Racial Inequality and the Spatial Organization of Employers in the U.S.

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Abstract

The aim of this dissertation is to examine how urban spatial structure, including the patterning and location of a city's residents and its jobs, affects racial inequality in the labor market. One key mechanism through which this happens is spatial mismatch. Research on spatial mismatch has been both voluminous and inconclusive, with broadly mixed results across a wide range of particular contexts and research designs. One important reason for this is that the bulk of studies exploring spatial mismatch have been cross-sectional and limited to narrow geographic contexts (typically single-city studies). This broad variation in context and method makes it unsurprising that this body of research has not produced a consistent set of findings. In this dissertation, I use novel data on private employers that covers that full U.S. in the period 1971–2011 to explore the effects of spatial mismatch across a range of geographic contexts, over time, for different groups, and using different methods to build a comprehensive picture of how the spatial organization of labor markets interacts with racially patterned housing to produce inequality in the labor market and beyond.

Contents

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was felled by a freak health disaster. During this long and frustrating period, I probably heard from Frank more often than from my own mother! The kindness and support he showed was no surprise, because that's how he had always treated me, but that support was critical nonetheless. Without it I would not be a sociologist today.

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Introduction

This dissertation explores how the spatial distribution of jobs affects racial inequality in many different ways. I use novel data on the locations and workforce compositions of private employers in the United States since 1970 to explore how these two things—where jobs are located and who holds jobs in different places—contribute to racial inequality in the labor market and beyond.

In Chapter 1 of this dissertation, I study how organizations and cities mediate racial inequality in labor markets. In particular, I examine how the spatial organization of urban labor markets affects racial inequality. I show how the spatial structure of different cities—from where people live and where they work to how densely they live and how easily they move around—creates inequality in employment opportunities for their black and white residents, and I show how this has changed over time.

The literature on spatial mismatch argues that black residents' concentration in central cities limits their job opportunities, because they live far from the suburban areas where most jobs have moved in the era of deindustrialization: black residents are "mismatched" to where the jobs are. Research on spatial mismatch has been both voluminous and inconclusive, with broadly mixed results across a wide range of particular contexts and research designs. But the theory of spatial mismatch is still largely framed in terms of a northern, industrial, Chicago-like city with high segregation, a large and monocentric concentration of black residents, and a relatively dense suburban ring. What is missing is an effort to broaden and unify the concept of spatial mismatch across a variety of urban contexts (and morphologies). In this work, I explore heterogeneity across cities in the effects of spatial mismatch, and I suggest that this previously unexplored heterogeneity is an explanation for the mixed findings of past research. Moreover, variation in spatial mismatch is just one case of urban heterogeneity that illustrates how urban sociological theories can benefit from looking at a broad range of different kinds of cities.

I find that spatial mismatch exhibits substantially different effects in different cities—the effect is at least five times larger in the city where it matters most than where it matters least. Moreover, I find that the level of segregation in a city is a key factor moderating the effect of spatial mismatch. This moderating role of segregation has been underappreciated, since past studies have used segregation as part of an identification strategy and have thus conflated segregation with job access. Many other city-level factors suggested by earlier literature, such as density and transit access, fail to explain variation across cities in the magnitude of spatial mismatch. Together these findings suggest that efforts to decrease racial inequality in access to jobs must address segregation in the housing market; race-neutral efforts to improve mobility and to create residential and business density are not enough.

To reach these conclusions, I carry out an organization-level analysis that looks at where firms—and thus jobs—are located and how close they are to black residents. I geocoded over forty years of longitudinal data (1971–2012) from the Equal Employment Opportunity Commission (EEOC) that measure the racial and gender composition of jobs at every private workplace in the U.S. that employs at least a hundred workers (and government contractors with at least fifty workers). This novel dataset provides a comprehensive, longitudinal picture of the spatial distribution of jobs in the U.S., making it possible for the first time to examine variation across cities and over time, and I show that organizations mediate inequality differently depending on their urban environments.

In Chapter 2 of this dissertation, I shift directions and look at how organizations contribute to racial inequality in exposure to toxic pollutants. Space plays a key role in this process, too. Where Chapter 1 explores the problems that arise when firms are located too far away from black residents, Chapter 2 examines the opposite phenomenon: Organizations that release toxic chemicals are more likely to be sited near racial minorities, creating inequality in exposure that contributes to racial inequality in health. But while space plays a key role in mediating how organizations affect the health of residents through siting decisions, previous research has already explored this in great detail.

I look at a different dimension of space in shaping the behavior of organizations—the geographic communities in which organizations are embedded. Organizations exhibit great variation in the amounts and kinds of chemicals they release, and this variation is an important part of differential exposure of residents to pollution. I draw on recent theories of organizational practices that emphasize the importance of managers to help explain these differences.

I address two related questions: How does racial diversity in an organization's managers affect its polluting behavior? And does this behavior depend on the community in which the organization is embedded? To examine this, I use longitudinal data on firms' toxic chemical releases, linked to data on the racial composition of those firms' workers and the composition of the neighborhoods where the firms reside. I find that when organizations are located in predominantly African-American and Latino neighborhoods, adding more African-American and Latino managers makes those firms pollute less. These findings link managers' behavior inside organizations to important resources in the external communities surrounding those organizations.

Chapter 3 of this dissertation returns to the territory of Chapter 1 and examines one of the background assumptions of the spatial-mismatch hypothesis: Do firms act in a race-neutral way when they choose locations? Or do they exhibit preferences that lead them away from neighborhoods with large proportions of black residents?

I use longitudinal data on firms' locations to analyze the forces that lead firms to relocate along with the neighborhood features that attract and repel them when they move. I find that firms do take race into account in their location decisions. Firms in neighborhoods with a greater share of black residents are more likely to leave those neighborhoods, and they are more likely to move to neighborhoods with fewer black residents. But this behavior is not universal: Firms that employ large numbers of black workers have less negative preferences towards neighborhoods with many black residents. At the top end of the distribution, majority-black firms do not flee predominantly black neighborhoods. Given enough black workers, the number of black residents in a neighborhood *increases* the likelihood that a firm will stay in that neighborhood or choose it as a destination when it relocates.

Chapter 1

Spatial Mismatch across Place and Time

Abstract

The spatial mismatch hypothesis suggests that residential segregation coupled with employment deconcentration has left black residents far from areas of job growth and thus harmed their employment opportunities. Previous studies have been mixed but have lacked the data to explore spatial mismatch across a large variety of places. This chapter examines how spatial mismatch varies across different urban contexts and over time. It demonstrates substantial heterogeneity in the strength of spatial mismatch across cities, and it finds that differences in segregation explain a large amount of the variation in spatial mismatch across cities. While previous theories have suggested other metro-level factors like population density and transit access as important moderators of spatial mismatch, this chapter finds no evidence of their effects.

The spatial organization of labor markets plays an important role in determining who has access to jobs, especially in American cities characterized by high levels of racial residential segregation, though its precise impact is far from settled. The spatial mismatch hypothesis (Kain 1968) suggests that black workers lack equal access to job opportunities because jobs have shifted toward the suburbs while black residents remain concentrated in central cities. Kain argued that—even if there were no discrimination by employers—the spatial distribution of jobs could by itself account for unequal employment outcomes between blacks and whites. Wilson's (1987; 1996) work on urban poverty has reinvigorated the debate over spatial mismatch, since his argument about inner-city poverty turns on the joblessness caused by the movement of jobs out of cities and into areas that are inaccessible to poor blacks. Massey and Denton (1993) have further highlighted the importance of spatial mismatch by demonstrating the continuing prevalence of high levels of racial residential segregation in the US.[1] Spatial mismatch provides a key mechanism that links residential segregation to blacks' employment outcomes.

Research on spatial mismatch has been both voluminous and inconclusive, with broadly mixed results across a wide range of particular contexts and research designs. Like the intellectual history of urban sociology, much of spatial mismatch research is rooted in the experience of Chicago. Kain (1968) explored both Chicago and Detroit, and later studies have added a greater variety of cities. But the theory of spatial mismatch is still largely framed in terms of a northern, industrial, Chicago-like city with high segregation, a large and monocentric concentration of black residents, and a relatively dense suburban ring. The theory of spatial mismatch first developed against the backdrop of

[1] Segregation has slowly declined since 1970, but absolute levels remain high (Logan and Stults 2011).

growing suburbanization and employment deconcentration, but it has not kept pace with the tremendous diversity of city-development trajectories that have emerged as cities have "spread out." What is missing is an effort to broaden and unify the concept of spatial mismatch across a variety of urban contexts (and morphologies). In this paper, I explore heterogeneity across cities in the effects of spatial mismatch, and I suggest that this previously unexplored heterogeneity is a plausible explanation for the mixed findings of past research.

I use rich data that allow me to explore the employer side of spatial mismatch. The data come from employer reports to the Equal Employment Opportunity Commission (EEOC) that detail, at the establishment level,[2] the breakdown of workers by race, sex, and occupation category. I geocoded the data so the specific geographic location of each firm is known, and it can be linked to census and other data. I use data from the period 1972–2010, which provides both historical power and the potential to shed light on more contemporary issues.

I focus on the distance firms lie from areas of high black residential concentration, using that distance as a measure of job accessibility for potential black workers. I analyze the relationship between this distance and the level of black employment in each firm. I also examine how this relationship varies across cities and the extent to which metro-level factors like segregation explain heterogeneity across cities.

[2] Many firms have multiple locations, or *establishments*. These data contain separate observations for each one. Except when referring explicitly to multi-establishment firms, I will use the terms "establishment" and "firm" interchangeably.

First, I document an increase in the proportion of black workers who work near their homes, thanks to slow desegregation and the suburbanization of black residents that has partially offset the effects of employment deconcentration. Second, I find strong evidence that the importance of spatial mismatch varies substantially across cities—the effect is at least five times larger in the city where it matters most than where it matters the least. Third, segregation is a key factor in moderating the importance of spatial mismatch in different cities. The effect of a firm locating farther from black residential areas on that firm's level of black employment is worse in more highly segregated cities than in less segregated cities. This moderating role of segregation has been underappreciated, since past studies have used segregation as part of an identification strategy and have thus conflated segregation with job access. Fourth, contrary to many predictions advanced elsewhere in the literature, a number of seemingly important metro-level factors like density and transit access fail to explain metro-level variation in the magnitude of spatial mismatch. But segregation explains a substantial proportion of this variation.

Spatial Mismatch in Context

The spatial mismatch literature grows out of the work of Kain (1968). Kain started with the premise that discrimination exists in the housing market, which constrains where blacks can live and leads to residential segregation in urban areas. He sought to establish a link between residential segregation and black employment problems. Prior to his work, much research had explored employment discrimination as well as the possibility that employment problems resulted from group differences, such as lower educational attainment among blacks. But Kain was the first to posit that, even in the absence of discrimination by employers or group differences, residential segregation alone could cause substantial employment problems for blacks if jobs were located far from black residential areas. Kain's work was set against the backdrop of the early stages of the suburbanization of employment that began in earnest after World War II, and he saw the beginning of a potentially large and lasting problem.

Previous research has produced mixed evidence on spatial mismatch. For comprehensive reviews, see Jencks and Mayer (1990), Holzer (1991), Kain (1992), and Ihlanfeldt and Sjoquist (1998). I argue that a likely reason for the inconclusive evidence is that we would expect spatial mismatch to function differently in different contexts. However, data limitations have largely prevented past research from exploring the effect of spatial mismatch in different times and across different cities. Most studies have been limited to cross-sectional data on a couple cities. These studies haven't ignored context—the Rust Belt looms large, for example. Instead, they've focused too narrowly on single contexts, making it hard to develop a theory of how spatial mismatch varies across contexts. These data limitations have also prevented exploration of spatial mismatch at a national scale, leaving us with conflicting, piecemeal results from many different cities and time periods.

One exception is Boustan and Margo (2009), who provide the best available evidence on changes in spatial mismatch over time. They use a clever research design that takes advantage of the fact that the U.S. Postal Service has its own industry and occupation codes in the U.S. Census. Postal workers can thus be identified in Census public-use microdata. They argue that since most postal processing centers remained in central cities after other jobs left, and since black postal employment has historically been very high because of civil-service rules, black workers should substitute toward postal employment as other jobs leave central cities. They look at whether black workers in more segregated cities are more likely to work for the postal service than are white workers in those cities. Here, segregation is used as a proxy for black job access, since they do not have data on individual residence locations or individual job locations.

Boustan and Margo (2009) find no evidence of spatial mismatch in 1940 or 1950, which is what we would expect, because employment suburbanization had not yet begun in earnest. In 1960 and 1970, as suburban employment grew, they find large associations between metro-level segregation and the individual probability of black employment. This relationship declines after 1970 but remains significant through the end of their study period in 2000. But their data are limited to a narrow slice of public-sector employment (postal workers), and their identification strategy relies on inter-city variation, which prevents them from exploring how spatial mismatch itself varies across cities. Moreover, they conflate segregation with black residential centralization, which is less and less reasonable after 1970 when black residents began to migrate toward the suburbs. They measure segregation at the census-tract level, a unit smaller than typical commuting distances, which means the segregation measure is likely to be weakly correlated with actual job access (Houston 2005).

While Boustan and Margo (2009) study spatial mismatch over time, Leonard (1987) was the first to study empirically the idea that spatial mismatch will function differently in different kinds of cities. Leonard uses detailed employer-level data from a sample of firms in Chicago and Los Angeles in 1974 and 1980 to construct a measure of job access based on how far each firm is located from the central black residential cluster in each city. He finds that the distance a firm is located from this central cluster strongly predicts the level of black employment in the firm, and this effect is stronger in Chicago than in Los Angeles, despite Los Angeles's greater decentralization and lower access to public transit. Leonard uses a direct measure of job access—the distance a firm lies from black residential areas. This avoids the problem of conflating segregation with job access.

I follow the lead of Leonard (1987), who focused on the much-neglected employer side of spatial mismatch. Leonard, however, was limited in that he only had a subsample of firms in two cities over two years. Because he relied on the two time periods for identification, he was limited in his ability to examine change over time in the importance of distance between firms and black residents. But he was an early pioneer in showing the importance of looking across different kinds of cities to better understand how spatial mismatch operates. I use the same data source as Leonard but expand Leonard's analysis to cover a 40-year period and every metropolitan area in the U.S., using a polycentric measure of black residential location and more precise job-location data.

Distance is the basic mechanism that translates residential segregation into employment disadvantage. Commuting is costly, and so is searching for a job. First, if black areas are far from areas of high job growth, commuting costs alone might discourage blacks from taking low-wage jobs far from their residences. This is made worse by poor access to public transportation and low rates of car ownership among inner-city blacks. Second, even if blacks were willing and able to commute long distances, they might lack information about jobs far from their residences. This is especially likely since most information about job openings come from one's social network, and the networks of black, inner-city residents are likely to be similarly geographically constrained. Thus, search costs for distant jobs can be prohibitive.[3]

There is little reason to expect spatial mismatch to function in the same way in different cities, but data limitations have prevented past research from exploring this variation systematically. I examine how

[3]There is surprisingly little evidence about the geographic segregation of social networks. Much evidence demonstrates racial residential segregation and racial segregation in networks, but little research aims to connect the two. Wilson (1987) treats the network mechanism as fundamental to spatial mismatch; Mouw (2002) assumes networks are not associated with geography and treats it as a mechanism that competes with spatial mismatch.

the effect of distance from black residential areas on black employment levels differs across six different city characteristics. (Houston (2005) has a brief discussion of most of these factors, but the purpose of his discussion is to point out a lack of empirical evidence regarding their effects.)

Segregation is a fundamental theoretical component of spatial mismatch, but previous research has treated segregation itself as the predictor of interest rather than looking at how the level of segregation in a city moderates the importance of a firm's location (distance) on black employment. I expect that distance will be more important in more segregated cities. At low levels of segregation, black residents are dispersed widely enough that proximity to concentrated black residential areas should not be as important in providing a supply of potential black workers to any given firm. Since segregation has slowly declined in most cities over time (Logan and Stults 2011), this is also a mechanism through which I expect the importance of spatial mismatch to decline over time.

City size (*land area*) is another factor that I expect to change the impact of distance. Small metro areas should experience spatial mismatch to a lesser extent, because the maximum distance a firm can locate from the center is small and less likely to be a constraint that prevents commuting and information access from the city center (Houston 2005).

Greater *density* in a city should be associated with smaller effects of spatial mismatch. Greater density means that a firm located a given distance from a black residential area will be near a larger absolute number of black workers, since residential areas are more dense. In other words, the effect of distance should be diminished, since the number of nearby workers available at any particular distance will be greater. Most measures of urban "sprawl" are based primarily or in part on density (Laidley 2015), so this measure captures an important aspect of urban morphology.

Cities with greater *residential mobility* should experience a smaller degree of spatial mismatch. In such cities, it is easier for residents to move closer to areas with greater job density. This, of course, assumes in part that residential mobility is equally distributed across races, something we know is not the case in many cities (Pais, South, and Crowder 2012). Despite this, greater mobility should still translate into some gains in employment access for black residents.

Widespread *car ownership* should decrease the impact of distance by reducing the burden of commuting longer distances to work. Indeed, Fernandez (1994)'s case study of a single plant in Milwaukee finds limited evidence that car ownership among blacks reduces the impact of spatial mismatch.

Similarly, better access to *public transit* should reduce the impact of spatial mismatch by expanding the distance individual residents can easily travel to work. Given the racial composition of transit riders, better access might even disproportionately benefit black residents. Garrett and Taylor (1999, p. 12) show that the racial makeup of public transit riders has shifted dramatically over time. In 1977 nonwhite riders made up 20% of transit trips; by 1995 nonwhite riders represented 58% of rail ridership and 69% of bus ridership. Of course, if job growth lies primarily in the suburbs, and public transit primarily facilitates travel within central cities without linking the city to the suburbs, then transit access will have a smaller impact on job access (Ihlanfeldt and Sjoquist 1998). Glaeser, Kahn, and Rappaport (2008) show that poverty is more concentrated in cities than suburbs in urban areas where access to transit is greatest, so public transit is also an important determinant of the geographic distribution of poor (and black) residents.

Houston (2005) suggests one other metro-level factor, the extent of mixed land-use patterns—how much residential and commercial areas overlap—but there are no comprehensive national data available on this (though some coarse measures are possible starting in 1990).

Black Suburbanization and Job Deconcentration

Two large trends have dominated the spatial reorganization of cities since 1960. First, job growth in the suburbs has deconcentrated employment. Cities were once monocentric entities with jobs concentrated in central business districts, but beginning in the 1960s jobs began to follow residents out of central cities and into the suburbs.[4] The suburbanization of residences began earlier, and job growth followed

[4]Cities were never really monocentric, of course, but the stylized model of Burgess (1925) fit better before 1950 than it does after.

on a lag. Boustan (2010) provides the most persuasive causal evidence that white flight—white residents leaving the city for the suburbs in response to black in-migration—was a primary driver of residential suburbanization in the period 1940–1970. Improved infrastructure via highway construction also drove suburbanization (Baum-Snow 2007) over this period. Regardless of its causes, the effect of suburbanization by 1970 was the concentration of black residents in declining central cities and white residents in expanding suburbs.

This set the stage for the second and later trend: the suburbanization of black residents. Farley (1970) documents the early beginnings of black suburbanization in the 1960s, but it wasn't for a couple more decades until there were a substantial number of inner-ring suburbs that were primarily black suburbs and not just suburbs with small enclaves of black residents (Massey and Denton 1988; Hanlon 2008).

Classical spatial mismatch studies have focused heavily on job movement from cities to suburbs. This made sense in the 1970s, when blacks in particular were concentrated in central cities and only beginning to migrate to suburbs. In this period, firm relocation from city to suburbs represented a clear decrease in access for blacks. More recently, black inner-ring suburbs have grown yet often left blacks just as isolated, despite drawing them away from central cities. The city–suburb divide is no longer clearly along racial lines; it has grown more complicated than that. In 1970 and into the 1980s, the terms "central city" and "suburb" were consistent with our familiar concepts describing the spatial structure of cities. Now, however, the census no longer even defines central cities—they have been replaced by "principal cities" that include suburban employment centers (to better reflect the deconcentration of employment). "Central business districts" haven't been officially delineated as such since 1982 (Kneebone 2009), so these are not such a meaningful concept anymore, either.

In this paper I follow Leonard (1987) in defining "center" and "suburb" in relation to where the black residential population lives so that those terms reflect a consistent theoretical usage over time rather than a specific morphological component of a city. This also makes it easier to compare cities with different morphologies. In many cities, especially monocentric, northern, industrial cities like Chicago, the "center" still maps to our common idea of central cities—working-class black suburbs exist, but they are contiguous with the concentrated black residential areas on the south side of the city. In more sprawling southern cities like Atlanta, the "center" includes a much larger geographic area spanning nearly all the inner-ring suburbs, and the term "suburb" is limited to areas farther beyond the inner ring. Historical definitions of central cities no longer match the spatial structure of many contemporary cities, especially when it comes to thinking about job access for black workers in the context of employment deconcentration. To study spatial mismatch across cities, it is thus fruitful to abandon these historical distinctions and focus on spatial measures that map specifically to the problem of spatial mismatch.

