1) $$S_{ict} = a + b_0\, time + b_1 X_{ict} + b_2 Z_c + b_3\, demog_{ict} + b_4\, demog_c + b_5\, demog_{ict} \cdot demog_c + e_{ict.}$$

[2] We control for each sample month.

While each store has a unique community, we will take advantage of the fact that many communities have multiple stores. For the theories of store-community match, the coefficient of interest is b_5, which tells us if adding more Hispanics to a store (for example) is more useful in areas with a high proportion Hispanic. For example, if b_5 is positive, then moving from 3 to 30 percent Hispanic employees in a community that is 20 percent Hispanic will increase sales more than the same shift in employee demographics in a community with 2 percent Hispanics.[3]

The main effect on store demographics b_3 captures worker characteristics correlated with race (for example, if whites attend better high schools than nonwhites) and characteristics of the neighborhood that predict what groups would choose to work in this sector (white men may work in low-wage retail more often when labor markets are weak). These are of secondary interest here. The main effects also capture customer discrimination that is shared by all demographic groups. For example, in our society, all demographic groups may prefer to be served by certain groups; either high-status groups or (if people prefer to have service people fit stereotypes) by low-status groups. Because the main effects on mean age, race and gender conflate these several forces, the coefficients on the main effects are open to a variety of interpretations.

One problem with estimating equation (1) is that the residual e_{ict} is probably correlated with unobservable features of the store and community. Specifically, assume the residual includes unmeasured store characteristics that are fixed (u_i), unmeasured community characteristics that are fixed (v_c), as well as a white noise residual ε_{ict}:

(2) $$e_{ict} = u_i + v_c + \varepsilon_{ict}.$$

If the persistent but unobserved determinants of a store's characteristics v_c are correlated with both sales and employee demographics, then estimates of the employee demographic coefficients in equation (1) will be biased. For example, if blacks work in areas with low incomes (beyond the effect absorbed by our direct controls for community income), then the low incomes, not race, could reduce sales.

[3] As noted below, results using the absolute value of the gap in store and community demographics resemble those in the interaction specification (1). This absolute value of the gap is more sensitive to mismeasurement of the appropriate community and racial boundaries than the interaction we use.

To the extent that the factors affecting both demographics and sales are fixed, we can first difference equation (1) to eliminate the omitted store and community characteristics (u_i and v_c):

3) $$\Delta S_{ict} = b_0' + b_1'\Delta X_{ict} + b_3'\Delta demog_{ict} + b_5'\Delta demog_{ict} \cdot demog_c + \Delta\varepsilon_{ict}.$$

First differencing also eliminates all fixed observable factors concerning the stores and communities (Z_c and $demog_c$).

The first difference estimator in (3) analyzes only a portion of the variance contained in the pooled time-series cross-section regression (1). That is, the cost of eliminating omitted factors (u_i and v_c) is thatwe throw out most variation in store demographics. To balance this, we also examine the between-store component that averages each store's sales and characteristics over the sample period:

4) $$S_{ic} = a'' + b_1''X_{ic} + b_2''Z_c + b_3''demog_{ic} + b_4''demog_c + b_5''demog_{ic} \cdot demog_c + e_{ic}''$$

Compared with the first-difference estimator (3), this estimator captures more of the long-term relations between community and store demographics and store sales. Given that preferences across these specifications depend on a complex balance of judgments, we will present both the pooled specification and its components, the within and between specifications, and a formal test of the fixed effects model.

A possible problem with even the first-difference specification in equation (3) is that the omitted community factors may not be fixed over time. In the worst case, they change over time while affecting both workplace demographics and sales. For example, a store that is experiencing a positive demand shock may hire from demographic groups that it normally avoids. In this case, we could be spuriously attributing the effect of other evolving factors to demographics, biasing the coefficient estimates. Equation (5) presents the residuals in this case:

5) $$e_{ict} = u_i + v_{ct} + \varepsilon_{ict}.$$

Any remaining omitted variable bias due to local shocks can be resolved by adding detailed location-specific time*place interactions, exploiting the fact that many communities, indeed many ZIP codes have multiple stores. This specification corresponds to including a separate intercept for each ZIP code in the first differences version of a two-period panel:

6) $$\Delta S_{ict} = b_0 + b_1\Delta X_{ict} + b_3\Delta demog_{ict} + b_5\Delta demog_{ict} \cdot demog_c + ZIP_c + \Delta e_{ict}.$$

The resulting estimates of the interaction term b_5 can be thought of as answering the following question: Consider increasing the proportion Hispanic in one store in a community but not in a nearby store. Will that addition increase relative sales of the increasingly Hispanic store more if it takes place in a highly Hispanic region of the Southwest than if it takes place in a low-Hispanic portion of the Great Plains?

The strength of this estimator is that we have differenced out both fixed-store characteristics and community-level shocks that might affect both store demographics and sales. The cost is that double differencing removes most of the variation in sales and in demographics, so precision declines.

The estimates that use time series variation will have autocorrelated errors in the history of each store. We correct standard errors for first-order autocorrelation using the Prais-Winsten correction.

In addition, we add measures of the level (equations 1 and 4) or change (equations 3 and 6) of workplace diversity to each equation to study the effects of changes in how employees resemble each other.

An important question is what sources of variation remain after all of this differencing. These workplaces hire roughly three entire workforces a year, as is standard in entry-level jobs. Thus, natural fluctuations in who walks in the door will provide substantial variation in employment that is reasonably exogenous to sales. (In related research we examine in more depth how the race of managers affects the hiring and retention of workers of different races [xx].)

Finally, because of the strong advantage that may arise from speaking a foreign language when customers do not speak English, we test whether the presence of Hispanic employees predicts higher sales when many nearby residents speak Spanish but not English, while Asian employees predicts higher sales when many nearby residents speak Asian-Pacific languages but not English. This test is a straightforward extension of the above models augmented with the share of Hispanic employees interacted with the share of nearby residents who speak Spanish but not English and the share of Asian employees interacted with the share of residents who speak Asian-Pacific languages but not English (as well as main effects for each share). Our estimates will understate the benefits of employees who speak the language of linguistically isolated customers to the extent employees who self-identify as Hispanic do not speak Spanish. Moreover, even Asian employees who speak an Asian language may not speak the language of all non-English-speaking immigrants from Asia who live in the store's community.

The Setting

The employer is in an industry characterized by numerous small outlets that sell somewhat differentiated products. Each workplace we study is company owned and typically employs 15 to 40 part-time employees with several full-time managers and assistant managers. Because employees work scattered shifts through the week, they work with a changing mix of the other employees. Most frontline employees rotate through the several tasks in the store, spending some of their time dealing with customers and other time in support tasks.

Nonmanagerial employees receive minimal training when they are hired. These employees interact with each other to maintain stock and service customers, but these interactions are not complex. The Taylorist production techniques, with highly centralized decisionmaking and limited local discretion, may well limit the potential impact of any employee differences on productivity. Further enhancing the likelihood that diversity effects will be muted managers receive some training in managing a diverse workforce.

The employer hires a diverse workforce. This employment pattern arises partly because the employer has a reputation for gender and race diversity in its marketing and employment. In addition, in our interviews, managers noted that they hire many employees from among the ranks of customers. A diverse customer base leads naturally to, but does not fully determine, a diverse workplace.

Data

We combine employee-level data on demographics, store-level data on sales, and data from the 1990 Census on community characteristics. The employee data are the complete personnel records from February 1996 to October 1998 on over . We analyze data on frontline workplace employees, dropping workplaces with fewer than ten employees. We organize the data into store-month observations.

We complement our quantitative analysis with semistructured interviews of roughly a dozen employees and a half-dozen managers at workplaces scattered across one region of the country. These interviews were neither random nor a representative sample, but they do help flesh out the statistical analyses.

Store-Level Variables

The dependent variable is the natural logarithm of real monthly sales. In our first set of specifications, we analyze data pooled across stores over time. We then look only at variation between stores, averaging each store's sales over all available store-months. We next analyze variation within the history of each store, looking at year-on-year differences in monthly sales. Finally, we add ZIP code fixed effects to the regressions on sales growth.

From the company's human resource database, we construct a store-month dataset of employee demographics, including the proportion female, average age, and the shares of three categories for race or ethnicity (black, Asian, and Hispanic, with white, the small percentage Native American, and unknown ethnicity categories pooled as the baseline). The race and ethnicity codes are the company's coding, and they create a set of mutually exclusive and collectively exhaustive categories that for simplicity we refer to as "race." Educational requirements are minimal, and educational attainment varies little. Few employees have a college degree. Additionally, the employer imposes few hiring prerequisites.

We control for a rich set of store characteristics when we analyze between-store variation; controls include the logarithm of employment, store age and its square, time since the last store remodel and its square, store size (measured in square feet) and its square, and indicator variables for if the store is on the street, a commercial strip, or in a mall.

Sales per store will also depend on the number of nearby competitors. We control for the number of establishments that are in the same county in the same four-digit industry as reported in the 1998 County Business Patterns. To control for other local factors, some estimates include an extensive set of dummy variables, one for each ZIP code with more than one store.

Community Variables

To construct community demographics, we use each store's ZIP code to identify a zone of "nearby" Census tracts, defined as those in its ZIP code or within two miles of the centroid of its ZIP code. We then merge 1990 Census data for this zone to each store.

We construct the proportion black, Hispanic, Asian, and female surrounding each store, as well as the age distribution in the surrounding community using the following data. The 1990 Census asks questions on race (black vs. white, etc.) separately from ethnicity (Hispanic vs. non-Hispanic). Thus, on the Census, respondents can categorize themselves as both black and

Hispanic or as both white and Hispanic. In contrast, the employer has mutually exclusive codes of white, black, and Hispanic (as well as Asian). We allow both the Census categories of population and the employer's categories of employment to enter unrestricted in our equations.

We control for several other community characteristics likely to affect product demand. As control variables, we use Census data on the household income distribution (percentages of households in each of ten detailed income categories), the age distribution (percentages of individuals in each of six age categories), total population within two miles, population within two miles categorized into six size groups, and the unemployment rate. Because population is measured within a fixed two-mile radius, it can be thought of as a population-density measure. The income figures are only available for the store's ZIP code, without the two-mile radius of surrounding tracts.

Store-Community Interactions

For matching theories, the variables of interest are the interaction between store and community demographics. Such interactions allow us to test, for example, for the effect of having a highly Hispanic workforce near a Hispanic population center. The racial composition of the stores are highly correlated with the composition of the community (for example, the white shares are correlated at 0.70); nevertheless, substantial variation remains across stores. In addition, the racial shares vary substantially over time as well.

We also measure the interaction between the proportion female at the store and in the community. Aside from some areas containing military bases, single-sex colleges, and mining operations, there is much less variation in gender shares than in race or ethnicity across locations. Thus, we have little testable variation in the proportion of females across communities.

Diversity Within the Store

We calculate age, gender, and racial diversity within the store as well as the surrounding community. For race and gender, we use a diversity index equal to the odds that two people selected at random from a workplace differ on race or gender. The formula is that the diversity index is one minus the sum of the demographic shares squared:

Diversity index on race or gender = $1 - \Sigma_i S_i^2$, where S_i is the share of each gender or racial group *i*. This diversity index is zero with complete homogeneity and is maximized when each group has an equal share of employment. Economists might naturally think of it as one minus the Herfindahl Index.

Most past researchers have used the coefficient of variation on age or the standard deviation of age to measure age diversity. We prefer to use the standard deviation within the workgroup of the natural logarithm of age. The standard deviation of log(age) implies that proportional gaps in age are what lead to social distance; for example, the age gap between 18 and 22 usually leads to more social difference than does the age gap of 40 to 44, although the two gaps are the same in absolute years. As with the race and gender diversity indices, the standard deviation of log(age) has a simple interpretation: It is approximately the expected percentage gap in the age of two people chosen at random. This relation holds exactly for normally distributed variables.

Results

Summary Statistics

Summary statistics are listed in Table 1. The mean age of employees in our data is only 24 years. As this is not a sector or a firm in which most employees stay to build a career, most employees fall within a fairly narrow range of ages. The mean of the within-store standard deviation of the logarithm of ages is only 27 percent.

We observe values of the gender diversity index in our sample covering the full possible range from zero (all female) to one-half (an even mix of men and women), with a mean of .34. An increase in gender diversity is not the same as an increasing proportion of women. The proportion of women in the stores ranges from 6 percent to 100 percent with a mean of 75 percent. The racial diversity index ranges from zero to .79, with a mean of .39. These are entry-level jobs; thus, the stores are more black, more Hispanic, more Asian, more female, and younger than their communities.