If suburbanization of employment were the dominant trend in creating employment disadvantage for black workers, we would expect to see a decreasing share of black workers' jobs located near their residences. Instead, Table 1.1 shows that the relative proportion of black jobs located in the central cluster has increased over time (from 28% in 1972 to 38% in 2010), while the proportion of jobs slightly farther away (the border region) has shrunk. There has been an increase in the proportion of jobs located far from black residential clusters (the suburbs region), but the increase is small (20% in 1972 to 22% in 2010). As black residents have moved toward inner-ring suburbs, the size of the central cluster has grown in many cities.[5] Table 1.1 thus suggests that the overall trend of black residential deconcentration has offset the trend of employment deconcentration such that black workers are more likely to work close to black residential areas now than they were in 1972.[6]

The gains in the center are primarily a result of the growing size of black residential areas. Appendix A, Table A.1 shows the relative proportion of black workers located in each region of the city using constant-1972 boundaries for each region. Based on that, black jobs have left the city center and the surrounding areas and shifted toward the 1972 suburbs—20% of jobs were located in the suburbs in 1972, but by 2010 35% of black jobs lay in the same physical space. But using

[5]This is not true in all cities. In Los Angeles, for example, the relative black composition of the population has shrunk as the Latino population has exploded, and the size of the central black residential cluster in 2010 is smaller than it was in 1972.

[6]Trends for all workers are virtually identical to the trends for all blue-collar workers. Trends for all black workers are substantially the same as for blue-collar blacks but with slightly more suburbanization, with 2% fewer black workers in the center (36%) in 2010 and 2% more in the suburbs (24%) than in Table 1.1.

Table 1.1: *Relative proportions of black (left) and all (right) male, blue-collar workers located in the central black residential cluster, the border of the cluster (0–5 miles), and the suburbs (>5 miles). Each row sums to one. Source: Geocoded EEO-1 data.*

	Black blue-collar workers			All blue-collar workers		
	Center	Border	Suburb	Center	Border	Suburb
1972	0.28	0.52	0.20	0.17	0.53	0.30
1980	0.31	0.48	0.21	0.18	0.49	0.33
1990	0.33	0.45	0.22	0.20	0.45	0.36
2000	0.36	0.40	0.24	0.20	0.41	0.39
2010	0.38	0.40	0.22	0.21	0.41	0.38

time-constant boundaries neglects the changing spatial organization of housing and labor markets in cities over the past forty years. Taken together, in many cities, these findings reflect slow desegregation, and they also reflect the suburbanization of black residents: black workers work in different physical spaces in 2010 than they did in 1972, but the residential structure of the average city has more than just kept pace. A greater proportion of blue-collar black jobs are located close to black residential areas than at any time since 1970.

Of course, it could be that overall black employment has shrunk, and the relative gains in the center are the result of shrinking black employment away from the center. Table 1.2 suggests that this is not the case. It shows the proportion of blue-collar jobs in each region of the city held by black workers. Black workers increased their job share in every part of the city between 1972 and 2010, which reflects their increasing share of the overall population (from 11.1% in 1970 to 13.6% in 2010). Gains were greatest in the center—again, more black workers work close to home that at any time since 1972.

Table 1.2: *Left: Black share of male, blue-collar jobs in the central black residential cluster, the border of the cluster (0–5 miles), and the suburbs (>5 miles). Right: Black residential share in the central cluster. Source: Geocoded EEO-1 data; Neighborhood Change Database.*

	Black job share		
	Center	Border	Suburb
1972	0.27	0.16	0.10
1980	0.27	0.16	0.10
1990	0.33	0.18	0.11
2000	0.30	0.17	0.11
2010	0.33	0.18	0.11

	Black residential share
	Center
1970	0.50
1980	0.55
1990	0.55
2000	0.53
2010	0.51

These findings provide new insight into the way that changes in the spatial organization of cities since 1970 have affected black employment. However, this masks a great deal of heterogeneity across cities, and I now turn to an effort to explore and model this heterogeneity in greater detail.

Data and Methods

The primary unit of analysis used here is an individual firm location (establishment). I use EEO-1 data from the Equal Employment Opportunity Commission (EEOC). Every private U.S. establishment with at least 100 employees (or 50 employees and a government contract of at least $10,000) is required annually to file an EEO-1 report.[7] EEO-

[7]This excludes state and local government workers, schools, and colleges, which provide different reports. The size cutoff also means that many small businesses are

1 reports provide counts of workers in each establishment broken down by nine job categories, each of which is split by sex and by five race/ethnicity categories.[8] I focus on blue-collar workers, which includes four job categories in the EEO-1 data: craft workers, operatives, laborers, and service workers.

The key outcome variable is the log of the odds within each firm that a male, blue-collar worker is black. For over-time comparisons, limiting the focus to men avoids potential confounding by changing women's labor-force participation.[9] I focus on blue-collar workers to compress the income distribution across workers in the sample. High-paying white-collar jobs often overcome the disadvantage of distance by paying enough to compensate for higher commuting costs and by allowing workers to move closer to jobs. By narrowing the range of income and focusing on the lower end of the distribution, I can more closely approximate a sample of workers whose relative wages and

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excluded. In total, the EEO-1 data include about half of the total U.S. labor force (Robinson, Taylor, Tomaskovic-Devey, Zimmer, and Irvin Jr. 2005).

[8]Beginning in 2007, the EEO-1 reports include six racial categories, each split into Hispanic and non-Hispanic ethnicity to match the categorization used by the U.S. Census. For these years I drop workers in the "two or more races" category, which includes 0.46% of all workers. This is a not-insignificant number, but race-bridging studies done by the Census suggest that about two-thirds of these individuals would have been classified as white under the old set of races (Ingram, Parker, Schenker, Weed, Hamilton, Arias, and Madans 2003). Results are robust to the extreme alternatives of categorizing all these workers as white or all of them as black.

[9]Nearly all studies of spatial mismatch by race focus on men for this reason but also because black male unemployment is the focus of much of the recent motivating literature (e.g., Wilson 1987). See Fernandez and Su (2004) for a review of the largely separate literature on gender and the spatial organization of labor markets. Of course, focusing on black men brings its own complications, given the backdrop of mass incarceration that has made black men "invisible" in employment (Western and Beckett 1999) and other official statistics (Pettit 2012).

commuting costs match the classical economic model of commuting (Leonard 1987).

I calculate many key predictors at the city level. Beginning in 2003, the Census switched to using core-based statistical areas (CBSAs) for defining metro-area boundaries. CBSAs include an urban core with at least 10,000 residents and adjacent areas that are strongly linked by commuting flows. A subset of CBSAs are metropolitan statistical areas (MSAs), whose urban core contains at least 50,000 residents.[10] There were 381 MSAs defined after the 2010 Census, and I focus on a subset of those that satisfy minimum black residential population requirements that I describe below. MSAs are the areal units most comparable to previous research in sociology, much of which focuses on the older system based on primary metropolitan statistical areas (PMSAs). Current MSAs are comparable to old PMSAs.[11] MSAs are defined based on strength of commuting ties among a set of urban areas, and they function well as broad boundaries for metro-area labor markets. For all years of analysis, I use 2013 MSA boundaries (defined based on the 2010 Census). I keep the metro-area boundaries constant over time so the areas are comparable, though the outlying areas in each MSA were much less developed in 1970 than they are now in most areas. About 85% of firms in my sample are in MSAs, so they still

[10]CBSAs that are not MSAs are called micropolitan statistical areas, which can confusingly also be abbreviated as "MSAs." I use "MSAs" exclusively for metropolitan areas.

[11]Older delineations also included consolidated metropolitan statistical areas (CMSAs) that combine multiple regional PMSAs. The CBSA system now includes consolidated metropolitan areas (CSAs) that are comparable to CMSAs.

contain the bulk of the labor force.

One of the key measures used in this paper is an MSA's central black residential cluster. Leonard (1987) defines the central black residential cluster as the largest contiguous block of census tracts where each tract's composition is at least 20% black. Leonard's definition thus assumes a monocentric distribution of black residences. While this worked well for Los Angeles and Chicago in 1980, it does not translate to cities with a polycentric distribution of black residences (e.g., New York, which has separate clusters in Harlem, Brooklyn, and Queens). Moreover, Leonard's standard can be inconsistent within the same city over time: In Leonard's 1980 Chicago, for example, the west-side and south-side clusters of black residences are linked into a single cluster. But by 1990, the growth of Chinatown and the whitening of the South Loop had split the central cluster in two, and Leonard's measure would exclude the western cluster.

I expand Leonard's definition to support polycentric cities by using multiple clusters; a side effect of this is to make the clusters more robust over time within cities. I start with the same central, contiguous cluster as Leonard, but I also identify all other contiguous clusters of black residents, which could include even a single, isolated tract with greater than 20% black composition. Tract-level data on the racial composition of the population come from the Neighborhood Change Database (NCDB), which reports decennial Census data from 1970–2010 in constant 2010 tract boundaries. I compute the total black population in the central cluster, and I add any other cluster whose black population is at least 25% of the size of the largest cluster. This ensures that all large clusters in polycentric cities are included in the definition of the central black residential area. In nearly all MSAs, this set of clusters includes over 60% of the metro-area black population.[12]

Using the geocoded EEO-1 data, I calculate the distance between each firm and the border of the closest central black cluster.[13] This is the key predictor of interest: how far each firm is from a large number of potential black workers. I refer to this predictor simply as *distance*. Distance serves as a direct proxy for measuring spatial mismatch at the level of a single firm. The counterfactual implied here is what would happen to a firm's black employment if that firm were located closer to (or farther from) a large supply of potential black workers. This is different from the counterfactual of no residential segregation, where *all* firms would be located near equal proportions of dispersed black residents. That is also substantively interesting here, which is why I look below at how distance is moderated by segregation.[14]

I model the relationship between distance and black employment levels using a series of fixed- and random-effects models. Both plots

[12] I exclude cities that have no census tracts with a black residential composition above 20%. Of the top 50 MSAs by population, this excludes only San Jose, CA, which is 2.7% black, one of only two top-50 MSAs where the black share of the population is under 5% (Portland, OR is the other, which is still included). At #51, Salt Lake City, UT is even whiter (1.8% black) and is excluded. Ninety of the top 100 MSAs are included in my sample and 264 MSAs out of 381 overall—smaller MSAs are more likely to have almost no black residents.

[13] Firms located inside the central cluster have a distance of zero.

[14] Distance measures black workers' access to jobs at a particular firm. Segregation, on the other hand, measures the *distribution* of job access across all firms in a city.

and fitted models suggest that the main relationship needs at least a quadratic term to properly represent it, so I include both linear and quadratic terms for distance (see Appendix B, Figure B.1). In the main model, I fit random intercepts at the metro level to model heterogeneity across cities. I also fit random slopes to the distance predictor, allowing the strength of the relationship between distance and employment levels to vary across cities. Moreover, a second set of models interacts distance at the firm level with a number of metro-level predictors. For example, interacting distance with segregation allows segregation to predict the magnitude of the slope on distance across different cities to determine what portion of across-city variation in the distance coefficient can be explained by differences in segregation levels. By allowing the coefficient on distance to vary both across cities and in interaction with the key metro-level predictors in each city, I can model how spatial mismatch changes across metro contexts. This is one reason I use distance instead of a more direct measure like commuting time: how distance translates to commuting time (and job access) will vary across cities based on, among other things, the prevalence of car ownership and the coverage of public transit. The varying-slopes model for distance allows the model to determine what a mile of distance means in each city while making distances comparable across cities.

I use six metro-level predictors in interactions with distance. I measure segregation in each MSA by calculating the black/white dissimilarity index from tract-level NCDB data. Data on total land area (measured in square miles) for each MSA also comes from the NCDB and is measured using 2013 MSA boundaries. Population density at the metro level is a population-weighted density measure that expresses the average level of density experienced by a resident in an MSA. Because it is population weighted, the density measure is not mechanically related to the MSA's total land area. This also means that it works well across years: MSAs are defined in constant-2010 boundaries, and using a total density measure would understate the true density of cities in the earlier decades when much of the outlying area was unpopulated. Population-weighted density is calculated from density at the tract level: density = $\sum_i (p_i d_i) / \sum_i p_i$, where p_i is the population of tract i and d_i is the density of tract i. Unpopulated tracts thus have no influence on the metro-level density measure, which better reflects how density works across different city morphologies.

I measure residential mobility as the proportion of MSA residents who moved in the previous year, which is the only measure available in both the decennial Censuses and the American Community Survey (ACS). Data on MSA-level car ownership comes from raw decennial Census data obtained from the National Historical Geographical Information System (NHGIS; Minnesota Population Center 2011). These data were transformed into 2010 tract boundaries using the crosswalk from the Longitudinal Tract Database (LTDB; Logan, Xu, and Stults 2014) then aggregated to MSAs. Race-specific car-ownership data does exist for some years, but it is not available for the full sample period here, so I do not use it. I measure public-transit access using the proportion of workers who use public transit to commute to work (NCDB data). This is not ideal, since it is based only on workers, and spatial mismatch focuses on how lack of job access disadvantages non-workers in particular. But better transit-access data is only available for a small set of cities over a limited time period,[15] and commuter use of transit is likely to be highly correlated with any broader measure of transit access, so this measure still likely captures most of the meaningful variation across cities.

Although the EEO-1 data is annual, Census data is limited to decennial years (at least until 2010, when the first five-year estimates from the ACS became available). For this reason, I limit my analyses to the decennial Census years. The EEO-1 data did not begin until 1971,[16] and I use 1972 instead of 1971 EEO-1 data to match with the 1970 Census, because the coverage was much broader and more reliable in 1972 than in the earlier 1971 data.[17] For 2010 data, I use decennial Census data whenever possible, including for the key measurements of tract-level racial composition. Questions that used to be part of the long form of the Census are now only available through the ACS, and I use the five-year estimates for 2006–2010 in 2010. These ACS vari-

[15]See Baum-Snow and Kahn (2005) for the best data available, which covers sixteen U.S. cities.

[16]EEO-1 data does exist for the year 1966, but the program was in its infancy and the data are very incomplete.

[17]All of the outcomes are implicitly lagged by six months: the Census is collected around March and EEO-1 reports are filed around September in each year. Findings are robust to a one-year lag of the EEO-1 outcomes, which is what we would expect given the relatively slow change in all the key predictors over a ten-year period.

ables are car ownership, commuting by public transit, and residential mobility.

All models contain industry fixed effects,[18] controls at the establishment level for federal-contractor status, single-establishment status, the proportion of craft (skilled blue-collar) workers, and firm size,[19] and controls at the city level discussed above, plus metro-level proportion black and state-level GDP growth.[20]

Some Threats to Inference

It is possible that firms choose their locations based on the racial composition of the local labor market—discriminatory firms will move to the suburbs to avoid black workers, for example. If this is true, then the relationship between a firm's distance from black residential areas and its black employment levels will reflect both job access *and* employer discrimination. If one is purely interested in quantifying the level of spatial disadvantage in employment experienced by blacks because of residential segregation, then this is exactly the measure one would want. Without residential segregation, discriminatory firms could not

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[18] There are 91 unique two-digit industries in the data, and each one gets a fixed effect.

[19] These establishment-level variables are all calculated from the EEO-1 data.

[20] State GDP growth data comes from the Bureau of of Economic Analysis (BEA). Real GDP at the state level was not calculated by the BEA until 1987; they only produced a national GDP deflator before then. To get around this, I calculate nominal GDP growth from 1970–2010. By using within-state growth, much of the effect of inflation is subtracted out. My measure has a 0.92 correlation with post-1987 real GDP growth and, unlike the real GDP series, allows me to cover the full time period under study here.

move away to escape black workers, because all-white areas would not exist. But spatial mismatch, as framed by Kain (1968), assumes a world without employment discrimination in an effort to isolate the pure effect of residential job access on employment, independent of employer discrimination. This allows policymakers to evaluate the potential of different strategies for improving job access for black residents without confounding from employment discrimination. (Of course, policies that directly eliminated residential segregation would not be confounded with discriminatory employer location decisions.)

However, it is difficult to support the claim that employers in large numbers moved to the suburbs to avoid black workers.[21] As Leonard (1987) points out, neither the courts (in Title VII employment-discrimination claims) nor the Office of Federal Contract Compliance Programs (OFCCP; responsible for reviewing affirmative-action compliance among contractors) conceive of local labor markets at geographic scales smaller than an entire metro area. This means that moving to the outlying area of a metro area does not shield firms from Title VII or from OFCCP enforcement. So then one has to attribute a rather convoluted logic to the fleeing discriminatory firm: If hiring

[21] McDonough (2007) offers a rich portrait of employer location decisions that goes beyond a simplistic economic model. She finds, among other things, that the short-term costs of regulatory/zoning obstacles and the lack of available undeveloped land in central cities play a large role in many employers' choices to locate in the suburbs. Employers do care about local labor conditions, citing factors like the quality of local schools, and such factors are certainly correlated with race due to all the historical factors linking race to poverty in the U.S. None of this is evidence that discrimination plays no role in location decisions, but it provides many compelling reasons for the suburbanization of employment that are unrelated to race.

discrimination were an option, discriminatory firms would simply not hire black workers and avoid the cost of moving. Hiring discrimination must be difficult, either because of legal barriers or because firm owners/executives are unable to impart their discriminatory preferences on human-resources managers.[22] Thus, firm managers cannot discriminate at the point of hiring, so they have to put distance between the firm and a residential supply of black workers by moving. But if that's the mechanism for moving, then policies aimed at decreasing the effects of distance (e.g., decreasing segregation, increasing transit access) should be effective in bringing black workers back to such firms, since the only thing their move changes is the pipeline of workers. In that case—in a world where employers find hiring discrimination hard but can move to escape black workers—then all the same implications of Kain's spatial mismatch still hold.

All of this is to say that, while there is little evidence that employers move to the suburbs specifically to avoid black workers, this undoubtedly does happen with some frequency. If one's prior belief is that this frequency is very high, then the results of this paper should be interpreted as capturing a total effect of spatial disadvantage wrought by residential segregation on the employment prospects of black workers. Some of this includes the effect of employers who locate far from

[22] There is, of course, voluminous social-science evidence that hiring discrimination exists (see Pager, Western, and Bonikowski 2009 for an example in a low-wage labor market similar to those studied in this paper), and I am not even hinting that it does not. Instead, I'm arguing that, given that it does exist as a low-risk option for most firms, firms that move to escape black workers must not find hiring discrimination to be a big enough lever.

black workers, a choice enabled by high levels of segregation. If one believes instead that hiring discrimination is widely prevalent but that employer location decisions are based primarily on factors other than race, then the results of this paper can be interpreted as capturing the impact of spatial mismatch as Kain envisioned it.

I use firm fixed effects in some models, which will solve this problem in firms whose taste for discrimination is constant over time. It will also mitigate this problem in firms whose taste for discrimination is decreasing over time, something research suggests is true on average, though little evidence speaks to how discrimination changes over time within individual firms. The fixed-effects models do change the sample of firms in an important way: it limits the sample to firms whose data are available over at least two time periods, so firms whose existence doesn't span at least one ten-year period are excluded. This makes them useful as a robustness check but not suitable for examining metro-level variation because the samples in many smaller cities are much smaller. Some level of discrimination in firm-location decisions might still remain, however, and the caveats above apply.

Another threat to inference is selective migration of residents to the suburbs: First, there is the problem of individual self-selection: suburban employment levels might be better (regardless of race) solely because better workers move there relative to those who live in cities. Second, there is an endogeneity problem, in that "good" jobs allow people to move to better neighborhoods.

One way I mitigate this issue is by limiting the analysis to blue-collar workers. Their lower incomes make long commutes too costly and limit their residential mobility, decreasing their rates of intra-city migration (Houston 2005). Low-skilled workers are thus less subject to the problem of selective migration, and low-skilled workers are precisely the group motivating the interest of inequality researchers like Wilson (1987). Robustness checks using firm fixed effects also reduce the problem of selective migration. Such models implicitly predict employment *growth*, not its level. Since selective migration is primarily a long-run response to the movement of jobs, looking at the effect of distance on shorter-term employment growth will eliminate much of the confounding. But selective migration is a thorny problem. To the extent that it remains, the self-selection problem will lead to overestimates of the importance of spatial mismatch (since the effect mistakenly includes employment differences based on worker quality), while the endogeneity problem is likely to understate the effect of spatial mismatch (because distances from jobs will be highest for both high-income suburban residents and for low-income residents of central cities). Since these biases go in opposite directions, they can be expected to mitigate each other to some extent.

Finally, Hellerstein, Neumark, and McInerney (2008) distinguish between overall access to jobs and race-specific access to jobs, positing that if discrimination or networks prevent blacks from getting many jobs, then what matters is not the presence of jobs in general but rather the presence of jobs that are open to black workers. The problem is not just space; it's a combination of spatial mismatch and "racial mismatch." Because I focus on the firm side of job access rather than locations of individual residents, it does not make sense for me to use such race-specific access measures, but their paper provides important evidence that casts doubt on the relative importance of space compared to other race-specific factors that I do not measure here.

Results

Table 1.3 presents results from varying-intercepts, varying-slopes models of the relationship between distance from the central black residential cluster and black employment levels. Models are estimated separately for each year.[23] The main linear effect on distance is negative, as we expect: firms located farther from black residential areas have lower levels of black employment. In all years, the coefficient on distance^2 is positive—distance hurts black employment, but the effect gets relatively smaller at greater distances. The linear distance coefficient dominates the distance^2 coefficient over the range of typical within-metro distances, so the sign of the linear coefficient is what matters for the overall effect. The effect of distance is stable in the 1970s, then it increases in 1990 and 2000, then it begins to decline again after 2000.[24] This time trend aligns with many other broad trends in U.S. cities. For example, suburbanization of both residents and jobs

[23]In pooled models with year fixed effects and year interactions with all of the distance and metro-level covariates, the results were virtually numerically identical for this and for Table 1.4. I show separate models by year for ease of presentation.