Pooled Time-Series Cross-Section

Sales depend on the community's racial and gender composition, even after controlling for the community's income, unemployment, and population density (Table 2 column 1). Sales are significantly higher in communities with a greater female population share and a lower black population share. Recall that female share varies very little; it is unclear whether this coefficient has any economic significance. It is important to remember that these results condition on the firm's decision of how to market and where to open stores. (Few stores close in our sample period.) Either the company has not completely succeeded in marketing to a diverse customer base, or its choice of locations has not equalized sales on the margin across stores. The impact on profits depends on the extent to which these sales differences are offset by store rents.

A store’s age and race distributions also help predict sales. Sales are significantly lower in stores with greater proportions of black employees. Under depressed economic conditions, white men tend to bump down into this sector, which works against finding negative effects for both female and minority employees. The black result is consistent with customer discrimination. A 10 percentage point increase in black employment share (at the expense of the baseline group of whites) is associated with .8 percent lower sales. The same increase in Asian employment share is associated with .6 percent greater sales. The Hispanic employment share does not significantly predict which stores have high sales. The workforce’s average age predicts slightly higher sales, a result consistent with the theory of general human capital. Many of these results are sensitive to the alternative specifications discussed below.

Store-Community Interactions

The store-commmity interactions are presented in Table 2, column 2; this specification corresponds to equation (1). In results not shown, we find (as expected) that store racial composition largely reflects the demographics of the community. Nevertheless, stores do not simply match their communities and there remains testable variation in store demographics beyond community demographics.

This column presents the first main result of the paper: Does matching a community’s race increase sales? The coefficients on the interaction of store and community race are mixed, providing no consistent support for theories of customer preference. Specifically, the coefficient on (Store %Asian)*(Community %Asian) is a small negative number (contrary to theory), while the interactions on black and Hispanic are small and positive; none are statistically significant. As noted below, the signs of these interactions are not stable across specifications.

Unlike race, the proportion female is similar in almost every community in the United States. To avoid extreme multicollinearity, we use the gap between store and community percent female (instead of their interaction) and contrast stores in the top and bottom quartile of this distribution with those in the middle. Stores in the bottom quartile of store percent female minus community percent female have 1.2 percent higher sales than stores in the middle two quartiles. Working against the importance of this result is that stores with the top quartile of store percent female minus community percent female) have 0.3 percent higher sales than stores in the middle two quartiles.

The important finding here is that we see neither significant nor substantial evidence that matching employment shares to population shares in the surrounding community matters for sales.

Positive and Negative Customer Discrimination

The results in column 1 of Table 2 included only a main effect on the share of each racial group in the store. In column 2 we add in quadratic terms, which permit tests of positive versus negative customer discrimination. Results differ across the racial groups.

When we look at the squared terms on the main effects of race, employing Hispanics is useful in the relevant range but at a declining rate. In other words, a store's sales are higher if it employs at least a few Hispanics, as predicted by our theory of positive discrimination.

The reception of blacks differs. In column 2 of Table 2, the first-order term on the proportion black in the store is insignificantly negative while the squared term is significantly negative. The combination of these results suggests that the first few blacks in a store have little effect on sales, but that beyond that low threshold, sales decline with the proportion black. Omitted productivity characteristics (for example, that blacks attend worse schools), could account for a linear effect. But the accelerating decline in sales as black employment share increases suggests negative customer discrimination – many customers avoid stores with blacks.

Diversity Within the Store

Employee diversity often matters, but in ways that are complex. Even where the effect of diversity on sales is statistically significant, it is often modest in magnitude. The small magnitude of most of these effects is our second major result.

Diversity is identified as a non-linear effect of changing demographic employment shares. When we add the store diversity measures (Table 2, column 3), age diversity is bad for sales, gender diversity is insignificant, and racial diversity is weakly positive. Given that most stores have a white majority, increasing racial diversity implies increasing the share of Asians, blacks, and Hispanics. When we include the negative main effects of each nonwhite race on sales to calculate the total derivative, we find that over most of the relevant range, the total effect of increasing diversity is small, negative, and not statistically significantly. In contrast, the estimated effect of age diversity is important; increasing our measure of age diversity by a standard deviation (that is, moving from a standard deviation of log(age) of .27 to .33) lowers sales by 15 percent.

When we combine the store-community interactions with the within-store diversity measures, results remain similar (results available on request).

Between-Store Results

Most of the main results from the pooled analyses reappear when we ignore time-series variation (the focus of the next section) and look solely at between-store averages. The results in Table 2, column 4, correspond with equation (4). Column 5 shows results with diversity indices. Gender diversity and matching a community's race or gender composition have no statistically significant effect on sales. As in the pooled specification, age diversity again predicts lower sales; the effect is even larger in the cross-section. Racial diversity helps sales, an effect that is both stronger and more significant in the cross-section than in the pooled specification. These results control for differences across community in income, unemployment, population density, and retail store density.

The positive coefficient on racial diversity implies that diversity predicts higher sales, holding all else constant. While we can statistically identify diversity as a nonlinear effect distinct from the main effects, at least two of the racial shares must change to change racial diversity. Thus, the total effect of changing the racial composition of a store to move from an all white store to one with a mixture close to the national average (70 percent white, 10 percent each of black, Hispanic, and Asian) would raise predicted sales by 4.2 percent. (This is statistically insignificant even at the 10 percent level.) Moving from that medium level to a highly diverse store (40 percent white, 10 percent each of black, Hispanic, and Asian) would lower predicted sales by 2.5%; again, the predicted change is not significant.

Within-Store Year-on-Year Changes

The pooled and between-store regressions are both subject to omitted variable bias due to unmeasured factors in a location that affect both sales and demographics. Although we control for income, unemployment, population density, retail density, and other community factors, a Hausman test strongly supports the importance of store fixed effects. The Hausman test examines if the coefficients on store characteristics are stable when we shift from random to fixed effects; the coefficients differ significiantly, suggesting that fixed effects is more appropriate.

The results in Table 3 are based on a specification that differences out the remaining omitted unchanging factors, as in equation (3). We estimate the regressions using year-on-year changes in log sales in column 1. [4]

As noted above, even these specifications are subject to concerns about omitted local shocks that affect both sales and demographics. For example, consider two stores in the same neighborhood. Whatever omitted forces that affect product demand or demographic supply in one such store are likely to affect the other store as well. We isolate from these demand or supply shocks common to such "brother" stores.

In columns 3 and 4 we add controls for community fixed effects based on ZIP codes, as in equation (6). Thus, two levels of differencing are applied: differencing within stores across time and comparing across stores sharing a ZIP code. This specification answers the question of whether when one store in a community moves to better match the community demographics, does its sales increase relative to a nearby store that does not adjust its demographics. This is a desirable "brothers" specification that fully exploits the richness of the data. For example, the location fixed effects fully capture any regional change in community income, taste, or demographics.

[4] Similar results are found comparing months, quarters or years one year apart.

The cost of this more rigorous procedure is that it reduces the number of stores and ignores all variation in sales that is persistent across malls or communities. When we run the regression on the rate of change of sales, the Hausman test strongly supports the importance of the ZIP code fixed effects.

Store-Community Interaction

We first examine the effects of store-community interactions. In both specifications, as with the pooled results, perhaps the most interesting finding is how few of the coefficients are large or statistically significant. That several statistically significant results in the cross-section (Table 2, especially column 5) are not present in the time series follows from less testable variation in the time-series, but may also suggest omitted variable bias in the cross-section despite the community controls.

Increasing the %Asian has no effect on sales in most communities, but the effect is negative in highly Asian communities (col. 1). The reduction in benefit in highly Asian communities remains but loses statistical significance with ZIP code fixed effects (col. 2).

In contrast, raising a store’s %black reduces sales slightly in highly black communities, but only when controlling for the ZIP code fixed effects (col. 3). This result suggests that the patterns we observe are not simply due to potential white customers discriminating against blacks. In communities with few blacks, in contrast, increasing the store’s black share has a modest but insignificant positive effect on sales.

Diversity Within the Store

When we turn to the effects of diversity within a store, results are similar to the pooled estimates. Growing age diversity predicts lower sales growth. A one standard deviation in the dispersion of log age (almost 5 percent, so that two worker picked at random are about a year further apart in age) reduces sales growth by slightly less than .5 percent in col. 1, and slightly more than .5 percent in column 3 (with ZIP code fixed effects).

The effects of rising racial diversity are also statistically significant and negative; in contrast, racial diversity had a positive effect in the pooled and between-store regressions. As always, we must consider a move in racial diversity in terms of the underlying shifts in racial employment shares. For example, a move from an all-white store to roughly the retail chain average (70% white and 10% each other group) predicts 1.3 percent lower sales (change is now statistically significant at the 1 percent level). If we continue to increase diversity and examine the shift from a moderate to a highly diverse store (40% white, 20% each other group) sales remain unchanged (the point estimate is a tiny and not statistically significant -.3 percent). Because of the positive main effect on %Asian and the negative main effect on percent black, this result varies depending on the precise mix of workers that changes to create any given shift in overall diversity. As noted above, these main effects could be due to customer preferences for the race of their service people, or to differences in human capital, among other explanations. In contrast to race, changes in gender diversity do not predict changes in sales.

Immigrant Enclaves

Our analyses of the importance of hiring staff who are likely to speak the language of nearby non-English speakers are presented in Table 4. The order of the columns follows the order of the previous tables: random effects on all stores; between stores; within stores, and first differences of stores including ZIP code fixed effects. Our main test is to see if additional Hispanic or Asian employees are particularly valuable in communities with nearby enclaves of Hispanic or Asian immigrants who do not speak English.

Column 1 presents the pooled time series, cross-sectional results (with random effects for stores). Stores with more Asian employees have higher sales if the community has many Asian immigrants who do not speak English. Recall that many Asians in the United States speak only English, and those who speak an Asian language speak a variety of them. We cannot distinguish the language skills of employees. Because we then necessarily group together Asian employees of varying languages and fluency, the effect of hiring an employee who speaks the language of the enclave is presumably larger than the estimate reported here. (To the extent managers look for employees who speak the language of potential customers, Asian employees at a workplace near an immigrant enclave may be more likely to speak the relevant language.

To understand the magnitude of the coefficient of 7.1 on the interaction of the share of the store's percent Asian and the community's percent speaking an Asian-Pacific language but not English, consider two communities that differ by ten percentage points on the share of linguistically isolated Asians. This coefficient implies that a store with a 10 percent point greater Asian employee share has 7.1 percent higher sales in the community with more linguistically isolated Asians than in the community with fewer. This effect is both economically and statistically significant across specifications.

When we look between stores (column 2), the interaction for Asians rises in size.

Examining a complementary cut of the data, when we look within stores (column 3), the point estimates on having a rising proportion of the store's workforce who share the background of the linguistically isolated remain statistically significant.

Finally, we also run the within-store regression with ZIP code fixed effects. The coefficient on the interaction for Asians drops in size but remains statistically significant. The effect of Hispanics remains statistically insignificant, but the confidence interval includes the possibility of economically important benefits to hiring Hispanics in communities with many Hispanics who do not speak English.

Robustness Checks

We have run a large number of robustness checks. In all cases, results are consistent with the results presented above, with most store-community interactions small and insignificant other than results concerning linguistically isolated customers. We first discuss robustness checks for store-community interactions, then for employment diversity.

Store-Community Interactions

We test if within-store racial diversity is most useful in racially diverse communities. This interaction is neither large nor statistically significant.

Store reputation might lag changes in employment demographics. As a check, in the pooled and within-store regressions, we use store demographics that are lagged a month or that are the average of the last year. In case reputations take a long time to change, we look at two-year changes in sales as a function of two-year changes in store demographics and their interaction with community demographics. In case reputations are less important in stores with unstable demographics, we check if matching the community matters more in stores with stable demographics. The store-community interactions neither increase in size nor gain statistical significance.

Year-on-year changes in monthly store demographics may amplify the importance of transitory fluctuations in demographics. We average sales and demographics over 3-month periods and analyzed year-on-year changes in quarterly store demographics and sales. Results are similar to those reported in the text.

Some stores are in neighborhoods that attract many shoppers who are not from the community. We use several means to identify such stores and rerun the analyses dropping stores likely to serve a broader customer base. Results remain unchanged.

To test whether the functional forms chosen might be driving the results, we perform a simple nonparametric test, looking at how store sales grow when the proportion black at the store rises as a function of the proportion black in the community. The results show no interaction. We repeat this exercise for the other racial and ethnic groups with similar lack of results.

We also replace the interactions of store and community race shares with the absolute value of the gap in store and community demographics. Results remain similar. Because the stores are typically less white than their communities, and because the absolute value of the gap is more sensitive to mismeasurement of demographics, we stress the specifications with the store-community interactions.