[24]These distance effects are robust to using MSA–year fixed effects rather than all the metro-level predictors.

Table 1.3: *Estimates of the log of the odds that a male, blue-collar worker is black in each firm. Models include varying intercepts for metro areas, varying slopes by metro area for the distance coefficients, and controls for industry, MSA %black, federal-contractor status, log firm size, single-establishment status, proportion of craft workers in the firm, and state GDP growth. The distance^2 coefficients are multiplied by 100 to put them on scales comparable to other predictors.*

	1972	1980	1990	2000	2010
Distance from cluster	-0.076^{***}	-0.075^{***}	-0.089^{***}	-0.092^{***}	-0.082^{***}
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Distance^2	0.163^{***}	0.178^{***}	0.192^{***}	0.194^{***}	0.178^{***}
	(0.014)	(0.014)	(0.014)	(0.014)	(0.013)
Segregation index	0.041	0.447^{**}	0.363	0.718^{**}	0.239
	(0.240)	(0.173)	(0.219)	(0.218)	(0.206)
Log(MSA land area)	0.068^{*}	0.015	0.020	0.005	0.023
	(0.031)	(0.023)	(0.027)	(0.025)	(0.023)
Log(Weighted MSA density)	-0.262^{***}	-0.251^{***}	-0.208^{***}	-0.158^{***}	-0.147^{***}
	(0.057)	(0.046)	(0.045)	(0.042)	(0.038)
MSA residential mobility	1.552^{**}	0.421	0.295	0.771	0.342
	(0.502)	(0.451)	(0.620)	(0.642)	(0.553)
MSA car ownership	0.772	1.692	3.926^{***}	5.798^{***}	5.804^{***}
	(0.936)	(0.932)	(1.167)	(1.168)	(1.084)
MSA public transit	2.771^{**}	4.795^{***}	6.654^{***}	7.225^{***}	6.759^{***}
	(0.901)	(0.928)	(1.166)	(1.083)	(0.946)
Std Dev: MSA Random Intercept	0.304	0.231	0.303	0.295	0.262
Std Dev: Distance Random Slope	0.046	0.048	0.046	0.051	0.043
Std Dev: Distance^2 Random Slope	0.119	0.121	0.131	0.146	0.131
Std Dev: Residual	1.301	1.219	1.201	1.190	1.192
AIC	260564	306107	288431	372319	447941
BIC	261563	307024	289334	373227	448965
N (establishments)	77080	94249	89560	116371	139959
N (MSAs)	208	231	258	266	264

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

increased from the 1970s on, but starting around 2000, people began to return to cities. The coefficient on segregation is difficult to interpret here, since it is conditional on distance but forced to be constant across all distances. The interaction models below explore this in greater detail.

Firms in denser metro areas, conditional on all other predictors, have lower levels of black employment, though the difference gets smaller over time. Firms in cities with higher levels of car ownership show higher levels of black employment. This is what we would expect if car ownership were equally distributed across races and neighborhoods, because cars would make it easier for everyone to access jobs. This is not consistent with cars being primarily owned by suburban residents; in that case, we would expect firms located in cities with higher car ownership to have lower black employment, since cars would primarily increase job access among suburban residents. Firms in cities with higher levels of public transit access also have higher black employment, which is what we would expect given the evidence described above that black workers are more highly dependent on public transit than white workers.

The middle section of Table 1.3 shows estimates of the hyperparameters of the variance components: the average metro-level variation in the intercept ("MSA Random Intercept") and the average metro-level variation in the coefficients on distance and distance^2 ("Distance [Distance^2] Random Slope"). It also shows the residual variance at the firm level. All of these are expressed as standard deviations so their magnitudes are comparable to the slope on distance. The variation in the slope on distance across metro areas is large. In 1972, for example, the coefficient on distance is -0.076, and its standard deviation across cities is 0.046.[25] This means that some cities show a coefficient around

[25]The standard error of the coefficient is 0.005—the value in parentheses under the coefficient. The metro-level standard deviation is in the middle section of the table showing the variance components.

zero for distance, while other cities have coefficients on distance more than twice as large as the average.[26] The relative magnitudes are similar in the other years. Figure 1.1 shows the distance coefficients in 2010 for the 100 largest MSAs, along with 95% intervals around the point estimates of the coefficients. In most of the cities, the uncertainty intervals are quite small, and the differences in the distance coefficients across the larger cities are statistically significant. Together this represents the first clear evidence that the effect of spatial mismatch varies a great deal across different cities.

Next I examine how much of this across-city variation can be explained by the key theoretical factors described above. Table 1.4 presents the same basic model as before, but it adds interactions between distance and each of the six metro-level predictors. The distance predictors (distance and distance^2) are modeled with varying slopes at the MSA level, which allows the coefficients on distance to vary for each city in each year. Metro-level interactions allow the coefficients on distance to vary systematically across cities based on factors like differing levels of segregation. Distance itself is no longer a significant predictor in any year, suggesting that the relationship shown in the basic model above operates through interactions with the other

[26]To see this, we can look at the 95% interval showing the distribution of distance coefficients across cities, which here is $-0.076 \pm (1.96 \times 0.046)$, or $(-0.166, 0.014)$. There's a large negative correlation between the coefficient on distance and the coefficient on distance^2, but the linear distance term still dominates over typical within-metro distances, so the linear term alone accurately represents the relative strength of the distance effect in different cities. Below I explore the full quadratic function of distance in greater detail.

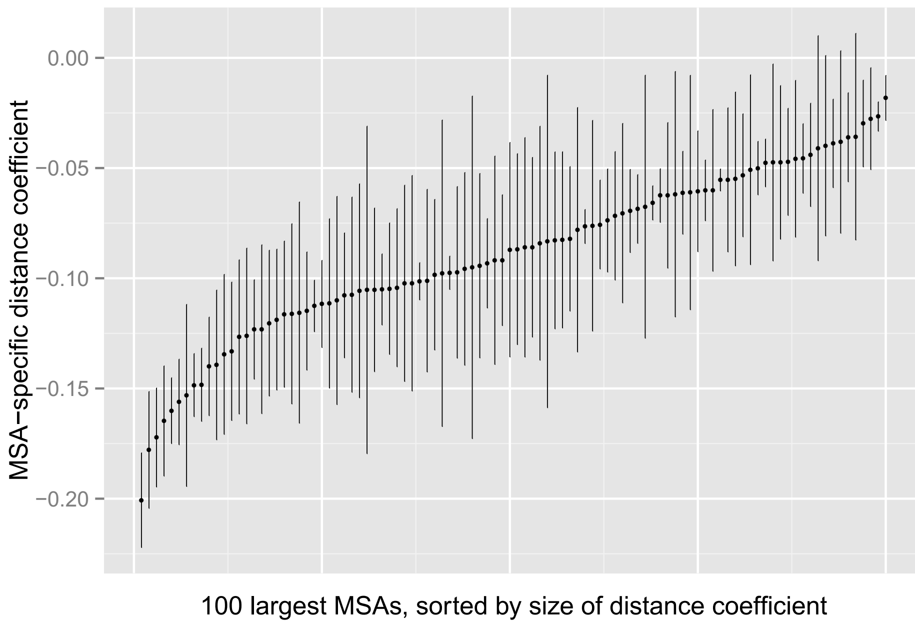

Figure 1.1: *Metro-varying slopes on distance with 95% intervals, 2010.*

metro-level factors.

Of the six metro-level predictors in interactions with distance, the largest and most consistent moderator of distance is segregation, and I return to it in greater detail below. Contrary to what we would expect from the theoretical literature on spatial mismatch, most of the other interactions have small effects, and those effects are not statistically significant. Car ownership, public transit access, and population density fail to show any measurable interaction with distance. Residential mobility has a significant negative interaction with distance in 2000 and 2010: as firms locate farther from black residential areas, they have even worse black employment levels in cities with greater residential mobility. This is the opposite of what we expected. Since we only see this in recent decades, a possible explanation is that mobility in these years reflects white suburban residents returning to cities, where they can more easily compete with black residents for central-city jobs. The effect is relatively small compared to that of segregation—the coefficients are similar in magnitude, but median values of residential turnover in those years are around 0.2, while median values of segregation (the dissimilarity index) are around 0.5, so the realized effect of residential turnover is only about 40% as large as the moderating effect of segregation.

Aside from segregation, land area is the only other interaction that is consistent and statistically significant over the full time period. All the interactions are positive and significant. Again, this is the opposite of what we expected based on the theoretical literature, which suggests that cities with greater land area should worsen the effect of spatial mismatch. Instead, land area dampens the effect. The simplest explanation for this is that the scale at which distance matters changes as cities grow larger in area. A mile of distance means less in a large city than in a small city. This makes sense if infrastructure (beyond what is measured by car ownership and public-transit access) in larger cities is better able to move people over large distances than infrastructure in smaller cities. Moreover, the average firm is likely to be located farther from the central black residential cluster in a larger city than in a smaller city (because all distances are larger in cities with larger land areas). If average distance grows as city size grows, then the realized impact of distance in larger cities will be comparable to the effect in smaller cities. Land area thus seems to act as a scaling factor for the effect of distance. To be clear, the effect of distance on black employment is still negative for all levels of land area, but the land-area interaction scales the effect to match the size of the city. Without this scaling, it would be harder to compare the magnitude of spatial mismatch across different cities.

Adding the six metro-level interactions reduces the variance of the random slopes for distance. In 1972, 1980, and 1990, adding the metro-level interactions reduces the remaining across-city variation in the slope on distance by about 30% in each year. In 2000 and 2010, it reduces the variation by 50%.[27] In other words, these six predictors go a long way in systematically explaining the variation in the effect of spatial mismatch across cities. Thus, not only does the model provide evidence on how much spatial mismatch varies across cities, it also illuminates the sources of that variation. Note that the individual-level residual standard deviations are identical between the interaction models and the previous models. The interaction models have only added predictors at the metro level, so the residual variance is unchanged.

The bulk of the variance reduction brought by the additional metro-

[27] To calculate this, take the variance component of the distance random slope in the interaction model and divide it by the variance of the distance random slope in the original model. In 2010, for example, this is $0.030^2 \div 0.043^2 = 0.49$, i.e., only 49% of the variation remains in the interaction model. Note that an exact calculation should also include the variance component for distance^2, but the distance^2 terms are multiplied by 100, and after converting to the original scale, the distance^2 random-slope variance is negligible.

Table 1.4: *Interaction models for each year of the relationship between distance to the central black residential area and black employment levels in each firm. All models include varying intercepts for metro areas and varying slopes by metro area for the distance coefficients, where the varying slopes are modeled with interactions between distance and a number of metro-level predictors. All models include industry fixed effects and controls for MSA %black, federal-contractor status, log firm size, single-establishment status, proportion of craft workers in the firm, and state GDP growth. The outcome variable is the log of the odds that a male, blue-collar worker is black. All distance² coefficients are multiplied by 100 to put them on scales comparable to other predictors.*

	1972	1980	1990	2000	2010
Distance from cluster	−0.123	0.195	−0.095	−0.111	0.122
	(0.162)	(0.192)	(0.200)	(0.190)	(0.177)
Distance^2	0.590	0.299	1.137	0.877	−0.562
	(0.614)	(0.666)	(0.743)	(0.681)	(0.686)
Segregation index	0.444	0.701***	0.832***	1.495***	0.920***
	(0.261)	(0.197)	(0.242)	(0.234)	(0.216)
Distance × segregation	−0.212***	−0.129**	−0.193***	−0.372***	−0.356***
	(0.050)	(0.046)	(0.046)	(0.040)	(0.036)
Distance^2 × segregation	0.455**	0.170	0.332	0.954***	0.978***
	(0.176)	(0.162)	(0.174)	(0.145)	(0.142)
Log(MSA land area)	0.023	−0.018	−0.021	−0.017	−0.005
	(0.033)	(0.025)	(0.030)	(0.027)	(0.024)
Distance × land area	0.022***	0.015**	0.017**	0.011*	0.014***
	(0.006)	(0.006)	(0.005)	(0.005)	(0.004)
Distance^2 × land area	−0.068**	−0.070***	−0.074***	−0.060**	−0.073***
	(0.022)	(0.020)	(0.021)	(0.019)	(0.017)
Log(Weighted MSA density)	−0.254***	−0.223***	−0.179***	−0.171***	−0.133***
	(0.064)	(0.052)	(0.049)	(0.045)	(0.040)
Distance × density	0.010	−0.006	−0.010	0.010	−0.003
	(0.012)	(0.011)	(0.009)	(0.007)	(0.006)
Distance^2 × density	−0.004	0.000	0.022	−0.050	0.008
	(0.043)	(0.037)	(0.033)	(0.026)	(0.025)
MSA residential mobility	1.619**	0.106	0.499	1.541*	1.073
	(0.533)	(0.496)	(0.681)	(0.688)	(0.589)
Distance × mobility	−0.020	0.160	−0.091	−0.387**	−0.380**
	(0.100)	(0.109)	(0.127)	(0.123)	(0.116)
Distance^2 × mobility	0.221	−0.022	0.431	1.336**	1.270*
	(0.356)	(0.389)	(0.466)	(0.452)	(0.500)
MSA car ownership	0.670	2.250*	3.776**	5.563***	5.888***
	(1.000)	(1.022)	(1.265)	(1.255)	(1.144)
Distance × car ownership	−0.045	−0.317	0.092	0.148	−0.048
	(0.189)	(0.208)	(0.214)	(0.199)	(0.189)
Distance^2 × car ownership	−0.266	0.357	−0.875	−0.672	0.584
	(0.716)	(0.712)	(0.780)	(0.706)	(0.732)
MSA public transit	2.727**	5.257***	6.178***	6.760***	6.289***
	(0.996)	(1.040)	(1.272)	(1.166)	(0.990)
Distance × transit	−0.260	−0.403	0.191	0.189	0.203
	(0.182)	(0.208)	(0.205)	(0.179)	(0.149)
Distance^2 × transit	0.042	0.695	−0.877	−0.423	−0.190
	(0.645)	(0.661)	(0.692)	(0.611)	(0.556)
Std Dev: MSA Random Intercept	0.298	0.227	0.292	0.283	0.250
Std Dev: Distance Random Slope	0.038	0.040	0.039	0.037	0.030
Std Dev: Distance^2 Random Slope	0.105	0.112	0.117	0.110	0.096
Std Dev: Residual	1.301	1.219	1.201	1.190	1.192
AIC	260588	306094	288441	372296	447913
BIC	261698	307124	289456	373320	449055
N (establishments)	77080	94249	89560	116371	139959
N (MSAs)	208	231	258	266	264

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

level interactions comes from one source—segregation. Including only the segregation × distance interaction reduces the distance random-slope variance by 25% in 1972, 1980, and 1990 and by 40% in 2000 and 2010.[28] The other metro-level interactions reduce this a bit more, but segregation is the key explanatory variable for metro-level differences in the effect of distance on black employment. Comparing the model fit between the basic model and the interaction model, the interaction model is a *worse* fit to the data. This is true based on both the Akaike information criterion (AIC) and the Bayesian information criterion (BIC). Both criteria compare the log-likelihood across models while adding a penalty for additional parameters (to avoid overfitting). So while it is true that adding the six metro-level interactions improved the log-likelihood (adding parameters can only improve it), the improvement was not enough to justify the additional parameters. I show the model with all six interactions because it is of theoretical interest—it includes the factors we expect to drive variation in spatial mismatch. But the model with only one interaction—segregation—fits better than both the basic model and the six-interaction model (based on AIC and BIC).[29]

The main effects of segregation are more easily interpretable now. Conditional on other predictors, the coefficient on segregation is sig-

[28]These numbers come from an additional model—not shown—that adds only the segregation interactions to the basic model.

[29]The segregation-only model even fits better than a model with just the segregation and land-area interactions. The land-area interaction appears to be precisely estimated but not very important in explaining metro-level variation in the slope on distance.

nificant and positive from 1980 on. This corresponds to the effect of segregation at a distance of zero—when a firm is located inside the central black residential cluster. More segregated cities have a higher concentration of black residents in their central clusters, so it makes sense that firms located there have higher levels of black employment. (All models control for the proportion black at the metro level, so higher values of segregation are not simply measuring cities with larger black populations.)

Figure 1.2 shows the interaction between distance and segregation graphically. Because distance is a quadratic function, it is difficult to see from Table 1.4 alone how different levels of distance behave across different levels of segregation, because there are three main effects and four interaction effects. Moreover, the coefficients in the table are coefficients on a log-odds outcome, which are hard to interpret in isolation, so it is important to look at the effects on the scale of the outcome. Figure 1.2 shows the three-dimensional gradient of the predicted outcome (black employment) across the important ranges of segregation and distance. Segregation (the black/white dissimilarity index) ranges from 0.35 to 1, matching the observed distribution across metro areas. Distance is shown between zero (i.e., firms are located within the central black residential cluster) and twenty-five miles, which is the important range for the vast majority of cities.

Figure 1.2 exhibits a number of general patterns. First, the highest levels of black employment are found inside the central black residential cluster (at the left of each plot where distance equals zero).

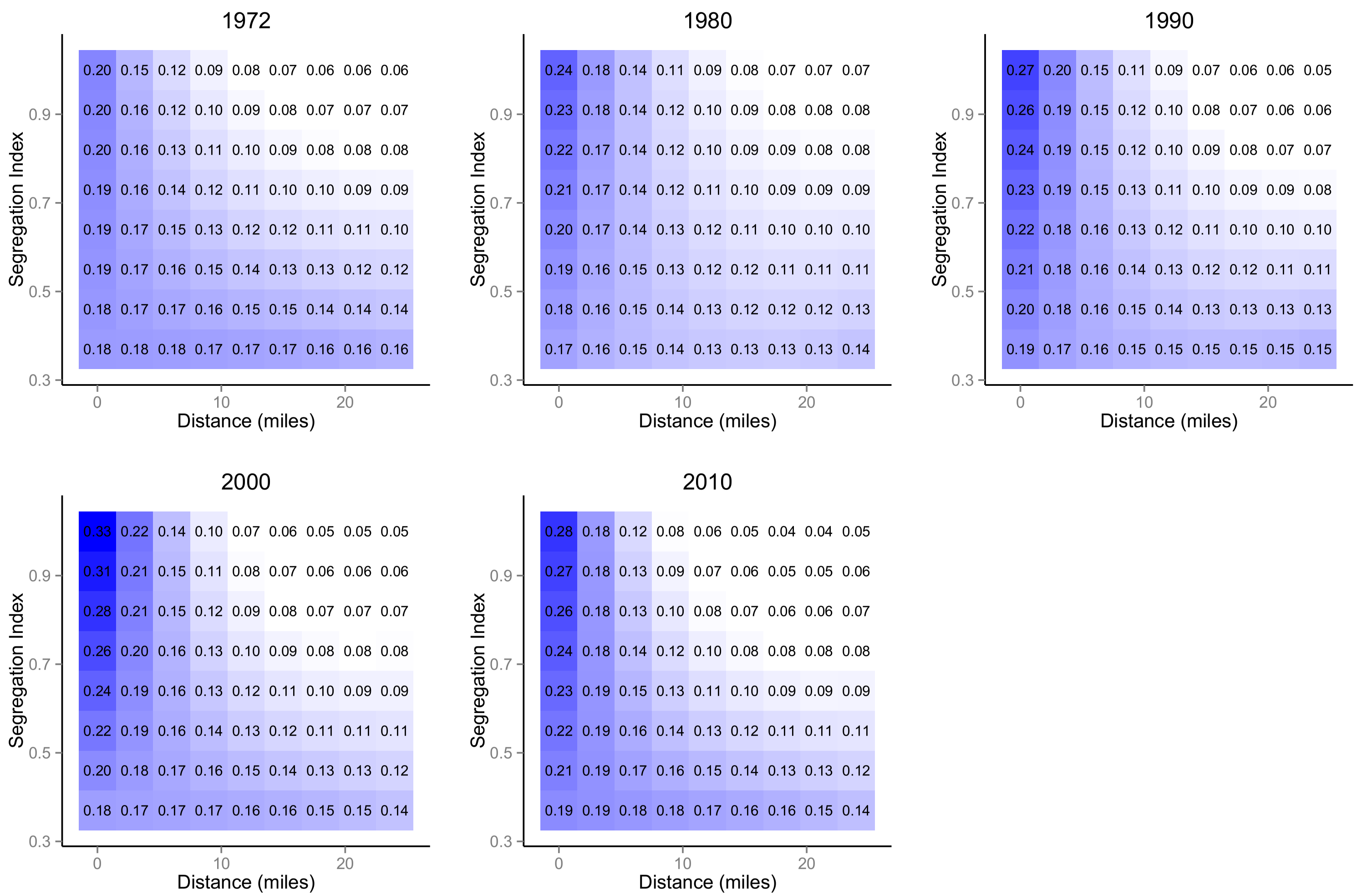

Figure 1.2: *Predicted outcomes for various values of distance and segregation. The gradient is darker for more favorable outcomes for black employment, showing how the effect of distance behaves in cities with different levels of segregation. Values show the predicted proportion of blue-collar workers who are black based on the model in Table 1.4. Predictions are averaged over all firms with all other covariates fixed at their empirical values.*

Second, black employment levels decrease at higher levels of distance (moving to the right in each plot) regardless of a city's level of segregation. Third, the gradient on segregation for small levels of distance is positive—more segregated cities have higher levels of black employment inside and just outside the central black cluster—but switches to negative at greater levels of distance.[30] Once a firm is located more than a couple miles outside the central black residential cluster, black employment levels decrease as segregation in a city increases. Fourth, the gradient on distance is less steep in less segregated cities. This matches my expectation about the importance of segregation: In cities with low segregation, black residents are dispersed widely enough that the distance a firm is located from the black residential center matters far less for employment. If we compare the top row of each figure to the bottom row—the highest level of segregation to the lowest level—we see that the gradient on distance is almost flat for cities with very low segregation.