We are also interested in whether some racial or ethnic groups avoid specific other groups; for example, if all nonblack groups avoid stores with blacks. This hypothesis is motivated by several observations; for example, Asians, Hispanics, and non-Hispanic whites intermarry among each other more often than any group does with non-Hispanic blacks. Turning to another sphere, Asians are more likely to live in racially integrated neighborhoods than are other groups. We replace the interaction of the percent black in the store times percent black in the community with the three interactions of percent black in the store with percent white, Asian, and Hispanic in the community. We perform similar substitutions for the other groups (percent Asian in the store times percent black in the community and so forth). Overall, results are rarely precisely estimated and show no strong patterns.

For the regressions analyzing linguistically isolated potential customers, we examine the effect of Asian and of Hispanic employees in communities with at least 1 percent and then again in communities with at least 5 percent linguistically isolated Asian or Spanish speakers. Results are consistent with the interactions presented in Table 4 in that minority employees are particularly useful in the communities where customers are most likely to need the employees' language skills.

We were interested in whether manager-community similarity increased sales. The hypotheses here are identical to those for worker-community similarity. The results were similarly unsupportive overall, with one exception. The single result supportive of manager-community similarity increasing sales is that when comparing across stores, stores with black managers had higher sales when in highly black communities than in other communities. At the same time, using the more convincing longitudinal variation, stores that gained a black manager had *slower* growth when the store was in a highly black community than in a less black community. Similarly, when controlling for ZIP code fixed effects, when a store switches to a Hispanic manager, sales decline in highly Hispanic communities. Other manager races interactions are negative but not significant.

In short, we tried a large number of variations and found no consistent evidence that having workers or managers who resembled their community affected sales.

Diversity Within the Store

Our most robust result concerning diversity within the store is the cost of age diversity. We replace mean age and the standard deviation of log age with the shares of employees who are teenagers, 20-22, 23-26, 27-33, and over 33. Compared to those 20-22, teens are less productive, while the older employees are slightly more productive, with the precise pattern depending on whether we use variation between stores or look at changes over time. However, when we control for both age diversity (the standard deviation of the log of age) and the proportion of the store under 20 or the proportion over 33, the age diversity measure remains strongly and statistically significantly negative, while the age shares are small and statistically insignificant. This result suggests that the negative effects of age diversity that we find results from something more than the lower productivity of teens or of employees who remain in this sector longer than most.

The Locus of Discrimination

Opinion surveys have for decades attempted to measure the extent and locus of discriminatory attitudes in the US. In recent decades, few will admit to holding such beliefs. While this is encouraging, one wonders whether the actions match the stated attitudes, or rather whether many have learned that it is no longer socially acceptable to state such beliefs. The stores we study are so pervasive and so uniform that we can use them as a probe of discrimination. Rather than ask about professed attitudes, we examine actions, using stores as a uniform test instrument. We ask whether sales in different situations are affected differently by employee demographics. We compare stores in communities with high and low black representation, and do the same for communities with high or low population shares of Asians, Hispanics, Females, and young. We also compare rich and poor communities classified by median household income, large and small cities classified by population density within 2 miles of each store, and large and small stores classified by square feet. In each case we compare the demographic effects on sales among stores in the first quartile of each distribution to the effects found among stores in the last quartile of each distribution. We also compare effects in the North to those in the Southern States. The results discussed in this section are based on cross-section specifications and are rarely significant in the time-series dimension.

The theories involved are of two sorts. Across communities of different demographics, the question is whether more heavily Asian, Black, Hispanic, or female communities show different patterns of discrimination in a nation more complex than the traditional black-white dichotomy. Comparing young to old communities captures both life-cycle and historical changes.

The comparisons across city size test a very different theory concerning search costs and the difference between thin and thick markets. Simply put, densely populated communities offer greater choice among retail establishments. Diversity across establishments - each one of which might be perfectly segregated- can substitute for diversity within establishment. At the other extreme, consider the general store in a small village: little choice of establishment, but broad scope within. We compare small and large communities to test whether diversity within a store is more important within smaller communities with less retail choice.

There is some evidence to suggest it is. To save space, we do not present tables. In cross-section estimates of our standard specifications, racial diversity has a significantly more positive effect on store sales in small than in large communities. The thicker markets in larger cities allow for more specialized stores, including those with more homogeneous staffs, to find sufficient customers. Customers with a preference for staff of a particular race can find them by searching across rather than within store. The implication of more racial segregation across stores in big cities than in small is, however, not strongly born out in the data. The test is not straightforward, since it depends on non-robust case-control methods that search for small cities with the population diversity found in big cities, and in big cities selects smaller stores that mirror store size in smaller cities. While the prediction of more segregation in bigger cities may seem a paradoxical result to those who think of bigger cities as more sophisticated, perhaps less discriminatory, and inherently more diverse, the result follows directly from classic economic models in which bigger markets allow greater specialization. Our result parallels a similar finding for radio stations (Waldfogel, 2001).

In bigger cities, black employees have a more adverse impact on sales, while Asians have a more positive impact. Similarly, racial diversity improves sales in small stores but not in big. Since in this company, big stores are found in big cities, this result may reflect the same model at work. A distinct theory for different effects between small and large stores is statistical. These workforces turn over 3 or 4 times a year. If customers are looking for demographic matches, past store demographics are a noisier measure of current demographics at small than at large stores because of the law of large numbers. Instead, we see that the negative effect of blacks on sales is greater at small stores.

A third stratification is between rich and poor communities. Because we measure both population and median incomes within two-mile circles, and because population density and incomes are positively correlated, this may again partially reflect city size effects. The adverse impacts of females and blacks on sales are significantly less in rich than in poor communities. Perhaps the rich are more tolerant concerning those who serve them.

The negative impact of blacks on sales is found in large cities, not in small, and the difference is significant. In addition to the theories examined above, this result is also consistent with suburban blacks differing from urban blacks in ways that whites are more comfortable with. While plausible, note that the adverse impact of females on sales is also worse in big cities suggesting other forces at work.

While racial discourse in the US is dominated by the categories of Black and white, the spectrum of race relations is more complex. We find that Hispanic employees have insignificant effects in both high and low Hispanic communities - without controlling for the potential barriers of language. Black employees have a better effect on sales in heavily Hispanic communities. But the reverse does not hold. Hispanic employees have a better effect on sales in non-Black communities. In other words, it appears that Hispanic customers tolerate Black salespeople more than Black customers tolerate Hispanic salespeople. Asian employees have a better impact on sales in heavily Asian communities, but have little significant effect elsewhere. Females have a positive impact on sales in communities with few teenagers. Perhaps the old are less bashful about who helps them. Older communities are also less sensitive to Black employees. Interpreted as a historical effect, this is not promising because it suggests more recent cohorts discriminate more. However, we cannot empirically distinguish this from the more optimistic interpretation that discrimination fades with age and experience. The negative effects of age diversity are also worse in younger communities.

Despite the perception left by the Civil War and Reconstruction, the South has had a longer experience of confronting racial division. We find that Blacks have a negative impact on sales only in Northern states. In the South, the effect is insignificant.

Discussion

Any study of how diversity affects workplace performance faces a number of challenges. First, because of potential legal challenges, it is rare that diversity and performance data at the company level see the light of day. Second, diversity exists as a concept along infinite dimensions. We focus here on the socially salient dimensions of race, ethnicity, gender, and age, although many other dimensions are expected to matter. Third, in practice diversity is often confused with the main effects of demographic differences. Finally, the effects of diversity are often confounded with other differences across jobs, employers, or communities.

Because women, blacks, and other minority groups typically work in different places and jobs than do white men, the challenge is to isolate the effects of diversity from the effects of both omitted location and occupation characteristics. We employ a study design that dramatically reduces this problem by using data from a single employer with more than 800 establishments. Just as a natural scientist would want to replicate conditions other than the experimental variable, the employer in this case promotes a consistent national brand and strives to hold fixed both human resource practices and the customer's experience across locations. This creates by design an unusual degree of homogeneity across locations.

Diversity studies can mistake not just employer differences but also community differences for diversity effects. In some specifications, we add extensive controls for community characteristics that might affect sales. In other specifications, we completely control for all unchanging store and community characteristics by examining changes in sales.

Finally, a community can experience an employment shock that might affect both the demographic mix of workers and demand for this company's products. In one set of specifications, we compare the effects of changing demographics on sales over time within store, holding constant regional shocks to sales or workforce demographics that might also affect a nearby store.

Summary

We study two distinct effects of employment diversity on sales, the first reflecting customer preferences, the second a direct output effect irrespective of customer demographic preferences. The results can be briefly summarized as follows:

- Evidence suggests that sales are higher if employees speak the language of customers who do not speak English.
- With that exception, our results do not support theories that employee-customer match increases sales. The effects of employee-community match are usually small and statistically insignificant.
- Previous theories suggest that diversity of gender or race might reduce sales due to worse communication and cooperation among workers, or raise sales due to pooling information, sparking creativity, and understanding diverse customers. Our results support neither set of hypotheses. Racial and gender diversity are generally not correlated with sales.
- Diversity of age consistently predicts lower sales. We must keep in mind how young and narrowly clustered this workforce is when determining the costs of age diversity in this sector: A 28 year old is an unusually old employee in this firm.

Limitations

Our results may be subject to several upward or downward biases. Moreover, even if they are accurate at this employer, they may not generalize to other sectors.

There are several sources of mismeasurement of the employee-customer match. For example, we are unable to measure how far customers travel to purchase goods and services from this workplace, and this distance varies by store. Moreover, in our interviews, several managers report that they often find employees by approaching customers and encouraging them to apply for a job. If this pattern is common, the actual match will usually be better than our measures indicate. Mismeasurement also arises because we merely tabulate the demographics of those living near a workplace; ideally we would weight each demographic group by its expenditures in this employer's sector. In addition, the relatively rapid turnover of employees implies that stores may not form strong reputations for their demographic mix. Moreover, the within-store estimates systematically remove the persistent portion of a store's demographic mix; thus, these estimates ignore effects that operate through the store's reputation for having a particular mix of employees. Each of these forms of mismeasurement is likely to bias down the coefficient on store-community match.

Offsetting these potential downward biases, it is likely that unmeasured neighborhood advantages are more common in stores with close customer-employee matches. Such advantages will bias the coefficients on customer-employee match upward, particularly in the pooled and between-store estimates. To see this effect, note that ethnic mismatch is typically smallest in communities with a very high proportion white. In the United States, the proportion white in a community is highly correlated with many other advantages such as high education and income (Currie and Duncan, 2000). Thus, unmeasured advantages may predict both low mismatch and high sales.

At the same time, this potential upward bias may be offset because the company knows something about the advantages and disadvantages of each community, and may avoid placing workplaces in disadvantaged communities. Low store density in disadvantaged communities implies relatively high sales per store.

Even if the estimates are unbiased at this employer, they may still not generalize to other employers or to other sectors of the economy. At the same time, the retail and restaurant sectors employ roughly one sixth of the U.S. workforce, so results that apply only in these sectors are still important.

On the one hand, diversity may matter less in this sector than elsewhere. These workplaces demand relatively little employee-customer interaction. Thus, there is below-average incentive for customers to seek a close match. The low status of these jobs implies that customers may care less about the race of those that serve them; for example, many customers may prefer an older white male to be their lawyer, but be happy to have a young Hispanic woman be their waitress or retail clerk.

This employer has a strong national brand. It is plausible that potential customers react more to the brand than to the demographics of current employees. Sales might be more responsive to employee-customer similarity at smaller employers without a strong brand image.

Diversity will also matter less in this sector than elsewhere because frontline workers have so little discretion. In workplaces with more decisionmaking power, diversity may be helpful in spurring creativity and costly in terms of raising communication costs. All of these forces are muted here.

Moreover, employees who work in demographically dissimilar communities may be more familiar with the local customers' group than the average person of their race. Whenever employee selection and self-selection occurs, we expect workplaces' customer-employee demographic match will matter less than if employers randomly assign employees.

On the other hand, the effects of diversity on sales may be greater in this sector than in others. It is easy for customers in malls and downtown shopping districts to look in the store window, see the demographic match, and choose a store based on similarities. In such a setting, customers may be particularly sensitive to demographic differences with potential salespeople.

In areas with high population density, this employer often has multiple workplaces in nearby shopping districts, and like many employers, may face incentives to segregate its workforce so that each workplace specializes in a single demographic group (Becker, 1957). In some cases, a chain of workplaces can maximize performance in diverse communities by operating multiple stores, each of which has a homogeneous workforce and appeals to a distinct segment of customers. For example, Garson (2002) describes several ethnically distinct shopping malls in the diverse city-state of Singapore. Each mall serves speakers of a specific language. The employer in this study, unlike most, can have several workplaces in a community, each of which has a distinct workforce and serves a distinct customer base. Our measure, by pooling the community, would erroneously report poor employee-customer match in all of the stores.