Over time the gradient gets steeper in both directions from 1972 to 2000, then flattens a bit in 2010. Thus the effect of distance grows stronger over time, and the effect of segregation also grows stronger over time. Average segregation levels in U.S. cities have slowly declined over time, but the increasingly strong effects of segregation have potentially offset any gains that black workers have made with the aggregate

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[30]This pattern is not an artifact of the quadratic functional form. Using a generalized additive model with a tensor-product spline that models the full three-dimensional relationship between distance, segregation, and black employment as a smooth surface, we see this same twist in the gradient from low to high distances.

decline in segregation. Disentangling the relative importance of these two forces is an important question for future research.

All the results are robust to adding firm fixed effects, but because those models leverage limited changes over time, it is easier to interpret the random-effects models presented above. There is weak evidence of unobserved firm-level confounding, but it changes only the magnitudes and not the general substantive pattern of results. Firm fixed effects cannot be implemented in a straightforward way, since the key predictor—distance—changes little in firms that don't move.[31] Since firm fixed effects subtract out any time-constant characteristics, they rely on a relatively small amount of within-firm variation for estimates. To compensate for this, I use Allison's 2005 hybrid fixed-effects model, which decomposes the predictors into a between-firm estimate and a within-firm estimate, where the latter is equivalent to the classical fixed-effects estimate. If there is no unobserved firm-level confounding, the between- and within-firm estimates should be identical.[32] By decomposing the estimates into between- and within-firm components, we gain information about the size and direction of the bias caused by unobserved firm-level heterogeneity.

Here, I find that the coefficients on distance are slightly too large (~15–20% across different years) in absolute value in the models re-

[31] Changes in immobile firms come from changes in the size and location of the central black residential areas in a city; even though a firm may not move, the black residential population might move closer to or farther from the firm.

[32] This is equivalent to the Hausman test that is typically used to test whether fixed effects are necessary instead of random effects.

ported above that do not account for unobserved firm-level heterogeneity, but everything that is statistically significant remains significant in the fixed-effects models. The magnitudes of the segregation interactions change more in the fixed-effects estimates: in some years, the within estimates are 50% smaller than the model estimates reported above. In all cases, though, the fixed-effects estimates remain large, negative, and statistically significant. The best estimates of these effects probably lie somewhere between the models reported above and the fixed-effects estimates here. The pure within-firm estimates are free of time-constant unobserved firm-level heterogeneity, but because within-firm change is limited, these represent a conservative lower bound on the effect size. Estimates that include some portion of the between-firm variation represent a broader set of firms and contexts and are likely to be more robust predictors (in the same sense that random-effects models are more robust from a mean-squared-error prediction perspective than are fixed-effects models). Fortunately, even for someone who prefers to take a conservative approach to identification, all the results presented above are robust in sign and statistical significance to controls for unobserved, time-constant heterogeneity in firms. Moreover, the fixed-effects models implicitly model the *growth* in black employment rather than its level. As mentioned above, focusing on growth should eliminate much of the problem caused by selective residential migration, so these models add confidence that the results above are not driven by selective migration.

Systematic and Idiosyncratic Differences across Cities

The models presented above showed that there are substantial systematic differences in the effect of distance in different cities. The point was most clearly driven home in Figure 1.1, showing the large variation in the magnitude of the effect across different cities. There are clear aggregate relationships between distance, segregation, and black employment across cities, but this should not lull us into thinking that all cities are the same. Metro-level predictors differ across cities, and so does the distribution of firm characteristics (including typical distances). The models presented above allow us to examine not just the systematic differences across all cities, but we can also explore spatial mismatch in particular cities and over time, because the models estimate city-specific intercepts and city-specific slopes on distance for each year.

Figure 1.3 plots the relationship between distance and average black employment levels for firms in Atlanta, Chicago, Detroit, Los Angeles, and New York in 2010. These five cities are all among the largest in the U.S., but they exhibit different levels of black population share, segregation, black suburbanization, and employment deconcentration. The curve for each city is drawn using predicted values from observed firms in that city, fixing distance at many different values and holding all other covariates at their observed levels. This allows for a simple counterfactual exploration of how levels of black employment might

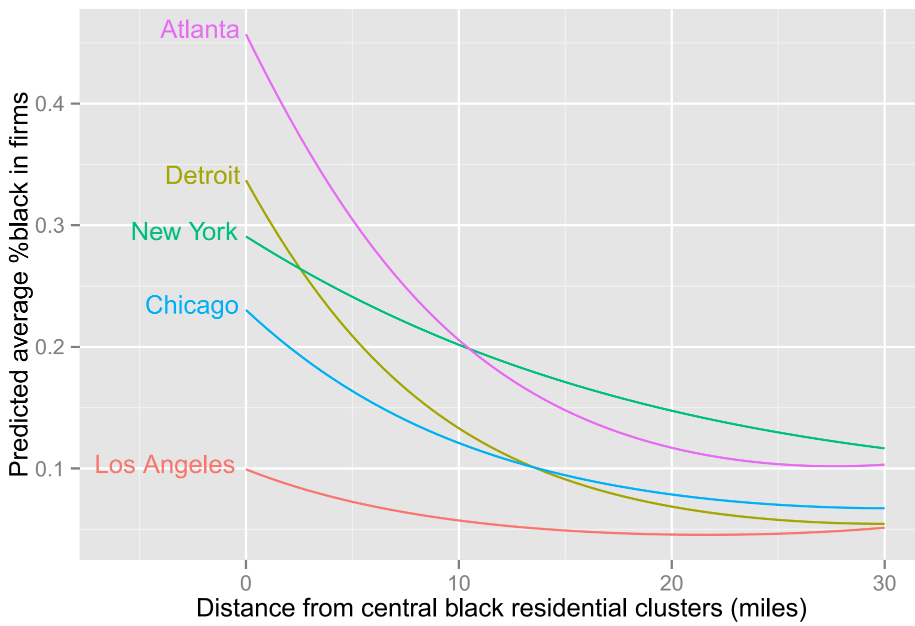

Figure 1.3: *2010 relationship between distance and average black employment levels within firms for five U.S. cities. Each curve uses the predictors from observed firms in each city while fixing distance at particular values to generate a curve of outcomes. Segregation in each city is fixed at the observed 2010 segregation level.*

change if firms moved closer to or farther from the central black residential clusters. I previously looked at the importance of segregation levels in moderating the effects of distance across cities. Here, I use the observed level of segregation in each city, so this illustrates how segregation and other city-level factors matter in practice to produce differences across cities.

Atlanta shows the largest effects of distance on black employment. In part this is because the black population in Atlanta is larger than in the other cities, so it starts from a higher baseline. Moreover, the central black residential cluster in 2010 Atlanta is extremely large, because

Atlanta is one of the cities where black suburbanization has advanced the furthest, so unlike other cities, the central black cluster in Atlanta includes the entire central city and most of the inner-ring suburbs. Atlanta still has a fair amount of segregation—it's in the middle of the range for cities with a large black population. So we still see a steep dropoff in black employment once we move outside the central cluster. Detroit remains the most segregated U.S. city in 2010 (Logan and Stults 2011). The steepness of the effect of distance there is almost as severe as in Atlanta. Despite a higher black residential share in the central city in Detroit than in Atlanta, firms located in the central black cluster in Detroit have much lower levels of black employment. This is what we would expect given the spatial structure of Detroit—there are very few black residents once you move beyond the inner-ring suburbs, so firms located that far from the center would require extremely long commutes for most potential black workers. Structurally, Chicago looks like a less extreme version of Detroit, with extreme segregation and relatively low levels of black suburbanization, particularly once you pass the inner ring. New York shows a relatively gentle decline in black employment as distance from the central cluster increases. This is not what we might expect from its segregation levels alone—New York was the third-most-segregated city in the U.S. in 2010. But New York is unique in the extreme polycentricity of its black residential population, with large but isolated clusters in Harlem, Brooklyn, and Queens. Combined with its extremely accessible public-transit system (with usage rates almost double that of the next-best cities), there are a huge number of jobs within easy reach of most black residents. Los Angeles has the smallest black population share of the cities shown, and that difference in scale masks the fact that the dropoff in black population with distance is just slightly less than that of Detroit and stronger than New York or Chicago. Los Angeles has mid-high levels of segregation, with a concentration of black residents in South LA and relatively little black suburbanization, so despite being geographically large, black employment drops to extremely low levels even ten miles outside the central black cluster.[33]

These figures make post-hoc storytelling too easy. The point here is not to explain the patterns we see in particular cities but rather to zoom in on how the model functions in specific cities, which helps us to better see its limitations and the areas where we still need more research to better understand how the mechanisms of spatial mismatch operate in different contexts.

Conclusion

This paper has presented new evidence on the way spatial mismatch varies across cities and over time in an effort to shed light on why previous research on spatial mismatch has been broadly inconclusive. I find substantively large differences across cities in the effect of spatial

[33]Note that we see a slight upward curve in black employment for Los Angeles after about twenty miles of distance. This is an artifact of the quadratic model combined with the extremely fast dropoff in black employment with distance. A quadratic curve cannot simply flatten out, and in order to capture the steepness of the dropoff at short distances, we get this artifact of an upward curve at longer distances.

mismatch. These effects are moderated by the level of segregation in a city, which explains a large proportion of the variation across cities. Other metro-level factors that have been put forward as theoretically important do not show evidence of moderating effects, though the land area of a city is important in scaling the effect of distance to different-size cities.

These results raise many questions. Are there other metro-level factors that are important sources of variation across cities? Results for specific cities suggest that our measure of segregation might not capture the morphological structure of black residential locations (and thus job access). New York has very high segregation but a city structure that keeps many black workers very close to most jobs, in contrast to Chicago, which has similar levels of segregation but whose spatial residential structure produces greater isolation from jobs for black residents. Measures that better capture not just segregation but the spatial distribution of black and white workers could better explain differences in job access across cities. Other measures of city morphology such as sprawl and land use also have potential to enrich our understanding of spatial mismatch across cities. Such measures would be a first step in creating updated conceptual measures that might take the place of the binary distinction between cities and suburbs.

The decline of the black population in Los Angeles and simultaneous growth of the Hispanic population raises the question of how spatial mismatch operates beyond black and white workers. Past research has focused primarily on spatial disadvantage as it affects black workers, but there is a large literature on segregation in other racial and ethnic groups (and limited research on spatial mismatch for Hispanic workers) that suggests it would be worth exploring spatial mismatch for other race and ethnic groups. As Alba and Logan (1991) point out, this is complicated for Asian and Hispanic groups, since those terms typically stand in for a large number of specific ethnic groups, often spatially concentrated immigrant enclaves from particular countries. But the residential patterns of these groups play an important role in the changing spatial structure of most large U.S. cities, and the growth of the Hispanic population in particular over the last few decades makes it important to understand spatial mismatch for all other groups if we are to understand the changing nature of its effects on black workers.

Future work should continue to iterate between explanations that build a systematic theory of spatial mismatch across cities and explanations that examine the theory in the context of particular cities. This work should better integrate the importance of economic organizations in the spatial development of cities. Such an approach offers an opportunity to continue to build links between urban sociology and organizations as mediators of inequality.

Chapter 2

Organizing Pollution: Organizational Demography, Neighborhoods, and Racial Inequality in Exposure to Toxic Chemicals, 1987–2012

Abstract

Many organizations release toxic chemicals that create serious health problems in the people who are exposed to them. This type of environmental pollution disproportionately affects residents of African-American and Latino communities. Previous research has looked primarily at where polluting firms choose to locate while paying scant attention to the large variation across organizations in the amounts and kinds of chemicals they release. To better understand the sources of racial environmental inequality, we must understand what drives differences across organizations in polluting behavior. I draw on recent theories of organizational practices that emphasize the importance of managers to help explain these differences. I address two related questions: How does racial diversity in an organization's managers affect its polluting behavior? And does this behavior depend on

the community in which the organization is embedded? To examine this, I use longitudinal data on firms' toxic chemical releases, linked to data on the racial composition of those firms' workers and the composition of the neighborhoods where the firms reside. I find that when organizations are located in predominantly African-American and Latino neighborhoods, adding more African-American and Latino managers makes those firms pollute less. These findings link managers' behavior inside organizations to important resources in the external communities surrounding those organizations.

Private organizations are the main source of toxic pollutants in the U.S. Toxic pollution has steadily declined over the past thirty years due to improvements in technology and more stringent environmental regulations. But toxic pollution still affects African Americans and Latinos far more than it does whites, and this creates high levels of environmental inequality (Brulle and Pellow 2006). The bulk of research on environmental inequality has focused on where hazardous facilities are located, demonstrating that they are disproportionately sited near African-American and Latino residents. Following these descriptive studies, researchers then set out to determine whether inequality exists because toxic facilities are built in neighborhoods where racial minorities live or because of demographic changes in neighborhoods after those facilities are built (for a recent review, see Mohai and Saha 2015).

These studies largely ignore the organizations that do the polluting, treating them as exogenous, disembodied sources of pollution. Sometimes their locations are used to ascribe motives of discrimination and racism to the organizations, but little effort has been made to take seriously the internal dynamics of organizations or even the variation across organizations in polluting behavior. Most studies simply look at the presence of a polluting facility in a neighborhood, not how much or how dangerous is the pollution it produces. But analyzing the mere presence of a polluting organization overlooks substantial variation in organizational polluting behavior. Even within the same narrow industries, organizations face a wide range of choices on what chemicals to use, how much they should use them, and how to dispose of them. The mere presence of a facility producing toxic pollutants provides little information about how dangerous that facility is to nearby residents. By focusing entirely on the initial siting decisions of firms—decisions that are often path-dependent results of historical choices made many decades ago (Elliott and Frickel 2013)—existing research has missed the opportunity to examine the main source of contemporary variation in pollution, which depends on different *polluting practices* across organizations. In one exception to this, Grant, Trautner, Downey, and Thiebaud (2010) urge sociologists to "bring the polluters back in" to research on environmental inequality, but that call has gone unanswered.

What determines organizational polluting behavior? Research on environmental inequality shows that the demography of the local community matters, but it is the leaders of organizations—managers—who make decisions about how (and how much) to pollute. Theorists of resource dependence have long argued that the demographic characteristics of a firm's workers shape its behavior (Pfeffer 1983), but empirical findings on the effects of organizational demography are mostly limited to workers' ages and tenure with a firm (cohort effects). Given the importance of the racial composition of communities in environmental inequality, I suggest that the racial composition of a firm's managers could also play an important role in determining its polluting behavior and thus its contribution to environmental inequality.

To examine this, I analyze longitudinal, organization-level data (1987–2012) on pollution emissions, worker demographics, and the demographic makeup of the communities where organizations reside. I show that changes in an organization's managerial demography—specifically, adding African Americans and Latinos to its management ranks—leads to changes in that organization's pollution emissions, but those changes depend on the organization's local community. When firms sited in predominantly African-American and Latino neighborhoods hire or promote more African-American and Latino managers, they produce less toxic pollution.

This finding has many implications for the study of organizations and of environmental inequality. First, it enriches our understanding of managerial demography, providing the first empirical evidence that the racial composition of a firm's managers affects its polluting practices. Second, it broadens our understanding of resource dependence in organizations, demonstrating a link between the internal resources of the firm (its managers) and the external resources of the community where the firm resides. Third, it provides new insight into environmental inequality, shifting focus away from the siting decisions of toxic facilities and towards the pollution behavior of the organizations responsible for pollution. Fourth, it shows that racial diversity in organizations can have multiplicative effects when it exists in positions of power: Promoting African Americans and Latinos into management benefits those individuals, but it also gives them power to adopt practices that are beneficial to African-American and Latino communities.

Organizations, Communities, and Toxic Pollution

We know little about the organizational sources of toxic pollution and even less about how organizations interact with their local communities to decide how to pollute. A few studies use organization-level data on pollution (e.g., Downey 2006; Crowder and Downey 2010), but they focus on questions of residential exposure and residential mobility, so they aggregate all organizations in a neighborhood and focus on neighborhoods, not organizations. But there are a few exceptions that analyze organizational polluting behavior. In three articles, Don Grant and colleagues explore how an organization's size and whether it is a branch or subsidiary of a larger organization affect its polluting behavior. Grant, Bergesen, and Jones (2002) show that bigger organizations produce toxic emissions at a higher rate than smaller organizations, and this effect is stronger for facilities that are branches of larger corporations. Grant, Jones, and Trautner (2004) elaborate on this to show that branches located far from their headquarters ("absentee management") pollute more. In their conclusion, Grant et al. (2002, p. 403) point out a key question that remained unaddressed by studies of environmental inequality, including theirs: "What organizational characteristics condition how effectively chemical plants manage their toxins in poor and/or minority neighborhoods?" Grant et al. (2010) set out to provide some initial answers to that question, but because they reduce toxic emissions to a binary measure of whether an organization is a "highly risky" polluter or not, their analysis is ultimately another study of the mere presence of a polluting organization in a neighborhood. Moreover, while the study points out the potential importance of how organizational features (size, branch status) interact with their local communities, the article is primarily concerned with criticizing the use of statistical methods in studying organizations and promoting an epistemological perspective that emphasizes in general terms the complex interplay of organizational features, without putting forth any specific theories or arguments about the polluting behavior of organizations.[1]

Why might local communities matter for organizational polluting? There is a long history of environmental activism aimed at curbing

[1] The article's findings are based on cross-sectional data on a single industry. I have been unable to replicate its broad findings using longitudinal data or across other pollution-intensive industries. This suggests that the eight different empirical "configurations" associated with polluting behavior could simply represent an overfitting of single-year, single-industry data. Grant et al. (2002)'s earlier study uses the same data and provides a clearer set of findings grounded in theories of organizational behavior, but it lacks the community-interaction perspective of the later study.

organizations' toxic pollution. Environmentalism as a social movement focused on industrial pollution in the 1960s and 1970s, culminating in legislation establishing the Clean Air Act, the Environmental Protection Agency (EPA), and Superfund. After these successes, its influence waned and it splintered into narrow, localized efforts centered around gender, race, and class inequalities (Gottlieb 2005). Environmental justice began as a social movement organized in African-American churches in the early 1980s, and its influence later spread throughout African-American and Latino communities (McGurty 1997), where it continues its efforts to create barriers against industrial polluters.

Pollution is unusually visible to the community surrounding an organization, because it affects all nearby residents. Coupled with the growth of the environmental-justice movement, we might expect organizational polluting behavior to receive greater community scrutiny than other organizational practices. Resource-dependence theories suggest that organizations must draw important resources from the communities in which they are embedded (Pfeffer and Salancik 1978). For consumer-oriented organizations in industries like retail, it is easy to see how organizations depend on local residents to survive. Organizations like Wal-Mart must respond carefully to the threat of consumer boycotts to maintain profitability (Ingram, Yue, and Rao 2010; McDonnell and King 2013). Ironically, though, highly visible pollution behavior is concentrated in producer-oriented industries like manufacturing and mining, where the support and resources of local residents is rarely necessary for an organization's success. Political pressure can be effective before firms establish their facilities, but once such facilities open, local political actors have little influence on their activities.[2] It is difficult for nearby residents to exert pressure on organizations that do not need them as consumers. This is particularly true for groups that lack political power, such as residents of predominantly African-American and Latino neighborhoods. Thus, it is not surprising that past research has conceived of an organization's environment as including other firms in its industry, important parts of its supply chain, legal and regulatory constraints, and other business-related features, while neglecting the local communities in which organizations reside.

This might sound as if polluting organizations can simply trample over local residents, particularly in poor African-American and Latino neighborhoods. Yet there is substantial variation in polluting behavior even within firms located in such neighborhoods, and the persistence of environmental inequality suggests that communities matter to organizational behavior. Thus, it is too simplistic to suggest that producer-oriented organizations ignore the resources of their local communities. Instead, some features of the communities external to the organizations influence their polluting behavior.

To understand how communities might influence organizational behavior, I focus on the key decision makers in organizations: managers. Resource-dependence theorists have suggested that organizational de-

[2]Moreover, before firms open their facilities, the perceived job-creation and tax-revenue benefits of mining and manufacturing firms often generate local support that welcomes such firms over the objections of concerned residents (Dokshin 2016).

mography plays a role in organizational behavior. Pfeffer (1983) argues that individuals inside organizations must enlist the support of other people within those organizations to solve problems and enact their agendas. He focuses on the importance of age and cohorts (people who enter an organization at similar times) as the key unifying factors that create coalitions that allow organizations to carry out actions. Social demographic characteristics like race and gender are also likely to be important factors in how workers inside organizations muster resources, but little empirical evidence has been brought to bear on characteristics beyond age and tenure. One exception is Dobbin, Kim, and Kalev (2011), who argue that white women play a key role in spurring firms to adopt diversity programs, because such programs support their own interests.

There are thus two different ideas about resource dependence that have previously been studied separately. On one hand, organizations depend on *external* resources, including those they draw from their surrounding communities; on the other hand, organizations depend on *internal* resources such as their workers. It is a short leap to suggest that the link between these two kinds of resources is also important in shaping organizational behavior.

How might the demographic composition of managers and the composition of a firm's local neighborhood shape managers' decisions about how to pollute? Such responsiveness to the local community is not a part of those managers' jobs, may not benefit them personally, and is often *at odds with* the interests of the organizations they serve.

I suggest three (complementary) mechanisms that could create this manager–community link:

1. *Direct community pressure*: African-American and Latino managers respond to pressure by local residents to curb pollution. The environmental-justice movement engages in activism against firms located in predominantly African-American and Latino neighborhoods. The match between the race of managers and the race of local residents/activists may make managers more willing to respond to their concerns, an effect that has been demonstrated in the public sector among bureaucrats and their constituents (Selden 1997; Meier and Stewart 1992).

2. *NIMBY motives among managers*: African-American and Latino managers are more likely to live in the neighborhoods where polluting firms are sited. The literature on neighborhood attainment shows that high-income African Americans (and, to a lesser extent, Latinos) are much more likely to live in poorer neighborhoods with high concentrations of African-American and Latino residents than are high-income whites (Bruch 2014). While workers with higher incomes (like managers) can generally afford to commute longer distances to work, barriers to neighborhood attainment likely keep African-American and Latino managers in neighborhoods close to where they work. This provides a "not in my back yard" (NIMBY) motivation for them to reduce pollution in their organizations, because it directly affects their lives and

the lives of their friends and neighbors.

3. *Racially salient concerns*: The environmental-justice movement has made pollution in minority neighborhoods into an issue that is salient along racial lines. African-American and Latino managers are thus more likely to make efforts to reduce their organizations' pollution when its consequence is to decrease racial inequality, which is what happens when polluting firms are located in predominantly African-American and Latino neighborhoods (cf. Keiser, Wilkins, Meier, and Holland 2002, p. 556, on gender salience). This salience exists regardless of where managers live or whether they experience direct local pressure.

The analysis I present below does not aim to distinguish between these mechanisms—doing so would require getting inside the heads of managers themselves. Instead, I argue that if any (or all) of these mechanisms operate to influence organizational polluting behavior, then the implication is that we should observe an aggregate interaction between the racial composition of a firms' managers and the race of its neighbors. I now turn to the analysis that demonstrates exactly that link.

Data and Methods

Toxic Pollution Emissions Data

The EPA collects annual data on the emission of toxic pollutants at the firm level.[3] The 1986 Emergency Planning and Community Right-To-Know Act (EPCRA) set out a new strategy for the EPA: "regulation by information." The EPCRA is a "sunshine law" aimed at reducing pollution by bringing information on corporate polluting behavior out into the open where it can be viewed by the public. The EPCRA established reporting requirements for firms, which includes all firms in a broad range of industries (mining, manufacturing, wholesale trade, utilities) that meet small minimum levels of chemical use, production, or storage. Based on these reports, each year the EPA releases its Toxics Release Inventory (TRI) data. I use the TRI data from 1987–2012 to analyze the polluting behavior of organizations. These data contain observations on 56,290 unique firms followed over time for a total of 610,085 firm–years.

The key outcome in my analysis is the amount of pollution emitted by each organization. For each firm, we know what toxic chemicals it releases, how much of each chemical it releases, and through what pathways it releases those chemicals. I use this information to construct a measure of *hazard-based pollution* that represent not just the raw amount of chemicals released but instead aims to quantify how

[3] I use "firm" to denote a single establishment in one location that could be part of a larger organization. If I mean to describe a multi-establishment firm, I am explicit.

dangerous those releases are.

The TRI identifies emissions for 609 different kinds of chemicals. It reports the number of pounds of each chemical released in each year. But all chemicals are not equally dangerous. For example, dioxins are so highly carcinogenic that their releases are reported in grams, not pounds. Moreover, chemicals have different levels of toxicity to humans depending on how they are exposed—whether they are inhaled through the air into the lungs or ingested through water or food. For example, asbestos is extremely dangerous when breathed into the lungs but relatively harmless when ingested.

TRI reports identify more than 30 different pathways through which a chemical can be released, including the release of airborne chemicals through smokestacks, the injection of chemicals into underground wells, and the release of chemicals into bodies of water or landfills. Some of these pathways are relatively safe. For example, landfills certified under the Resource Conservation and Recovery Act Subtitle C (RCRA C) must meet stringent EPA guidelines that prevent chemicals from leaching into groundwater. Emissions into such landfills are safe, and firms that take these extra safety precautions are less dangerous in their polluting behavior than firms that dump chemicals into unsafe landfills. The TRI reports thus make it possible to distinguish between organizational polluting practices that pose more or less danger to nearby residents.

To account for the variety of possible chemicals and exposure pathways, I use the EPA's Risk-Screening Environmental Indicators (RSEI) model (Environmental Protection Agency 2015). The RSEI was originally developed to quantify neighborhood-level exposure to toxic pollution. It uses coarse models of air- and water-based diffusion of chemicals in conjunction with TRI data to estimate the risk faced by a resident in a particular neighborhood. However, I am interested in the risk created by a particular *organization*'s pollution—neighborhood exposure is a consequence of this but not the primary outcome.[4] As part of the RSEI model, the EPA gathered thousands of studies on chemical toxicity and used them to create toxicity weights for each chemical that summarize the total health risks of exposure to that chemical. Each chemical has two toxicity weights, one for exposure through air (inhalation) and one for exposure through water (ingestion).[5] The RSEI model also defines the relative safety of different methods through which facilities release chemicals.

I use the toxicity weights along with exposure pathways to compute

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[4]Grant et al. (2010) use an exposure-based measure to analyze organization-level pollution, which conflates the composition of its neighborhood (e.g., population density) with its polluting behavior. They also collapse the large variation in pollution across organizations into a binary measure of whether an organization's pollution is "highly risky" or not, which eliminates much of the analytical leverage present in organizational-level data. The broad choices of which chemicals to use and the large number of ways those chemicals can be released or disposed creates a long continuum of risk levels created by organizations that is not captured in a binary measure.

[5]The RSEI includes toxicity weights for 424 of the 609 chemicals in the TRI. These 424 chemicals account for more than 99% of all emissions. For the 185 chemicals with missing toxicity weights, I imputed their toxicity to be at the 30th percentile of all toxicity weights. Because the weights are skewed—the most dangerous chemicals are a billion times more dangerous than the least—this is very conservative. The 30th percentile of toxicity is twenty times less dangerous than the chemical with the median level of toxicity and a thousand times less dangerous than the chemical at the 80th percentile of toxicity. All results reported below are robust to instead treating these chemicals as harmless (a toxicity weight of zero).

a toxicity score for each chemical released by each firm in each year. This produces a unit-less measure of the hazard of the release that is comparable across chemicals. I then sum the hazard scores over all chemicals produced by each firm to create a measure of the total hazard of the emissions it produces in each year, which can be compared across firms to evaluate the relative danger produced by each one. The distribution of releases (both raw and hazard-weighted) is heavily skewed across firms, so I analyze the log of the hazard score.

Much previous research has relied simply on the total amount of chemicals released. The failure to adjust for the relative safety of different chemicals and different release paths can lead researchers to conclude that two firms with similar gross emissions are equally dangerous when in fact one firm produces pollution that is orders of magnitude more dangerous than a firm that uses safer methods of disposal or less-toxic chemicals. The correlation between my hazard-based pollution measure and the total pounds of chemicals released is 0.016. Studies that rely on pounds-based emissions totals, unadjusted for chemical toxicity and exposure pathway, are studying an outcome that is practically unrelated to the danger created by those emissions.

EEO-1 Data on Firm Demography

The key predictors in this analysis are the racial composition of a firm's managers and the racial composition of its surrounding area. The Equal Employment Opportunity Commission (EEOC) collects annual EEO-1 reports at the firm level that document the race/ethnicity and sex breakdown of all workers in each firm, split by nine job categories. All private U.S. employers with at least 100 workers (or 50 if the firm has a federal contract) are required to submit these reports annually. I use the EEO-1 data from 1986–2011, which matches the 1987–2012 time period covered by the TRI data on pollution, since I use firm characteristics to predict pollution behavior in the year that follows.

I geocoded the firms in the EEO-1 data, allowing me to identify their location by latitude and longitude. This allows me to link firms to tract-level Census data on their surrounding neighborhoods.[6] For Census data, I use the Neighborhood Change Database, which translates Census data from 1970–2010 into common 2010 tract boundaries, which allows neighborhood characteristics to be analyzed over time using comparable geographic borders. Census data are collected decennially, while the EEO-1 and TRI data are annual, so I interpolate between Census years.[7]

Automated Linking of Pollution Data to Firm Demography

The TRI data on pollution and the EEO-1 data on firm demography are collected by different government agencies for very different purposes,

[6] While the TRI data also provide location information, I use my own geocoded EEO-1 locations to match firms to Census tracts, because the TRI data only contain addresses for the most recent year the firm is in the data (see below).

[7] Where past research has used linear interpolation between Census years, I interpolate using a piecewise cubic polynomial (Akima 1970). With five Census data points for each tract (1970, 1980, 1990, 2000, and 2010), I can compare different interpolation methods by leaving out one point in the middle (1980, 1990, or 2000) and using the other four points to interpolate the (known) missing point. I find that piecewise-polynomial interpolation has substantially lower mean squared error than linear interpolation when doing prediction on 1980, 1990, and 2000.

so they have no common identifier that allows them to be linked to one another. Each dataset contains the firm's address, its name, its parent company's name, industry information, and a few other possible identifiers. However, these identifiers are messy and often inconsistent. For example, "ABC Shipping Company, 433 First St Suite 1" in the TRI data might appear as "ABC Co, 433 1st Street" in the EEO-1 data. It is easy for a human to determine that these two observations represent the same firm, but there is no simple characteristic that a computer can use to match these two entities. With 56,290 unique firms in the TRI data, it is not feasible to match them manually.

I use methods from machine learning to develop a novel way to automate the matching of firms in the TRI data to firms in the EEO-1 data. Linking is a prediction problem where the goal is to take two observations, one from the TRI data and one from the EEO-1 data, and estimate a predicted probability that those two firms are a matched pair. To do this, first I manually matched about 200 firms in the TRI data to their corresponding entries in the EEO-1 data. This creates a *training set*, observations where I know the true link status between pairs of firms. (By finding 200 true links, I could also create a large number of known false links, so the training set contains both matches and non-matches with known link status.) This manual-matching step is crucial because it allowed me to identify subtle but important features of the data that could be used for linking. For example, I discovered that the TRI data retroactively updates all old address information for each firm to match its address in the last year it was observed, wiping away any evidence that a firm might have resided at a different address in an earlier period. This made it clear that I should not simply try to match firms in the TRI data to firms in the same year in the EEO-1 data; instead, early years of the TRI data were more likely to match later years of the EEO-1 data because of the TRI's retroactive adjustment of its addresses. This made matching a two-step problem, where first I had to identify whether a firm in the TRI data matched any firm in any year of the EEO-1 data, then I could harmonize the years so the proper time periods were linked.

Second, once I created the training set, I then created a set of "features" (covariates) that might potentially predict that two firms matched one another. I cleaned and standardized all the addresses (e.g., converting all instances of "Street" to "St," spelling out all numbered streets, eliminating suite numbers, stripping punctuation), and I parsed addresses into separate components (street number, street name, street direction, etc.). For company names and parent-company names, I generated many variations—the raw name, the name excluding the 50 most common words in company names (e.g., "Co", "Inc"), only the first word in the name (which is often the most informative), etc. To compare these features between firms, I need a way to quantify how similar two strings are to one another. There are a large number of such "string distance" measures available. Some measures work well for multi-word strings, others for short strings; some work well for matching abbreviations to full words, and others work well for matching pairs where one string has extraneous words added. I use a variety of string-distance measures for different characteristics. Some important characteristics (like firm names) exhibit a lot of variation even within multiple years of the same firm, and I use multiple different string-distance measures to compare them for matches. (Because this is a prediction problem, it is not a problem to have multiple, possibly redundant measures that can be highly correlated.)

Another useful feature uses the fifteen-character ID assigned to each firm in the TRI data. In the manual matching step I noticed that this ID was predictable: it is constructed from the five-digit ZIP code, the first five consonants of the firm's name ("ABC Shipping Co" becomes "BCSHP" since vowels are removed), and the first five non-space characters of the street address ("433 1st St" is "4331S"). Moreover, where the firm names and street addresses in the TRI data were retroactively adjusted to equal the most recent data, the name and address used to construct the 15-character ID were based on the year when the firm first entered the data. This became an important source of matching information for firms that had changed names (often through acquisition) or moved over the course of the study period. I also calculated the geographic distance between the latitude and longitude of the firm as reported by the TRI data and the latitude and longitude of firms I geocoded in the EEO-1 data. In all, I calculated 32 features for every pair of potential matches in the TRI data and the EEO-1 data.[8]

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[8]It is not feasible to calculate these for every possible pair of firms in both datasets: There are over 600,000 observations in TRI data and 7,000,000 in the EEO-1 data, so

Third, I used a machine-learning algorithm called gradient boosting (Friedman 2001) on the training set to estimate how the 32 features I created could be combined to predict whether two firms matched one another. Gradient boosting is built on tree-based methods that partition the data into a large number of subgroups based on their features. This allows it to automatically model complex, multi-way interactions and thresholds between different features. Given a large number of features, we often have little prior information about which ones will be important, and it is extremely difficult to quantify how the features relate to one another.[9] In the example given at the top of this section, a human can see that the two firms match, because we recognize that "ABC" is the important part of the firm name, we see that they have the same street address even though the street names are spelled differently, and we know that suite numbers are often omitted in addresses, so we treat that as unimportant. But we cannot describe an algorithm for reliably combining these features that will work for all the different variations we see in messy data. This is why manually coding matches to create the training set is so important and also why I created so many features to use for matching even though I didn't

all possible pairs would be over four trillion. Instead, I first created a set of "potential" matches by matching exactly on city name and state, so each firm in the TRI data is matched only to every firm in the EEO-1 data in its same city and state. Cities are sometimes misspelled, so I used fuzzy string matching on city names so minor typographical errors would not prevent a firm from entering the potential match set.

[9]Note that Feigenbaum (2016) finds that logistic regression performs best for linking Census records, but that is based on a much smaller set of features (name, age) with far less variation than what we see in firm names, addresses, and other identifiers. I find that without the automatic interactions, thresholding, and feature selection provided by gradient boosting, match quality is very poor.

know which ones would ultimately be useful: An algorithm like gradient boosting takes the matches I provided and attempts to uncover the unknown (latent) process through which the human coder determined the matches. The algorithm can then be used to determine whether any two firms (even those that weren't part of the training set) match one another.

Having "trained" the gradient-boosting model on the manually generated matches, I then used the model to compute the predicted probability that any two potential matches did in fact match each other. This segmented the data quite effectively—most of the predicted probabilities were very close to zero (since the bulk of pairs do not match), and out of the rest, most of the probabilities were above 0.9. For every unambiguous pair with match probability greater than 0.9, I treated it as a match.[10] Some firms had multiple potential matches above 0.9. The bulk of these represented matches between the same firms in different years and were easy to reconcile. I treated all remaining ambiguous cases as non-matches and did not use them in the analysis.

This resulted in matching 20,983 unique firms in the TRI data to the EEO-1 data, and these were matched over time, resulting in 205,181 firm–years. This represents a match rate of 37%. This is to be expected, since the EEO-1 has a size threshold that limits which

[10]Manually inspecting the matches made it clear that anything in the middle of this range was probably not a match. Pairs in the 0.6–0.8 range were often ambiguous: a lot of the information looked like it matched and it was impossible to rule out, but it was hard to be certain. Everything with probability above 0.8 looked like a true match, but to be conservative I used 0.9 as the cutoff.

firms are included, and while the EEO-1 covers almost half of the U.S. labor force, it covers fewer than half of all firms, since small businesses make up the long tail below the size cutoff. Since I was conservative in classifying two firms as a match, the linking process should introduce relatively little noise into the data; as long as firms that are not linked do not differ from firms that are linked in important ways related to the analysis, linking should not create any bias. There is no evidence that the firms I failed to link to the EEO-1 differ from the firms that I did link. I would expect the firms that cannot be linked to produce less pollution, since they are more likely to be small firms that fall below the size cutoff of the EEO-1 data. While the mean level of hazardous emissions is smaller in unlinked firms, the difference is small, and on the log scale the distribution of pollution in linked and unlinked firms is almost identical.

Modeling Organizational Pollution

To understand the effect of an organization's managerial demography and its neighborhood context on toxic pollution emissions, I estimate the following linear regression model:

$$\mathrm{Y} = \beta_0 + \beta_1 \mathrm{RM} + \beta_2 \mathrm{RN} + \beta_3 (\mathrm{RM} \times \mathrm{RN}) + \gamma \mathbf{X} + \delta_t + \alpha_j + \epsilon,$$

where the outcome Y is the log hazard of total toxic emissions in a firm, lagged one year; RM is the race of managers, i.e., the proportion of managers who are African American or Latino; RN is the race of the neighborhood, i.e., the proportion of neighborhood residents who are African American or Latino; **X** is a matrix of control variables; δ_t represents year fixed effects, where t indexes time; α_j represents firm fixed effects, where j indexes firms; and ϵ is an error term.

Our quantity of interest is the marginal affect of additional racial diversity in management:

$$\frac{\partial Y}{\partial \mathrm{RM}} = \beta_1 + \beta_3 \mathrm{RN}$$

This is the effect of adding additional African-American and Latino managers (∂RM) on a firm's pollution output (∂Y), given the neighborhood's racial diversity RN. I am not interested in the individual regression coefficients β_1, β_2, and β_3 but rather in their joint effects. Indeed, despite common practice to the contrary, when interacting two continuous covariates like RM and RN, the individual regression coefficients are not interpretable as interesting marginal effects, and inferences about the marginal effects are impossible using only the individual coefficients (Brambor, Clark, and Golder 2006).[11] I present results that focus instead on the key quantity of interest, the marginal effect $\partial \mathrm{Y}/\partial \mathrm{RM}$. If this marginal effect is negative (for a particular value of a neighborhood's racial composition), that means that more racial diversity in management leads to less toxic pollution.

[11] To make inferences about the marginal effect of interest here, one needs the full variance-covariance matrix of the regression coefficients.

Control Variables

To identify the marginal effect of interest, I control for a number of factors ($\mathbf{X}$, δ_t, and α_j) that I expect *a priori* to affect both the key predictors and the outcome.

- Firm fixed effects (α_j): These control for all unmeasured characteristics of firms that do not change over time. Many organizational characteristics might confound the effect of racial diversity on pollution, and these control for many such factors. These include a firm's industry, its state, its city (for nearly all firms; few firms move in these data), its time of founding (imprinting; Stinchcombe 1965), and any fixed taste for discrimination or progressivism it might have.

- Year fixed effects (δ_t): These control for any secular trends that affect all firms equally, including decreasing pollution over time and increasing worker diversity over time. The also control for changes in federal environmental regulations.

- Firm gender composition: We know that women in management promote policies aimed at increasing racial diversity (Dobbin et al. 2011). We do not yet know anything about the effect of gender diversity on pollution, though my own preliminary results suggests there is an association. We might also suspect that the proportion of women in both managerial and non-managerial roles can serve as a proxy for the level of time-varying "progressivism" in a firm, which would affect both racial diversity and pollution levels.

- Firm size: Bigger firms pollute more (Grant et al. 2002), and bigger firms are more racially diverse (Holzer 1998).

- Local economic trends: I include state-level GDP growth and state-level unemployment. Minority workers are often the first to be laid off during hard economic times (Couch and Fairlie 2010), and recessions can mean lower levels of industrial production, which often means fewer emissions.

- Federal-contractor status: Contractors are subject to additional equal-employment-opportunity regulations and have higher racial diversity (Leonard 1990), and contractors are likely to be more sensitive to regulation in general (Dobbin, Schrage, and Kalev 2015), so we might also expect them to pollute less.

- Branch status: We know that branches of multi-establishment firms pollute more (Grant et al. 2002). Little evidence exists on how branch status might affect the diversity of workers, but there is also no reason to believe that it is post-treatment (i.e., that racial diversity causes branch status), so there is no risk in controlling for it.