Some results with this dataset, unlike the test reported here, do support the importance of similarity attraction (Leonard and Levine 2002). For example, men, older workers, whites, and blacks (but not the other groups) have lower turnover when they work around many similar co-workers. Similarly, blacks and Asians (but not whites or Hispanics) turnover less when customers are more likely to share their race.

Conclusion

Asian immigrants who do not speak English apparently buy more from those of similar background. Beyond that result, we find no consistent evidence that most customers care whether the salespeople who serve them are of the same race or gender. Additionally, we have aimed to test whether employment diversity might still affect performance through a direct effect on teamwork among employees. We find no consistent evidence that the workgroup's performance depends on its racial or gender diversity, identified as a nonlinear effect. Age diversity, in contrast, does predict lower sales. While the effects of diversity vary, these results do not support the claim that employee diversity is important because customers desire to be served by those who physically resemble them (e.g., Cox, 1993; Jackson and Alvarez, 1992).

It is possible that customers discriminate in other sectors. Moreover, workgroup diversity’s effects for both good and ill are likely stronger in settings where employees have more discretion and autonomy, where workgroups are more stable, and where relations with customers are more complex.

To those concerned with the long and troubled history of discrimination, and with its continuing specter in this country, these results should be heartening. After all, one of the painful paradoxes of customer discrimination is that it could lead employers to discriminate in pursuit of greater profits even if they are indifferent to race and gender issues. The paradox is heightened by diversity proponents who argue that customers discriminate and should be pandered to. At least at this workplace, race and gender diversity do not appear costly. Moreover, managers in mostly white communities will not suffer lower sales if they hire black, Hispanic, or Asian employees. Neither the potential customers nor the employees' performance as measured by sales is much affected by the race or gender diversity of the workplace. This is good news.

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Table 1: Summary Statistics

Variable	Pooled data Mean	Pooled data Std. Dev.	One-year changes Mean	One-year changes Std. Dev.
log real sales	(omitted)	0.658	(omitted)	0.180
log employment (Average employment is about 30 frontline employees per store, mostly part-time)	(omitted)	0.505	0.127	0.237
Store Demographics				
Average age	24.3	2.28	-0.213	1.727
%Female	0.750	0.137	-0.004	0.089
%Black	0.119	0.134	0.013	0.071
% Hispanic	0.100	0.131	0.007	0.060
%Asian	0.070	0.089	0.006	0.054
Average age^2	595.1	115.4	-10.4	87.9
%Female^2	0.582	0.204	-0.006	0.129
%Black^2	0.032	0.070	0.006	0.038
% Hispanic^2	0.027	0.074	0.002	0.032
%Asian^2	0.013	0.037	0.002	0.021
S.D.(Log(age))	0.270	0.062	0.004	0.047
Gender Diversity= 1-[(%female)^2 + (%male)^2]	0.337	0.140	0.005	0.088
Racial Diversity = 1-[(%White)^2+(%Black)^2+(%Hispanic)^2+(%Asian)^2]	0.392	0.207	0.018	0.112
Community Demographics				
%Female	0.512	0.017		
%Black	0.075	0.094		
% Hispanic	0.051	0.069		
%Asian	0.051	0.078		
% Speak only Spanish	0.005	0.011		
% Speak only an Asian language	0.005	0.015		
%Female^2	0.262	0.017		
%Black^2	0.014	0.047		
% Hispanic^2	0.007	0.030		
%Asian^2	0.009	0.047		
Gender Diversity	0.499	0.002		
Racial Diversity	0.318	0.184		
Store-Community Interactions			Changes use levels of community variable and change in %of store	
Store %Female – Community %Female	0.238	0.138	-0.002	0.046
(Store %Black)*(Community %Black)	0.015	0.039	0.002	0.014
(Store % Hispan)*(Community % Hispan-all races)	0.019	0.061	0.001	0.009
(Store %Asian)*(Community %Asian)	0.008	0.036	0.001	0.013
(Store % Hispan)*(Community %speak only	0.001	0.006	0.0001	0.0013
(Store %Asian)*(Community %speak only Asian	0.001	0.004	0.0001	0.0014

The sample contains over 20,000 store-months at over 800 stores. Between-store summary statistics resemble pooled.

Table 2: Pooled Time Series Cross Section & Between Stores

	(1) Baseline Pooled	(2) Interactions Pooled	(3) Diversity Pooled	(4) Interactions Between	(5) Diversity Between
Dependent Variable	**Log Real Monthly Sales**	**Log Real Monthly Sales**	**Log Real Monthly Sales**	**Log (Average real sales)**	**Log (Average real sales)**
Store Employees Avg. Age	0.004**	0.023**	0.007**	0.020	0.020**
	(0.001)	(0.009)	(0.001)	(0.042)	(0.005)
Store %Female	-0.024	0.006	-0.002	-0.390**	-0.348*
	(0.016)	(0.023)	(0.033)	(0.141)	(0.156)
Store %Black	-0.078**	-0.003	-0.118**	-0.064	-0.408**
	(0.020)	(0.035)	(0.027)	(0.164)	(0.098)
Store % Hispanic	0.030	0.047	-0.011	0.661**	-0.050
	(0.024)	(0.038)	(0.030)	(0.194)	(0.120)
Store %Asian	0.058*	0.015	0.010	-0.132	-0.456**
	(0.026)	(0.041)	(0.035)	(0.247)	(0.160)
Community %Female	1.123**	1.138*	1.117*	-0.852	-0.798
	(0.434)	(0.449)	(0.457)	(0.552)	(0.547)
Community %Black	-0.455**	-0.526**	-0.475**	-0.329	0.063
	(0.076)	(0.144)	(0.116)	(0.192)	(0.154)
Community % white Hispanics	0.578**	0.756*	0.586**	0.001	0.448*
	(0.124)	(0.321)	(0.142)	(0.450)	(0.199)
Community %Asian	0.133	0.443*	0.121	0.061	0.421**
	(0.084)	(0.220)	(0.101)	(0.317)	(0.161)
$(\text{Store Avg. Age})^2$		-0.000*		-0.000	
		(0.000)		(0.001)	
$(\text{Store \%Black})^2$		-0.176*		-0.374	
		(0.069)		(0.332)	
$(\text{Store \%Hispanic})^2$		-0.141		-1.398**	
		(0.108)		(0.521)	
$(\text{Store \%Asian})^2$		0.133		-0.361	
		(0.146)		(0.905)	
$(\text{Community \%Black})^2$		0.178		0.233	
		(0.307)		(0.474)	
$(\text{Community \%Hispanic})^2$		-0.475		0.200	
		(0.639)		(1.044)	
$(\text{Community \%Asian})^2$		-0.503		0.054	
		(0.357)		(0.807)	
Top quartile (Store %Female – Community %Female)		0.003		0.009	
		(0.005)		(0.027)	
Bottom quartile (Store %Female – Comm. %Female)		0.012**		-0.028	
		(0.004)		(0.025)	
(Store %black)*(Community %black)		0.012		0.448	
		(0.156)		(0.551)	
(Store % Hispanic)* (Community % Hispanic)		0.230		0.881	
		(0.215)		(0.720)	
(Store %Asian)*(Community %Asian)		-0.038		0.617	
		(0.269)		(1.488)	
Store Age Diversity = S.D.(log(age))			-0.157**		-0.821**
			(0.039)		(0.195)
Store Gender Diversity $= 1-[(\%female)^2 + (\%male)^2]$			0.022		-0.110
			(0.034)		(0.163)
Store Racial Diversity $= 1-[(\%W)^2+(\%B)^2+(\%H)^2+(\%A)^2]$			0.046*		0.278**
			(0.022)		(0.094)
R^2	0.78	0.78	0.78	0.86	0.86