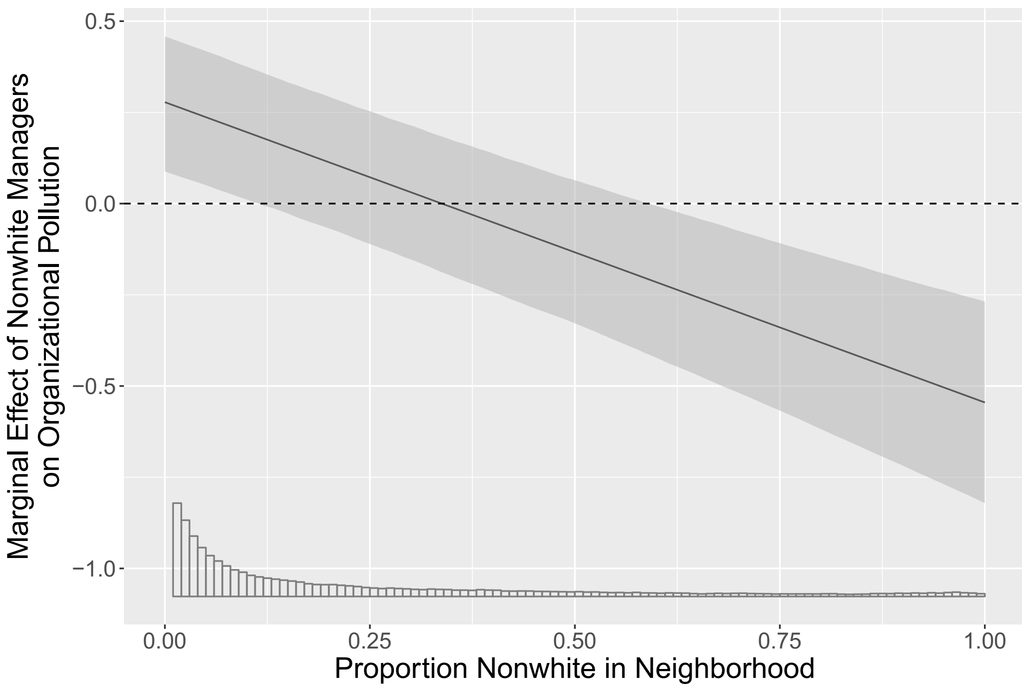

Figure 2.1: *The key marginal effect $\partial Y/\partial RM$ as it varies across firms located in neighborhoods with different proportions of African-American and Latino residents.*

Results

Figure 2.1 plots the estimates of the key quantity of interest. The y-axis shows the size of the marginal effect of adding additional African-American and Latino managers on a firm's pollution emissions ($\partial Y/\partial RM$). The x-axis shows the proportion of African Americans and Latinos who live in the neighborhood. The figure shows how the effect of a firm's managerial demography changes as the firm's neighborhood changes. The gray bands plot 95% confidence intervals, so the marginal effect is statistically significant (at the 0.05 level) wherever the interval does

Table 2.1: *"Nonwhite" here is short for the proportion of African Americans and Latinos. Models include year fixed effects and controls for firm size, branch/subsidiary status, federal-contractor status, local economic conditions (state GDP and unemployment), and gender composition of managers and non-managers; N=173,833 (18,033 unique firms).*

	log(Pollution)
Proportion Nonwhite Managers	0.278^{**}
	(0.095)
Proportion Nonwhite Neighborhood	-0.396^{*}
	(0.171)
Managers × Neighborhood	-0.822^{***}
	(0.128)
Firm Fixed Effects	Yes

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

not cross zero.

For firms in neighborhoods with at least 60% African-American and Latino residents, adding additional African-American and Latino managers results in a statistically significant *decrease* in the pollution produced by those firms. This decrease is more pronounced as the neighborhood proportion of African-American and Latino residents grows. This provides evidence that an organization's managerial demography interacts with demographic characteristics of its local community to change its polluting behavior.

(Table 2.1 shows regression coefficients for the key predictors; full tables can be found in Appendix Table C.1.)

The histogram along the bottom of the x-axis in Figure 2.1 shows the distribution of neighborhood–race proportions in the sample. Neighborhoods with more than 60% African-American and Latino residents constitute about 20% of the sample. This represents about 4,000 unique firms and almost 40,000 observations. This shows that that model is not extrapolating beyond the range of the data to estimate this effect—there is a lot of data in the tail of the distribution to estimate this. Moreover, this also shows that these effects matter for a substantial portion of the organizations in the sample.

What do these marginal effect sizes mean in substantive terms? Table 2.2 shows the percent change in an organization's pollution for six different representative situations. The columns show two different levels of change in the proportion of African-American and Latino managers. The first column shows the effect of a firm moving from having 5% to 10% African-American and Latino managers, and the second column shows what happens when it moves from 5% to 15%. At the start of the analysis period (1987), 7% of the average firm's managers were African American or Latino. At the end of the period (2011), this figure rose to 12%, a five percentage-point average gain comparable to the predictions in the first column. The rows show how this varies depending on the neighborhood where the firm is located. It shows effects for neighborhoods with populations that are 30%, 60%, and 90% African American and Latino.

A five percentage-point increase in a firm's racial managerial diversity produced, on average, a 1.2% decrease in pollution among firms located in neighborhoods that were 60% African American and Latino and a 3% decrease in pollution for firms in neighborhoods with 90%

Table 2.2: *Predicted decrease in an organization's pollution after a change in the racial composition of the organization's managers, conditional on the composition of its surrounding neighborhood.*

		Change in Nonwhite Managers	
		5%→10%	5%→15%
Neighborhood Nonwhite	30%	No change	No change
	60%	1.2% decrease	2.4% decrease
	90%	3% decrease	5.9% decrease

such residents. A ten-percentage-point increase nearly doubles these effects (recall that the model is on the log scale, so the effect is not linear) to a 2.4% decrease in 60% neighborhoods and a 5.9% decrease in 90% neighborhoods. (As expected from Figure 2.1, firms in 30% neighborhoods show no effect of increased managerial diversity.) Between 1987 and 2012, the median firm reduced its pollution by 65%, so these effects could account for a substantial portion of that reduction.

Moreover, firms located in neighborhoods with 60% or greater proportions of African-American and Latino residents have a large labor pool from which to recruit managers to increase their diversity. The number of African-American and Latino managers is much higher for firms located in these neighborhoods. The firms in the sample located in neighborhoods with at least 60% African-American and Latino residents had, on average, 14% African-American and Latino managers at the start of the analysis period in 1987, and that figure grew to 22%, on average, in 2011. This average gain of eight percentage points puts these firms closer, on average, to the large marginal effects shown in the second column of Table 2.2.

Note that Figure 2.1 also shows the marginal effect of adding African-American and Latino managers to firms in all-white (or nearly all-white) neighborhoods: it increases pollution in those firms. The theory I sketched at the beginning of this paper has no clear implications for this situation, so this finding should be seen in a more speculative light. Controlling pollution is costly for organizations, and the positions of African-American and Latino managers are likely to be more vulnerable than white managers. Given these things, it could be that African-American and Latino managers are more likely to toe the corporate line when the issue of pollution hasn't been made racially salient and provided a reason for them to expend scarce resources to curb pollution. (This is consistent with Kanter's (1993) arguments about conformity pressures for women managers.) Evidence that managerial diversity had no effect in all-white neighborhoods would be consistent with the implications of the three mechanisms outlined in the theory above. It is the presence of a positive effect—increased pollution—that needs an additional mechanism (conformity pressure) to explain. This provides a promising avenue for future exploration.[12]

Robustness to Alternative Explanations

Firm fixed effects control for a variety of potential confounders, including any fixed taste for discrimination or polluting behavior that

[12]One might suggest instead that this finding captures the fact that firms in all-white neighborhoods are better behaved and have less room to decrease pollution, but that explanation is not consistent with the descriptive data.

a firm might have. If these tastes are correlated, we might think of them together as a firm's "progressivism." Year fixed effects control for aggregate changes in the progressivism of firms—the willingness to hire and promote both women and racial minorities to management has increased over time, and so have normative pressures on firms to become "greener." What if firms' levels of progressiveness change over time in ways that are out of sync with aggregate trends? This could confound our results: if racial diversity and pollution control are two components of the same unmeasured progressiveness, then the effects we estimate might simply reflect that.

To test this potential for confounding by unmeasured progressiveness, I treat the proportion of women in management as a noisy proxy for progressiveness. If my findings on racial managerial demography simply say that more progressive firms pollute less when located in predominantly African-American and Latino neighborhoods, then the same thing should be true for women in management. But when I estimate a model interacting the proportion of women in management with the proportion of African-Americans and Latinos in a firm's neighborhood, the marginal effect is flat—there is no significant interaction between women managers and neighborhood racial composition. This provides some confidence that the main findings are not confounded by unmeasured progressiveness. If such a confounder exists, it would have to cause firms to promote more African Americans and Latinos into management but not more women, *and* it would have to decrease the firm's pollution emissions.

One such confounder might relate to the firm's local labor market: while women are distributed equally across space, African Americans and Latinos are not. Perhaps the main findings reflect something about differences in local labor markets for firms with many African-American and Latino managers. To test this, I substitute the proportion African-American and Latino among *non*-managers, and estimate the model using them instead (with and without controlling for the main effects of managerial racial composition). I find no evidence that an organization's polluting behavior in predominantly African-American and Latino neighborhoods changes with its proportion of African-American and Latino non-managers. This provides more evidence that the observed effect is not a result of confounding with unmeasured progressivism. It is also consistent with the implications of the theory sketched above, which relies on African Americans and Latinos occupying positions of power (management) to bring about a reduction in pollution.

Because the model uses firm fixed effects, it relies entirely on within-firm changes to estimate the marginal effects of interest. From a causal-inference perspective, this is a stringent, conservative test, since it does not rely on comparisons between very different firms. However, most neighborhoods do not change much in their racial compositions over time, and only firms in changing neighborhoods contribute variation to these estimates. We might expect these forces to operate differently in predominantly minority neighborhoods that never change. If the effects are stronger in such neighborhoods, the fixed-effects estimates will understate the average marginal effect sizes. To examine this, I estimate a model without firm fixed effects. I find that the marginal effects are much larger. (Column (1) in Appendix Table C.1 reports these results, and the marginal effect is plotted in Appendix Figure C.1.) These effects should not be taken as realistic, however, since they fail to account for all the stable firm attributes that are controlled for by including fixed effects. If your prior is that between-firm differences also matter for the marginal effects, then the fixed-effects marginal effects reported in Figure 2.1 are likely too small, and the true effect lies somewhere in between these two estimates.

It is common to treat interactions as linear, but growing evidence suggests that this is often an unreasonable assumption. I test the linearity assumption using the binning estimator and the kernel estimator proposed by (Hainmueller, Mummolo, and Xu 2016). I find some evidence of nonlinearity at the far left end of Figure 2.1: the marginal effect appears to slope more sharply upwards for neighborhoods with very few African-American and Latino residents. Beyond that, though, the marginal effect curve is essentially linear once it passes about 15% neighborhood African American and Latino. Above that level, the curve hews remarkably close to the line plotted in Figure 2.1. This does nothing to change the main story: the marginal effects in predominantly African-American and Latino residents are the same. It does, however, suggest that the positive effects (increase in pollution) observed for firms in all-white neighborhoods might be even larger than those estimated by the linear model in Figure 2.1. However, this might also be a case where the linearity assumption is effective in smoothing out noise in one tail of the marginal-effect estimate, so I emphasize the more conservative magnitude of the effect in all-white neighborhoods estimated by the model in Figure 2.1.

To test whether the effects are ephemeral and whether I have posited causality in the right direction, I estimated the main model with the outcome (pollution) on a five-year lag (instead of a one-year lag). I find the same effects as with the one-year lag, with the magnitudes only slightly attenuated. This suggests that the decreases in polluting behavior happen relatively quickly (since they exist with one-year lags) but also that they persist.

Finally, throughout the paper I combine African Americans and Latinos, both in my measure of management diversity and of neighborhood racial composition. This makes theoretical sense: all the reasons that environmental inequality is salient along racial lines (including the environmental-justice movement) apply to residents of both predominantly African-American and predominantly Latino neighborhoods. Moreover, the combination of the residential segregation of even high-income racial minorities (Bruch 2014) and spatial factors that push people to work close to where they live (Fernandez and Su 2004) means that African-American managers are more likely to be found in firms located in neighborhoods with high proportions of African Americans, and similarly for Latino managers. By combining them, I am likely to average their effects, not conflate the effects of two different groups.

But one might still want to know what happens if the groups are split and the effects are estimated separately. The results are consistent with the main argument. When firms in predominantly African-American neighborhoods add African Americans to management, they see very similar changes in pollution as the estimates I show in Figure 2.1. The magnitudes are the same, but the uncertainty intervals are a bit wider (since the estimates are based on fewer firms and fewer neighborhoods), and the marginal effect becomes statistically significant in neighborhoods with at least 67% African Americans, not 60%. But this is entirely due to the change in uncertainty, not a change in magnitude. When I do the same thing for firms in Latino neighborhoods with Latino managers, the effects are somewhat weaker in magnitude. This makes sense, because until recently Latinos made up a much smaller proportion of managers than did African Americans, and Latinos are less segregated than African Americans, so there are fewer neighborhoods that are predominantly Latino. Neither of these alternative models are inconsistent with the main findings. Instead, I conclude that it makes sense to combine these two groups not only for theoretical reasons but also for reasons of statistical power.

Conclusion

In this paper I suggest that managers' behavior within organizations depends on the neighborhoods where their organizations are located. In support of this, I show that increasing the proportion of African

Americans and Latinos in a firm's management leads that firm to decrease its production of toxic pollution, but that is true only when the firm is located in a predominantly African-American or Latino neighborhood. This opens up a new dimension to our understanding of resource dependence within organizations: it suggests that the internal resources mustered by an organization's managers depend on the external resources of the organization's surrounding community. I describe three mechanisms through which this novel manager–community link might function: race matching between managers and members of local social movements, self-interested NIMBY behavior by managers on behalf of their racial groups, and the responsiveness of managers to issues that are racially salient (such as pollution, which is racially salient thanks to the environmental-justice movement).

The evidence I present is consistent with all three mechanisms, and an important avenue for future research will be to understand which mechanism(s) are responsible for the aggregate effects we observe. One way to explore the social-movement mechanism would be to collect detailed data on local environmental-justice activism and look at whether the effects shown in this paper hold when one substitutes local environmental-justice activism in place of neighborhood racial composition. Of course, measures of social-movement activities are notorioustly difficult to collect. Newspaper stories are one traditional data source, but because the environmental-justice movement is concentrated in mostly poor, minority neighborhoods, it is typically ignored by major newspapers. To test the NIMBY mechanism, matched employer-employee data that contains information on where employees live and where employees work could be linked to organization-level pollution data. In this paper, I also presented a novel method for automating the linking of large data on organizations, and that might facilitate such a test. Tests of racial salience will be more difficult to carry out using aggregate data. Interviews with individual managers and case studies of polluting organizations could shed light on manager motives. Organizational psychologists are also well equipped to study racial salience and decision making in an experimental setting.

Resource-dependence theorists have not previously explored the manager–community link, and it has implications for other organizational outcomes, not just a firm's polluting behavior. Such a process might also be evident in labor-market outcomes. We know that women advocate for diversity practices in hiring and promotion (Dobbin et al. 2011), and we might expect the managerial-demography mechanism to work through race as well. My findings suggest that the composition of firms' communities could amplify the effects of managerial demography on diversity in employment in the same way that they do for organizational pollution. Manager–community mechanisms might also contribute to an organization's choices for charitable giving and other forms of corporate social responsibility.

Another important direction for future research is to determine in more detail what is happening at the level of organizational practices to achieve decreases in pollution. In the data I construct, I build a measure of pollution that aggregates a wide range of practices related to pollution, from the choices of toxic chemicals to use to the choices of how to dispose of them. This measure weights all these choices to produce a single value that summarizes the risk created by all the polluting practices chosen by an organization. But the TRI data collected by the EPA also makes it possible to disaggregate these practices. For example, one might analyze whether the pollution danger produced by a firm decreases because it switches to safer chemicals, because it switches to improved methods for disposal (recycling, off-site treatment), or because it simply reduces its toxic output. Showing how managerial demography and the manager–community link affects these more specific practices would strengthen our confidence that pollution reduction is a result of decisions made within organizations.

For policy makers, activists, and managers concerned about organizational pollution and environmental inequality, the results I present suggest that efforts to increase the numbers of African Americans and Latinos in management might have multiplicative benefits for African Americans and Latinos in the aggregate. Promotion to better, higher-paying managerial jobs is good in its own right. Placing African Americans and Latinos in positions of power also allows the voices of traditionally marginalized groups to be heard, and it allows managers to enact policies and practices that benefit these groups.

Chapter 3

"White Flight" among Employers: Evidence from Firm Relocations, 1971–2011

Abstract

From the 1960s until recently, jobs in the U.S. increasingly shifted out of central cities and into suburbs. This shift stoked fears that black residents, stuck in central cities because of all the forces behind residential segregation, had been left without ready access to suburban jobs. Even if employers did not intend to distance themselves from black residents and were simply pursuing more highly skilled suburban workers, the end result would be to create new barriers to black employment. But were firms behaving in a race-neutral way, or was race a factor in their preferences for suburban neighborhoods? I use longitudinal data on firms' locations to analyze the forces that lead firms to relocate, along with the neighborhood features that attract and repel them when they move. I find that firms do take race into account in their location decisions. Firms in neighborhoods with greater shares of black residents are more likely to leave those neighborhoods, and they are more likely to move to neighborhoods with

fewer black residents.

Do firms flee black neighborhoods? This question is raised by nearly every study in the voluminous literature on spatial mismatch, where it looms as a background assumption or as a potential mechanism to explain why most jobs are located far from black residents. Despite its prevalence, no previous study has documented the influence of neighborhood racial composition on firms' relocation decisions. In this paper, I examine what factors are associated with firms' decisions to move and what kinds of locations attract and repel them.

We know that jobs have shifted from cities to suburbs over the past several decades, while black residents continue to be segregated in central cities, leaving black residents on average farther from job opportunities, both in terms of commuting and information about jobs. These are the key empirical patterns underlying the spatial-mismatch hypothesis (Kain 1968; for a review, see Fernandez and Su 2004). A key assumption in Kain's formulation is that spatial mismatch results from discrimination and segregation in the housing market, not the labor market. Employers are race-neutral, moving to areas with lower land costs and access to skilled suburban workers. If employers are instead relocating in an active effort to avoid black workers, it changes the way we might try to remedy racial inequality in employment.

Two landmark case studies of firm relocations have deepened our understanding of spatial mismatch. Zax and Kain (1996) examine the relocation of a firm from central Detroit to the suburb of Dearborn, using detailed employment records to examine how workers responded to the move. The move placed the firm much closer to where its white workers lived and farther from where its black workers lived. Unable to move to neighborhoods closer to the new location and unable to bear the increased cost of commuting, many black workers quit—evidence that spatial mismatch is at work. Some of these black workers sued the firm, alleging that the move was discriminatory, which raises questions about the nature of how spatial mismatch operates. Were black workers "left behind" as an unintended consequence of the move, or did the firm use relocation as an instrument to get rid of its inner-city black workers? Without learning more about the firm's preferences and its relocation decision, it is impossible to know the answer.

Fernandez (2008) studies the relocation of a Milwaukee firm, and he does have clear information about why the firm chose to relocate: it was trapped in a dense city location with no room for expansion, and the addition of new machinery had already complicated operations at this location. Fernandez also describes how the firm chose its destination: it studied multiple potential locations based on the residential locations of its current workers, and it chose the one that minimized disruption to their workers. The firm guaranteed jobs for all existing workers at the new location, and it actively recruited them to move with the firm. Despite these efforts, the relocation hurt black workers, because their housing options were more constrained than those of white workers because of all the factors behind residential segregation. For Fernandez, this move represents a "best case" scenario for a firm move, in that the firm was clearly not trying to rid itself of black workers.

These two relocation studies offer compelling evidence that black workers face more obstacles than white workers in adapting to relocations by their employers. These studies use firm relocation as an identification strategy to better understand the *consequences* of the shift in employment away from central cities, but they tell us little about what *causes* employers to relocate more generally. Moreover, we do not know how the finding that black workers are disproportionately hurt by relocation translates into aggregate inequality, because there is no research on how often firms move away from predominantly black neighborhoods or to what kinds of neighborhoods they move.

One partial exception to this is Iceland and Harris (1998), who use data from the Multi-City Study of Urban Inequality (MCSUI) to study firms' intentions to move. MCSUI surveyed firms on whether they were "currently planning to relocate." Iceland and Harris find that firms located in neighborhoods with growing black populations were more likely to say that they intended to move. But the findings were mixed: the effects were evident only in Los Angeles and in Boston, while neighborhood racial composition did not predict intentions to move in Atlanta or Detroit. The MCSUI survey was cross-sectional, so it doesn't contain data on whether any of the firms followed through with their intentions to move. It is difficult to know if firms are fleeing black workers without data on whether firms act on their intentions to move and, more importantly, without knowing what kinds of neighborhoods they move *to*.

Other research on race and employment gives us reason to believe that employers might make efforts to avoid black workers. Cole and Deskins (1988) provide some of the only evidence from the post-Civil-Rights-Act period showing that firms make initial siting decisions based on race. They find that Japanese auto makers that came to the U.S. in the 1980s chose locations in mostly white areas and assert that they did this to avoid black workers. More generally, sociologists have found evidence of all kinds of labor-market discrimination against black workers (Pager and Shepherd 2008). If employers are willing to discriminate at the hiring stage to avoid black workers, it is reasonable to think that they might relocate to prevent black workers from entering the hiring pipeline in the first place. But previous research provides little firm-level evidence to evaluate this claim, and this claim is important for understanding how employers' actions help to create racial inequality in the labor market.

Economic geographers have studied firm location choices (and, rarely, relocation choices). This literature is dominated by evidence on how firms choose what region or what city in which to locate and, because of data availability, focus mostly on Europe (Pellenbarg, van Wissen, and van Dijk 2002). Studies focus primarily on how local fiscal policies and regulatory regimes attract new businesses and on the formation of agglomeration economies. But spatial mismatch has primarily been studied *within* cities. Some more recent studies have focused on U.S. counties, which can shed light on firm locations within metropolitan areas, but research in this area focuses on similar factors—local taxes, industrial concentration, and (much less often) labor-market factors like wages (Arauzo-Carod, Liviano-Solis, and Manjón-Antolín 2010). Most of these studies focus on firms' initial siting decisions, so they lack information about firms before they move in, and they conflate the decision of where to locate with the decision of whether to enter the market at all.

All these location studies require firm-level data on location choices. Of course, it is far easier to find aggregate measures of where jobs are located, and these measures tell us a great deal about the spatial distribution of employment opportunities in the U.S. But they tell us little about the mechanisms that create this spatial distribution. To do that, we need to bring the firm back in.