Notes: Standard errors in parentheses. * (**) significant at 5% (1%). Additional controls include store age, time since last remodel, store square feet, and their squares, log(employment), store division, store type (mall, street, etc.), store and community %Native Americans and their interaction (col. 3-5), store and community % other races, 9 community income shares (such as % of households with incomes $50-75,000 per year); %unemployed in community, 5 measures of community age shares (such as % ages 30-49), six measures of population density (such as between 80,000 and 320,000 live within 2 miles), the number of competing establishments in this 4-digit SIC in this county, and month dummies (Col. 1-3). Col. 3-5 include community racial diversity and gender diversity. Col. 5 includes each store's months in the sample and a count of the number of Decembers. Sample is over 800 stores and over 20,000 store-month observations (col. 1-3).

Table 3: Year-on-Year Changes

Dependent Variable = 1 year %change in sales

	Within-Store Estimates Entire sample		**Adding ZIP Code Fixed Effects** Sample contains stores that have at least two stores in the same ZIP code	
	(1) Interactions	**(2) Diversity**	**(3) Interactions**	**(4) Diversity**
Δ Avg. Age in the Store	0.006	0.004**	-0.004	0.005**
	(0.007)	(0.001)	(0.008)	(0.001)
Store Δ %Female	-0.394	-0.014	-0.169	-0.005
	(0.379)	(0.030)	(0.410)	(0.034)
Store Δ %Black	-0.078**	-0.044*	-0.037	-0.041
	(0.029)	(0.022)	(0.034)	(0.027)
Store Δ %Hispanic	-0.041	0.023	-0.047	0.022
	(0.032)	(0.025)	(0.035)	(0.028)
Store Δ %Asian	-0.010	0.084**	0.064	0.089**
	(0.032)	(0.027)	(0.036)	(0.031)
Δ (Avg. Age in the Store ^{2})	-0.000		0.000	
	(0.000)		(0.000)	
Store Δ (%Black ^{2})	-0.014		0.044	
	(0.061)		(0.073)	
Store Δ (%Hispanic^2)	0.143		0.134	
	(0.093)		(0.099)	
Store Δ (%Asian ^{2})	0.373**		0.095	
	(0.128)		(0.154)	
(Store Δ %Female)*(Community %Female)	0.804		0.344	
	(0.742)		(0.804)	
(Store Δ %Black)*(Community %Black)	0.072		-0.447*	
	(0.152)		(0.184)	
(Store Δ %Hispanic*(Comm. % Hispanic-all races)	-0.183		-0.129	
	(0.204)		(0.238)	
(Store Δ %Asian)*(Community %Asian)	-0.671**		-0.477	
	(0.225)		(0.269)	
Store Δ st.dev. ln(age)		-0.071*		-0.112**
		(0.031)		(0.034)
Store Δ Gender Diversity		-0.031		-0.012
= 1-[(%female)^2 + (%male)^2]		(0.029)		(0.032)
Store Δ Racial Diversity		-0.040*		-0.042*
= 1-[(%white)^2+(%black)^2+(%Hispanic)^2+(%Asian)^2]		(0.016)		(0.019)
Observations: stores	over 800	over 800	over 600	over 600
store-months	over 20,000	over 20,000	over 10,000	over 10,000
Number of 5-digit ZIP code dummies	0	0	over 300	over 300
R-squared	.239	.240	.338	.338

Notes: Standard errors in parentheses. * significant at 5%; ** significant at 1%. Additional controls included %change in employment, store age and its square, time since last remodel and its square, store size in square feet and its square, store division, store location type (mall, street, etc.; col. 1 only), Δ% Native Americans, Δ% other races, and month dummies. Standard errors are adjusted for first-order autocorrelation within stores and for heteroskedasticity across stores.

Table 4: Results Concerning the Linguistically Isolated

Specification	**Pooled Time Series Cross Section**	**Between stores**	**Year-on-Year Changes**	**Year-on-Year Changes with ZIP code fixed effects**
Dependent variable	Log Real Monthly Sales	Log (Average real sales)	One year %change in sales	One year %change in sales
Controls and sample as in:	Table 2, col. 2	Table 2, col. 4	Table 3, col. 1	Table 3, col. 3
(Store % Hispanic)* (Comm. % Hispanic-all races)	0.199 (0.265)	1.001 (0.789)	-0.112 (0.277)	-0.342 (0.326)
(Store %Asian)* (Community %Asian)	-0.574* (0.285)	-0.586 (1.517)	-1.238** (0.246)	-1.007** (0.313)
(Store %Hispanic)* (Community % speaking only Spanish)	0.898 (1.831)	-0.805 (4.157)	-0.955 (1.769)	2.335 (2.503)
(Store %Asian)* (Community % speaking only an Asian-Pacific language)	7.058** (1.264)	15.414** (5.155)	8.654** (1.701)	5.709** (1.885)

Notes: Each column represents a subset of the coefficients from a separate regression specification. Other controls include the percent speaking only an Asian-Pacific language, the percent speaking only Spanish, and the additional variables as indicated at the top of each column. The proportions speaking only Spanish or an Asian language measure people who do not speak English; they may speak other non-English languages. The first-differences specifications (col. 3 and 4) include first differences of store variables, but not community ones.

(11/18/02)