I construct the first longitudinal, national, micro-level data on firm relocations in the U.S. The data provide a comprehensive picture of firm mobility, including every relocation by a private employer in the U.S. with at least 100 employees, from 1971–2011. I use a new model of relocation decisions that allows me to simultaneously estimate a firm's decision to move and its choice of destination while disentangling the effects of neighborhood features that keep the firm in the same place from the features that attract it to new destinations. I focus on firm relocations within metropolitan areas, because those are the moves most relevant to our understanding of how employer locations contribute to racial inequality.

I find that firms flee black neighborhoods. The greater the share of black residents in a firm's neighborhood, the more likely it is that the firm will choose to relocate. And in choosing a new location, more black residents make a neighborhood less appealing as a destination. I adjust for a variety of other neighborhood characteristics, including the skill level of the neighborhood residents, the cost of the neighborhood, and the density of employment in the neighborhood. This suggests that firms are not race-neutral seekers of optimal locations; race matters in where they choose to do business. But I show that this is not universal. Majority-black firms do not flee predominantly black neighborhoods. Given enough black workers, the number of black residents in a neighborhood *increases* the likelihood that a firm will stay in that neighborhood or choose it as a destination when it relocates.

A better understanding of how firms choose their locations enriches our understanding of how workers' residential decisions contribute to racial inequality in the labor market. Evidence that firms are not race-neutral in their location choices suggests that improving job access for black workers (through housing and transportation policies) will do little to improve the employment prospects of black workers if it is not accompanied by changes in employer preferences.

Data

EEO-1 Data for Firm Location History

Under Title VII of the Civil Rights Act of 1964, which established the Equal Employment Opportunity Commission (EEOC), private U.S. employers submit annual EEO-1 reports that document the race and sex composition of their workers, split into nine occupational categories. This applies to all firms with at least 100 employees—or 50 employees for firms with federal contracts. (Before 1983, these cutoffs were 50 and 25 employees, respectively.) Each firm in the data is assigned a unique identifier by the EEOC that allows that firm to be followed over time. I use these data from 1971–2011, which cover almost half of the U.S. labor market. The full data contain observations on one million unique firms, and each firm appears in the data for seven years on average, for a total of seven million firm–years of data.

One feature of the EEO-1 reports has so far received little use: The EEO-1 reports also contain each firm's address. Each establishment in a multi-establishment organization must submit a separate report, so locations are available for each one separately. Humans fill out these forms, and much of the sample period occurs before the forms were computerized, so the addresses are often messy: they are full of non-standard abbreviations and extraneous information, and the same address for the same firm is often inconsistent across years. After carefully cleaning the address data, I geocoded each firm's address, giving me the latitude and longitude of each firm's location. The data provide a comprehensive, longitudinal portrait of the spatial distribution of employer locations in the U.S.

Because the data are longitudinal, I can use the data to identify firm relocations. I am conservative in classifying firms as moving, because false negatives (actual moves that I classify as non-moves) will influence the analysis less than false positives, since moves are relatively rare (about 5% of firm–years), and moves create much of the variation in the data. For the analysis in this paper, I look only at moves that take place within a firm's metro area (its core-based statistical area, i.e., CBSA), so I exclude firms outside CBSA's. I limit the analysis to metropolitan areas (excluding micropolitan areas)—these include 85% of the U.S. population. I also exclude firms with any gaps of more than two years in the data. Firms sometimes come in and out of the EEO-1 data, possibly because they fall under the size required for reporting, and if they move during a gap in reporting, I can't pinpoint the year in which they moved. Finally, I exclude firms that are only present in the data for a single year, since a single year provides no information about whether a firm relocates. After applying these restrictions, I am left with a sample of 523,117 unique firms followed over a total of 3,728,710 firm–years.

I use each firm's geocoded location to link it to its Census tract in each year. I then merge the firm-level data with Census data from the Neighborhood Change Database (NCDB). The NCDB harmonizes Census tracts over time so Census data from 1970–2010 can be analyzed using constant 2010 tract boundaries. Since the EEO-1 data (and thus my location data) are annual, I interpolate data for the years between decennial Censuses.[1]

[1] Where past research has used linear interpolation between Census years, I interpolate using a piecewise cubic polynomial (Akima 1970). With five Census data points for each tract (1970, 1980, 1990, 2000, and 2010), I can compare different interpolation methods by leaving out one point in the middle (1980, 1990, or 2000) and using the other four points to interpolate the (known) missing point. I find that piecewise-polynomial interpolation has substantially lower mean squared error than

Analyzing Firm Relocation Choices

The goal of my analysis is to find out whether a firm's choice to move out of its current neighborhood and its choice of a destination neighborhood is influenced by the racial composition of those neighborhoods, net of other neighborhood features and characteristics of the firm. Sociologists have studied inter-neighborhood migration and neighborhood preferences in great detail (Bruch and Mare 2012), and I build on the methods used in that literature to study firm relocations and neighborhood preferences. I use a new parameterization of a discrete-choice model developed by Steele, Washbrook, Charlton, and Browne (2016) that makes it possible to simultaneously estimate a firm's decision of whether to move along with its choice of destination, while disentangling the *inertia effects* of its current neighborhood (factors that makes it more likely to stay put) from the *pull effects* of other neighborhoods (factors that make it more likely to move to particular destination).[2]

The model takes the same general form as a classic discrete-choice

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linear interpolation when doing prediction on 1980, 1990, and 2000.

[2]Steele et al. (2016) separate effects into three categories: push, pull, and inertia. They use "inertia" to refer only to firm-level characteristics that predict whether a firm stays at the same location, and they use the term "push effects" to describe how the characteristics of the neighborhood where the firm currently resides affect its decision to stay put. For clarity, I group both of these factors under the umbrella term "inertia" since they both predict whether a firm stays at its current location. I use "pull effects" in the same way that they do—to describe features that "pull" firms to move to particular destination neighborhoods. The "push" vs. "pull" distinction is also made in the economic geography literature, but it can be unintuitive to try to interpret positive "push" effects to mean that a firm is *less* likely to be "pushed" out of a neighborhood, which is the standard usage.

model:

$$\Pr(y_{i(t+1)} = r \mid \eta_{rit}) = \frac{\exp(\eta_{rit})}{\sum_{k \in C_{it}} \exp(\eta_{kit})}$$

Our goal is to predict the probability that firm i in year t chooses to locate in a particular location r (where r can also be its current location, i.e., a decision not to move). We call this location choice our outcome $y_{i(t+1)}$, with $(t+1)$ referring to the fact that we are interested in predicting the location of the firm in the following year. This probability is conditional on any characteristics of the firm and characteristics of the set of neighborhoods C_{it} that it chooses between. I'll refer to C_{it} as the firm's choice set, which can vary across firms and across time. These characteristics combine to form a linear predictor η_{rit}, which is specific to each possible neighborhood choice. In the classic model, the linear predictor is a simple linear equation with a coefficient for each choice-specific covariate.

Steele et al. (2016) point out that longitudinal data on location choices makes it possible to identify substantively interesting effects that have been overlooked in previous research. Bruch and Mare (2012) first point out that longitudinal data makes it possible to treat a firm's (or in their case, a resident's) current location as different from its other choices. Firm moves are relatively rare, and we would like to model the decision to stay put simultaneously with the decision of where to move. Among other things, this allows the decision to move to depend on the other available neighborhood choices. Bruch and Mare show that adding a dummy (indicator) variable ω_{rit}, which equals one for a firm's current location, captures its propensity to stay in one place (its "inertia"). Interacting ω_{rit} with firm and neighborhood characteristics allows us to estimate how other features of the firm and the neighborhood where it's currently located affect its inertia.

Building on this, Steele et al. (2016) go one step further and show that by using ω_{rit} and its complement, $1 - \omega_{rit}$, we can separate the inertia effects of a particular neighborhood characteristic from its pull effects. For example, it might be the case that firms are more likely to move out of a black neighborhood (i.e., the inertia effect of the proportion black in a firm's current neighborhood is *negative*) but that race plays no significant role in its choice of destination neighborhood, conditional on choosing to relocate (i.e., the pull effect of neighborhood race is approximately zero). Or inertia and pull effects could go in the same direction but have very different magnitudes. This parameterization also makes it possible to estimate the effect of firm-specific features on the firm's choice of whether to move or not. Typically this is not possible, since firm-specific features do not vary across the firm's potential neighborhood choices; interacting firm-specific features with ω_{rit} makes them vary (ω_{rit}=1 only for the firm's current location) and makes it possible to estimate the inertia effects of firm-level predictors.

This model parameterizes the linear predictor η_{rit} as follows:

$$\begin{aligned}\eta_{rit} = \omega_{rit} &\left\{\alpha_0 + \alpha_1 x_{it} + \beta_0 z_{rt} + \beta_1 x_{it} z_{rt}\right\} \\ &+ (1 - \omega_{rit}) \left\{\gamma_0 z_{rt} + \gamma_1 x_{it} z_{rt}\right\},\end{aligned}$$

where x_{it} are firm-specific features and z_{rt} are neighborhood-specific features. α and β estimate inertia effects—factors that keep a firm in its current neighborhood. The α coefficients model a firm's baseline propensity to stay in place (α_0) and the effect of firm characteristics that keep a firm from moving (α_1). β contains the inertia effects of a firm's current neighborhood that keep it in place (including interactions between firm-specific characteristics and neighborhood characteristics). γ denotes the pull effects of neighborhood characteristics that make firms choose to move *to* those neighborhoods. Inertia and pull effects are interpreted in the same way: positive values of α and β mean that the covariate makes a firm more likely to choose its current neighborhood, and positive values for γ mean that the firm is more likely to choose destination neighborhoods with that characteristic.[3]

Note that inertia effects are estimated for both firm-specific covariates and neighborhood-specific covariates, but pull effects are only estimated for neighborhood-specific covariates since firm-specific covariates do not vary within each choice set. Pull effects can incorporate firm characteristics through interactions with neighborhood characteristics, which is what γ_1 estimates.

I use maximum likelihood to estimate this model under the standard assumption that η_{rit} represents a random utility and its errors follow a type I extreme value distribution. I cluster standard errors

[3]For simplicity of notation, I use the same x_{it} and z_{rt} to define the inertia and pull effects, but in practice different sets of both firm-specific and neighborhood-specific covariates can appear in any of these effects, and the $x_{it}z_{rt}$ interactions need not include the full set of x_{it}'s and z_{rt}'s. In the analysis I show below, for example, I typically include only a couple $x_{it}z_{rt}$ interactions from a much larger set of x_{it}'s and z_{rt}'s.

within firms to account for the fact that firms appear more than once in the sample.[4]

Defining a Firm's Neighborhood Choices

Each firm has its own choice set of possible neighborhoods, C_{it}. I define that choice set to include all neighborhoods (all Census tracts) in the firm's metro area (CBSA). A potential difficulty is that large metro areas contain a very large number of Census tracts (e.g., New York has 4,701 different tracts), and firms are disproportionately concentrated in these very large metro areas. To estimate the model, it is necessary to construct a new dataset with one observation for each possible choice in the choice set for each firm–year in the data. In my sample, this would require a dataset with 4.4 trillion observations. (The average firm is located in a CBSA with more than 1,000 neighborhoods from which to choose.) It is not computationally possible to estimate this model on such large data.

Fortunately, one advantage of using the standard discrete-choice likelihood assumptions is that it makes it possible to take a subsample from each firm's set of neighborhood choices, and the estimates will still be consistent. The firm's actual destination choice must always be included in the sample, and the other neighborhoods can be sampled

[4]Note that the model developed by Steele et al. (2016) can be expanded to include many other components, including firm-level random effects that model unobserved heterogeneity between firms as well as the correlations between effects. This comes at great computational cost, however, and it is infeasible to estimate a random-effects model on a sample as big as mine. Clustering the standard errors within firms represents a compromise that should still yield reasonable inferences from the model.

probabilistically. Sampling probabilities need not be uniform for all neighborhoods. To account for sampling from the choice set, the linear predictor η_{rit} needs to be adjusted with an offset term $-\log(q_r it)$, where q_{rit} is simply the probability that choice r is included in the sample for firm i at time t. For statistical power, in addition to always including the firm's chosen location (which is $y_{i(t+1)}$), I always include the firm's current location (i.e., for the neighborhood where $\omega_{rit} = 1$, $q_{rit} = 1$).

Although this estimator remains consistent when the choices are sampled, it is still biased in finite samples. But the bias decreases with the square of the number of sampled choices, so it becomes very small quite quickly. In simulated data generated to be similar to my real data, I estimated the bias for different sizes of the sampled choice set. I found that once the number of sampled choices gets above 40 for each firm–year, the results are indistinguishable from using the full choice set.

To ensure that I was safely above this threshold, I sampled an average of 50 neighborhoods for each firm in each year to serve as possible destinations. The set of neighborhoods always contained both the firm's current neighborhood and its choice for the following year (if it moved). For firms in metro areas divided into fewer than 49 Census tracts, I used the full set of neighborhoods for each firm–year. For firms in metro areas with at least 49 tracts, I sampled each neighborhood with probability $q_{rit} = {}^{49}/_{n_{i(tracts)}}$, where $n_{i(tracts)}$ is the number of tracts in the firm's CBSA. For those firms, this produced an average of just over 50 neighborhoods in each choice set. This resulted in a dataset with N=177,780,645 observations, one for each firm–year–choice.[5]

Key Predictors and Controls

The key predictor in my analysis is the proportion of black residents in each neighborhood, which comes from Census data and is used to estimate both an inertia effect (how a firm's probability of remaining in its current location changes with the proportion of black residents in its neighborhood) and a pull effect (if it chooses to move, how its preference for destination neighborhoods changes based on their proportions of black residents).

A second important predictor is the proportion of black workers in the firm. This comes from the EEO-1 data on workforce composition. I use this to estimate an inertia effect (how a firm's likelihood of moving changes with the proportion of black workers it employs). I also interact it with neighborhood proportion black to see how the marginal effect of neighborhood proportion black differs for firms with different numbers of black workers. This lets me examine whether firms with more black workers have different preferences for staying in and moving to black neighborhoods than do firms with fewer black workers.

In an effort to isolate the effect on relocation choices of neighborhood racial composition, I include a number of controls:

[5]The true average choice-set size for each firm–year is 47.7, slightly below the targeted value of 50. This is because of metro areas with fewer than 49 tracts. But for those metro areas, there is no bias from sampling since firms in those places use their entire choice sets.

- *Neighborhood worker quality*: To control for the quality of the labor force in a neighborhood, I include the neighborhood's average household income, education distribution (proportion with college degree and proportion with less than a high-school degree, with those in the middle as the reference category), poverty rate, and proportion of residents who own the home they live in. The combination of these features also proxies land value in the neighborhood.

- *Job density*: As a proxy for unmeasured neighborhood features that make it appealing to businesses, I include a measure of the neighborhood's job density (the number of workers working in the Census tract), compiled from the geocoded EEO-1 data. We know that industrial agglomeration is an important predictor of firm location, as is the cost of land and an area's access to transportation (Arauzo-Carod et al. 2010). The number of other firms who have already chosen to locate in a neighborhood is a useful proxy for these unmeasured neighborhood characteristics. Job density is highly skewed and it is larger in absolute terms in larger metro areas, so I standardize this measure within each CBSA (since CBSA determines a firm's set of choices) and take the log of this value.

- *Population density*: Population density adjusts for the size of the local labor pool, and it is also a proxy for land value (since more densely settled areas have relatively scarce land). I take the log of this density. I also include an indicator for whether a tract has zero population. This accounts for the fact that the NCDB Census data is standardized to 2010 Census tracts, and many populated tracts in 2010 were completely undeveloped in 1970. Such tracts were unlikely to be appealing destinations for firms.

- *Distance from current location*: Firm moves decline with distance—firms are most likely to move to neighborhoods closer to their current neighborhoods. For each firm, I calculate the distance between the firm's precise current location and the center of each possible destination neighborhood. Since distance is zero for a firm's current location, I only estimate a pull effect for distance, not an inertia effect.

- *Proportion of blue-collar workers*: Based on the EEO-1 data on workforce composition, I construct a measure of the proportion of blue-collar workers in each firm.

- *Industry*: Firms in different industries have different propensities for moving, and I control for these inertia effects.

- *Firm size*: I control for inertia effects of the log of a firm's size (total number of workers).

- *Firm growth*: We might expect firms that are changing in size to have a different probability of moving in order to adjust for their changing status, so I estimate inertia effects for the change in the number of workers since the previous year.

- *Contractor status*: I control for whether the firm is a federal contractor, since we know that contractors respond more strongly to federal equal-opportunity regulations (Dobbin et al. 2015).

- *Branch status*: I include indicators for whether a firm is a single establishment or whether it is the headquarters of a multi-establishment organization (so the reference category is a firm that is a branch of a multi-establishment organization).

Results

Table 3.1 shows estimates for the key predictors of interest for three different model specifications. Each model includes the full set of controls, which can be found in Appendix D. Column (1) of Table 3.1 shows results from the primary model of interest. I set out to examine how firms' neighborhood preferences change as the proportion of black residents in a neighborhood increases.

Are firms more likely to move out of neighborhoods with more black residents? Yes. The inertia effect for the proportion of black residents in a neighborhood is negative (−0.258) and statistically significant. Once those firms decide to move, they are also less likely to choose a given destination neighborhood as the proportion of black residents in that neighborhood increases. The pull effect for the proportion of black residents in a neighborhood is also negative (−0.409) and statistically significant. Moreover, the magnitude of the pull effect is substantially larger than the magnitude of the inertia effect (and the difference is statistically significant at $p < 0.001$). This implies that a firm is less sensitive to the proportion of black residents in its own neighborhood when contemplating a move than it is to the proportion of black residents in potential destination neighborhoods, having already decided to move.

These models condition on all the firm and neighborhood characteristics described in the previous section. If firms were simply relocating because they did not like the "quality" of workers available in their current neighborhoods and were race-neutral, as Kain's (1968) formulation of the spatial-mismatch hypothesis assumes, then we would expect that controlling for a neighborhood's income levels, education levels, homeownership levels, and poverty levels would remove any detectable relationship between race and neighborhood choice. It does not, which provides evidence that firms are not race-neutral in their choice of whether to relocate or in their choice of neighborhoods.

Do these preferences differ in firms with a large number of black workers? Perhaps some firms have a taste for discrimination, which is reflected in the proportion of black workers they employ. If that is true, then we would expect firms with such tastes to have the strongest preferences *against* neighborhoods with many black residents. Table 3.1, Column (2) shows a model that includes interactions between the proportion of black residents in a neighborhood and the proportion of black workers in the firm. All the main effects are statistically significant, but our interest lies in the marginal effect of the proportion of black residents in the neighborhood: how does this effect differ for

Table 3.1: *Estimates of key predictors from discrete-choice model predicting a firm's preference for staying in its current neighborhood (inertia) and, if it chooses to move, its preferences for other neighborhoods (pull effects). N=177,780,645 choices over 3,728,710 firm–years for 523,117 unique firms. Each model contains a full set of control variables: see Appendix Tables D.1, D.2, and D.3.*

	(1)	(2)	(3)
Inertia Effects:			
% Black Residents (N'hood)	-0.258^{***}	-0.509^{***}	-0.507^{***}
	(0.022)	(0.028)	(0.033)
% Black Workers (Firm)	0.632^{***}	0.890^{***}	0.711^{***}
	(0.027)	(0.036)	(0.037)
Neighborhood % Black		1.196^{***}	1.208^{***}
× Firm % Black		(0.082)	(0.084)
% Blue-Collar Workers (Firm)			0.410^{***}
			(0.015)
Neighborhood % Black			0.059
× Firm % Blue-Collar			(0.053)
Pull Effects:			
% Black Residents (N'hood)	-0.409^{***}	-0.983^{***}	-1.183^{***}
	(0.022)	(0.029)	(0.033)
Neighborhood % Black		2.791^{***}	2.504^{***}
× Firm % Black		(0.082)	(0.085)
Neighborhood % Black			0.683^{***}
× Firm % Blue-Collar			(0.054)

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

firms with more black workers?

Figure 3.1 plots the marginal effects for both the inertia effect (top panel) and pull effect (bottom) of the proportion of black residents in a neighborhood.[6] Gray bands show 95% confidence intervals, so the

[6]For an overview of why plots of marginal effects are essential for interpreting the

effects are statistically significant wherever the bands do not touch zero (the dotted line). This shows that firms that employ more black workers are indeed different in their preferences for neighborhoods. The greater the proportion of black workers employed by a firm, the more favorably the firm views neighborhoods with high proportions of black residents and the less likely they are to move away from their own neighborhoods based on the proportion of black residents there. The average firm in my sample employs 12% black workers. For such a firm, there is still a strong negative preference for neighborhoods with many black residents. Thus, while firms differ in their preferences, most firms are still far from being race-neutral in their neighborhood preferences.

There is some cause for optimism, however. Figure 3.1 shows that in majority-black firms (those with more than 50% black workers), firms actually *prefer* to locate in neighborhoods with higher black populations. This provides evidence that the preference against locating in black neighborhoods is not universal among firms. About 5% of firms are majority black. But to get sizable positive preferences, the level of black employment in a firm must be even higher. About 3% of all firms have more than 60% black workers. These firms are distributed relatively evenly across large Northern and Southern cities with large black populations. (There are far fewer such firms in the West because there are few cities with very large black populations there.)

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interaction between two continuous variables, see Brambor et al. (2006).

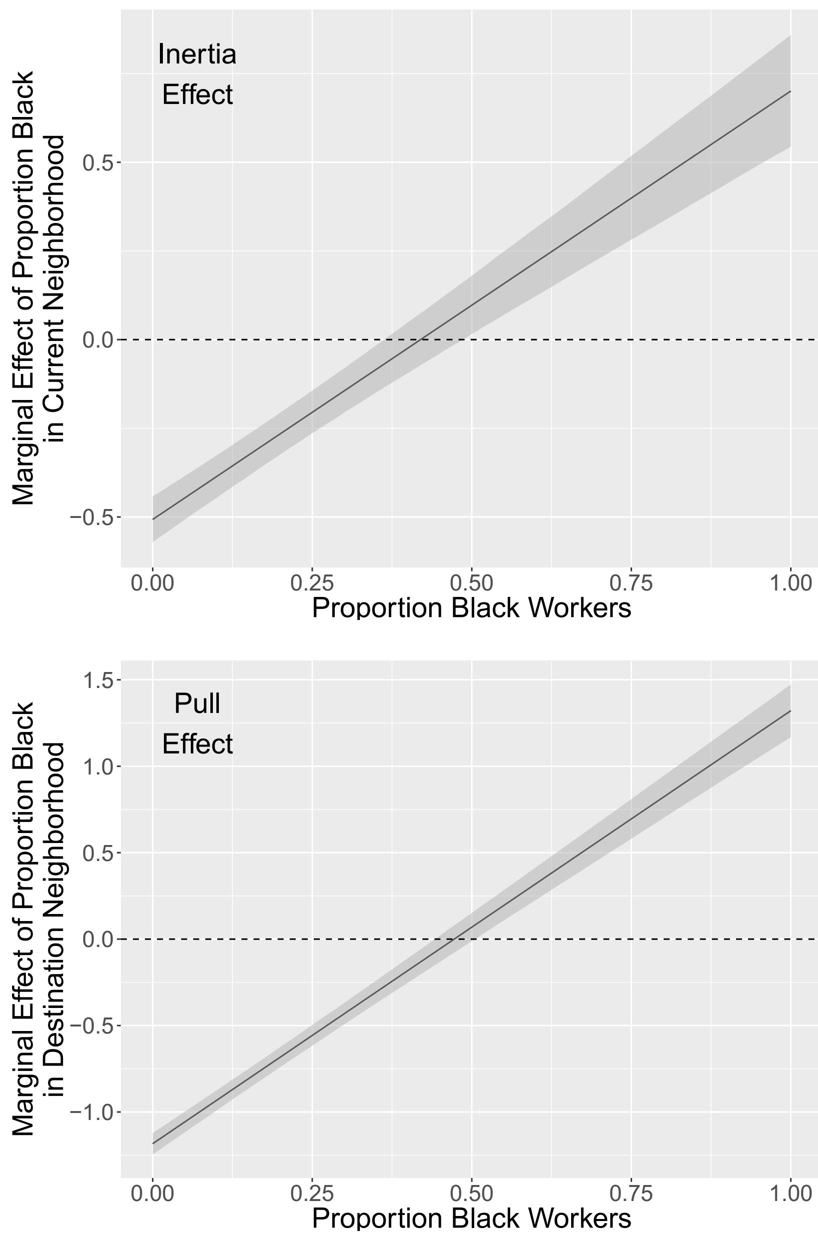

Figure 3.1: *Marginal effect of the proportion of black residents in a neighborhood as it varies across the proportion of black workers in the firm. Gray bands show 95% confidence intervals. The top panel shows estimates of the inertia effect, and the bottom shows estimates of the pull effect. Results from Table 3.1 Column (2).*

Alternative Specifications

If firm location preferences are really about the *kinds* of workers a firm needs to fill its ranks, then perhaps the preference against black neighborhoods is only visible for firms that employ highly skilled workers, since black workers have historically had much higher employment levels in blue-collar jobs. Table 3.1, Column (3) adds an interaction between the proportion of blue-collar workers employed by a firm and the proportion of black residents in a neighborhood. I do not plot the marginal effects for this model, but the results look almost identical to the model shown in Figure 3.1. The change in preferences for firms with more black workers is not simply a proxy for its need for blue-collar workers.

We might think that a firm's neighborhood preferences are concentrated in its leaders—the ones making decisions about where to move. Perhaps the declining preference against black neighborhoods in firms with more black workers reflects the fact that there are more black managers in positions of power, and they influence firms' relocation decisions. To test this, I split black workers into two categories, managers and not managers, and I included separate interactions with each group instead of the interaction between neighborhood percent black and all black workers. I find that non-managers are responsible for the bulk of the effects—the magnitude of their effects is larger, and there are far more black non-managers employed in the average firm than black managers, so their effects are amplified. This suggests that the preference differences across firms with different shares of black workers is not a story about managerial power. Instead, it is more consistent with the idea that firms that employ more black workers also have a lower taste for discrimination, and their taste is reflected in both their hiring and their location decisions.

I ran additional models that include state GDP growth and state unemployment rates as firm-level predictors of inertia. However, state-level unemployment data are unavailable before 1981, and state-level data on GDP growth is only available for some states in the 1970s, so including these covariates would mean limiting the period under study so it starts in 1981 instead of 1971. Including these covariates didn't alter the other coefficients, so for the sake of keeping the full time period in the data, I don't include them in the models reported here.

Conclusion

Do firms act in a race-neutral way when they choose locations? Or do they exhibit preferences that lead them away from neighborhoods with large proportions of black residents? Previous research has lacked the data needed to explore this question, despite its importance as a background assumption of the spatial-mismatch hypothesis. I find evidence that firms do take race into account when deciding whether to move and in choosing a destination neighborhood.

Kirschenman and Neckerman (1991) show how employers stereotype black workers, particularly those who live in central cities. Such attitudes likely play an important role in determining firms' neighborhood preferences, and it is possible that it is not neighborhoods with large proportions of black residents that firms are avoiding but rather particular kinds of such neighborhoods that are more easily stereotyped, such as inner cities. Future research should look more carefully at different kinds of predominantly black residential neighborhoods.

Related to this, the recent increase in the number of black, inner-ring suburbs highlights the dynamic nature of migration by both employers and residents within cities. Neighborhood preferences are likely to shift over time as employers form new attitudes towards different groups of urban and suburban residents. Future research on firms' neighborhood preferences might fruitfully analyze this as a time-varying process, and changes over time can be used as analytical leverage to better understand what drives the spatial redistribution of jobs.

Much like the impact of spatial mismatch itself, it is likely that employer behavior differs in different cities. Iceland and Harris (1998) have already provided limited evidence demonstrating how relocation intentions differ across cities. Future research should extend the model presented in this paper to estimate how the relationship between neighborhood racial composition and firm relocations varies across cities and what might explain that variation. Moreover, while the findings presented here relied on industry-specific effects on a firm's decision to move, future research should examine how industry interacts with neighborhood characteristics in shaping firm relocation decisions. Weterings (2014) has shown that some physical characteristics of neighborhoods predict the likelihood that firms move (in the Netherlands) and that this relationship differs across industries. Future research should extend the model in this paper to explore industry variation in the importance of race for firm relocations.

Finally, policy makers should take note of the evidence I have presented suggesting that employers are actively avoiding black neighborhoods. Policies aimed at merely increasing job access by making it easier for central-city residents to reach far-flung jobs (through transportation policy) or that try to move black residents closer to job-dense areas (through housing policy) may not have their hoped-for impact until employers change their minds about who counts as desirable workers.

Conclusion

In this dissertation, I explore how the distribution of *locations* of private U.S. employers contributes to racial inequality. This happens because a large proportion of jobs are located far from where African-American residents live. One exception to this—employers that often locate near African-American and Latino residential populations—are firms that emit toxic chemicals to the neighborhoods around them. Whether firms are far from African-American residents, creating disadvantages in the labor market, or close to them, creating health inequalities, the locations of firms matter for racial inequality. This dissertation aims to shed light on how this works.

Chapter 1 examines how spatial mismatch—the imbalance between where African-American workers live and where jobs are located—varies across cities. I show that spatial mismatch is much worse in some cities than in others, providing a potential explanation for the large body of mixed empirical evidence on spatial mismatch. Moreover, I show that many seemingly important features of cities fail to explain this variation, including city size and density, access to cars and public transit, and residential mobility. But segregation does play an important moderating role: cities with higher levels of segregation exhibit a greater degree of spatial mismatch. This shows that firms mediate inequality differently based on their macro-level community contexts.

Chapter 2 shifts to firms' micro-level community contexts: their neighborhoods. I look at how the behaviors of firms that release toxic chemicals respond to the neighborhoods around them. More broadly, I ask how the racial composition of a firm's managers interacts with the racial composition of its surrounding neighborhood to create differences in organizational behavior. I find that when African Americans or Latinos occupy managerial positions in a firm, *and* that firm is located in a predominantly African-American or Latino neighborhood, the firm engages in less dangerous polluting behavior. This is consistent with three different mechanisms for how managers respond to communities: (1) managers listen to direct community pressure from their own racial groups; (2) managers protect the interests of the communities they live in, regardless of the interests of the firms they work for; and (3) managers respond to issues that are salient along racial lines to their own racial group. These findings show how managers' actions are linked to the communities where firms are located. In the domain of pollution, I show that managers can play an important role in advancing (or not) the interests of predominantly African-American and Latino communities, whose residents have historically lacked the power to shape the behavior of firms around them.

Chapter 3 shifts from examining the effects of firm locations to examining what shapes firms' location decisions. I ask whether firms are race-neutral actors that choose their locations based on skills characteristics of local workers and features of the land and business environment in an area, as the spatial-mismatch hypothesis assumes, or whether firms take race into account when choosing their locations. I find that firms avoid African-American neighborhoods. I analyze firm relocation decisions and show that firms are more likely to relocate when their neighborhoods have more African-American residents. Moreover, when choosing a destination neighborhood for their relocations, firms prefer neighborhoods with fewer African-American residents. However, not all firms behave like this. Firms with a majority of African-American workers choose to stay in neighborhoods with more African-American residents, and when they do move, they gravitate towards neighborhoods with more African-American residents.

Together, this dissertation shows that lack of access to nearby jobs plays a significant role in shaping African-American employment disadvantage. But my findings suggest that race-neutral policies to improve job access may not be sufficient to reduce this disadvantage. The results of Chapter 1 show that residential mobility and access to public or private transportation—two common policy prescriptions—do not help to explain variation in the effects of spatial mismatch, so it provides no evidence that such policies would increase African-American employment. Instead, it shows that segregated cities exhibit greater spatial mismatch, and we do not yet understand how this works. It may be that segregated cities are also cities where firms engage in stronger labor-market discrimination against African Americans. Chapter 3 further supports this picture, suggesting that employers are not race-neutral in choosing their locations, and policies that aim to improve (geographic) access could simply bring African-American workers closer to firms that will not hire them anyway.

But this dissertation does point to a remedy: increasing racial diversity inside firms. Chapter 2 shows that hiring and promoting more African Americans and Latinos to managerial positions can have a spillover effect to the communities around the firms that do so: Putting African Americans and Latinos into positions of power increases the likelihood that firms act in the interests of African-American and Latino neighborhoods. Chapter 3 buttresses this conclusion, suggesting that firms with a high proportion of African-American workers do not act in a way that creates disadvantage for African-American residents and can even improve their opportunities. Policies that promote diversity in hiring and promotion within firms thus have a multiplier effect: they help not only the individuals who are directly hired and promoted, but they also improve job access and create a healthier environment for the African Americans and Latinos who live around firms with diverse workers.

Appendices

Appendix A Job Shares in Constant 1972 Boundaries

Table A.1: *Relative proportions of black (left) and all (right) male, blue-collar workers located in the central black residential cluster, the border of the cluster (0–5 miles), and the suburbs (>5 miles), using constant 1970 cluster boundaries for all years. Each row sums to one. Source: Geocoded EEO-1 data.*

	Black blue-collar workers			All blue-collar workers		
	Center	Border	Suburb	Center	Border	Suburb
1972	0.28	0.52	0.20	0.17	0.53	0.30
1980	0.24	0.53	0.23	0.14	0.51	0.35
1990	0.20	0.52	0.27	0.12	0.48	0.40
2000	0.18	0.50	0.32	0.10	0.44	0.46
2010	0.16	0.49	0.35	0.10	0.42	0.49

Table A.2: *Black share of male, blue-collar jobs in the central black residential cluster, the border of the cluster (0–5 miles), and the suburbs (>5 miles), using constant 1970 cluster boundaries for all years. For a baseline, we know from Table 1.2 that the black residential share in the center in 1970 is 0.50. Source: Geocoded EEO-1 data.*

	Black job share		
	Center	Border	Suburb
1972	0.27	0.16	0.10
1980	0.27	0.17	0.11
1990	0.29	0.19	0.12
2000	0.32	0.21	0.13
2010	0.31	0.21	0.13

Appendix B Functional Form of Distance

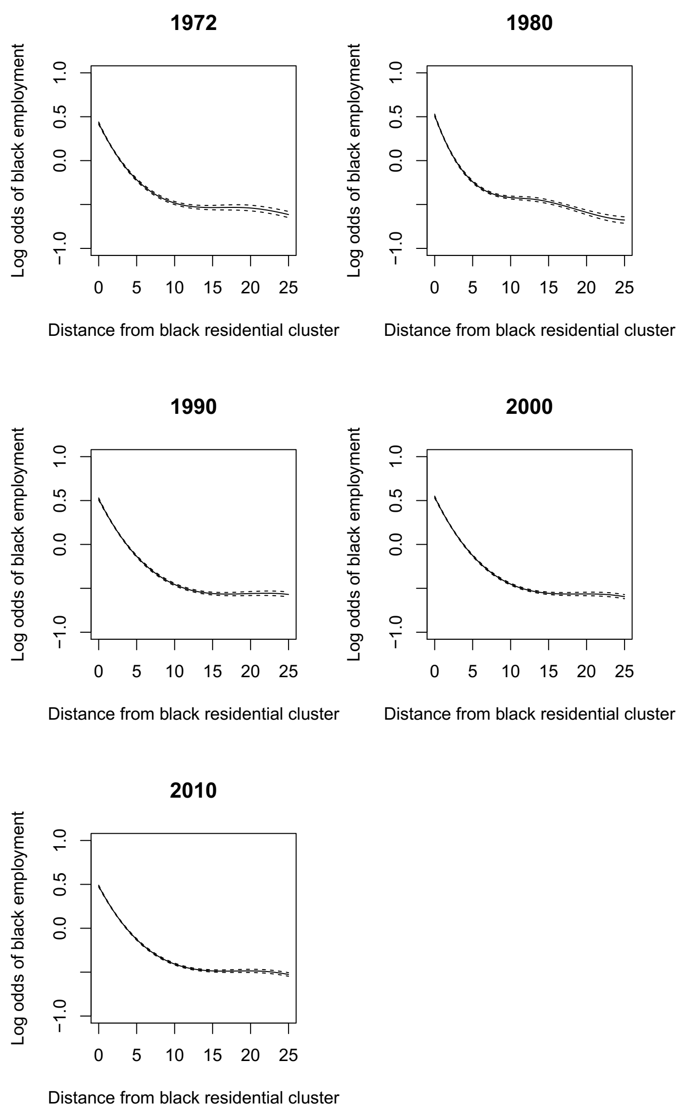

Figure B.1: *Smooth plot of the relationship between distance from central black residential cluster and black employment, using a generalized additive model with penalized spline smoothing.*

Appendix C Full Regression Results

Table C.1: *Full regression results. The model reported in Table 2.1 corresponds to column (2), which includes firm fixed effects. Great care should be exercised in interpreting any of the coefficients on the control variables. They represent partial associations, no effort has been made to ensure that they are properly identified, and they are likely to be meaningless. "Nonwhite" here is short for the proportion of African Americans and Latinos. N=173,833 (18,033 unique firms).*

	(1)	(2)
Nonwhite Managers	0.274^{*}	0.278^{**}
	(0.129)	(0.095)
Nonwhite Neighborhood	0.054	-0.396^{*}
	(0.089)	(0.171)
Managers × N'hood	-1.256^{***}	-0.822^{***}
	(0.192)	(0.128)
Women Managers	-4.015^{***}	-0.294^{*}
	(0.168)	(0.138)
Women Non-managers	-7.492^{***}	0.263
	(0.111)	(0.224)
Firm Size (log)	1.394^{***}	0.841^{***}
	(0.018)	(0.029)
Single Establishment	-0.303^{***}	-0.155^{*}
	(0.051)	(0.065)
Federal Contractor	0.217^{***}	−0.046
	(0.036)	(0.033)
GDP Growth (State)	-3.873^{***}	-1.368^{*}
	(1.006)	(0.615)
Unemployment (State)	0.060^{***}	−0.001
	(0.017)	(0.014)
Intercept	7.141^{***}	
	(1.073)	
Year Fixed Effects	Yes	Yes
Firm Fixed Effects	No	Yes

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

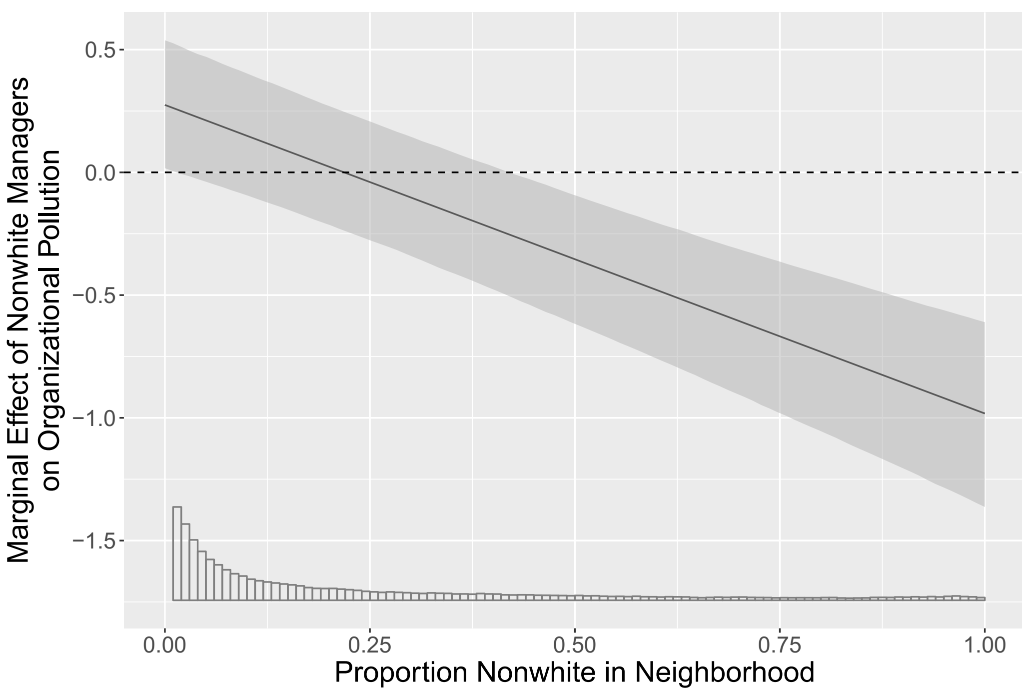

Figure C.1: *The key marginal effect* $\partial Y/\partial RM$ *as it varies across firms located in neighborhoods with different proportions of African-American and Latino residents, based on model without firm fixed effects in Table C.1, column (1). The magnitude of the marginal effect is much larger without the firm fixed effects and is statistically significant for firms in neighborhoods with at least 40% African-American and Latino residents.*

Appendix D Full Regression Results

Table D.1: *Part 1 of model results, showing inertia effects by industry. Agriculture is the reference category.*

	(1)	(2)	(3)
Inertia Effects (Industry):			
Mining	−0.093	−0.097*	−0.074
	(0.048)	(0.048)	(0.048)
Construction	0.045	0.040	0.005
	(0.038)	(0.038)	(0.038)
Manufacturing	0.433***	0.429***	0.406***
	(0.034)	(0.034)	(0.034)
Transit & Communications	0.251***	0.245***	0.239***
	(0.035)	(0.035)	(0.035)
Wholesale	0.248***	0.245***	0.283***
	(0.035)	(0.035)	(0.035)
Retail	0.932***	0.927***	0.946***
	(0.034)	(0.034)	(0.034)
FIRE	0.109**	0.102**	0.192***
	(0.034)	(0.034)	(0.034)
Services	0.061	0.055	0.084*
	(0.033)	(0.033)	(0.033)
Public	0.130	0.122	0.143
	(0.119)	(0.119)	(0.119)
Nonclassifiable	0.390***	0.384***	0.409***
	(0.037)	(0.037)	(0.037)

***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table D.2: *Part 2 of model results, showing the remaining inertia effects.*

	(1)	(2)	(3)
Inertia Effects (Continued):			
Baseline Inertia	2.394***	2.403***	2.176***
	(0.053)	(0.053)	(0.053)
Federal Contractor	−0.083***	−0.082***	−0.070***
	(0.008)	(0.008)	(0.008)
log(Firm Size)	0.104***	0.103***	0.096***
	(0.004)	(0.004)	(0.004)
Single Establishment	0.037**	0.037**	0.016
	(0.012)	(0.012)	(0.012)
Headquarters	−0.356***	−0.357***	−0.305***
	(0.010)	(0.010)	(0.010)
Growth in Firm Size	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)
log(Job Density)	0.592***	0.594***	0.616***
	(0.008)	(0.008)	(0.008)
log(Population Density)	0.169***	0.168***	0.175***
	(0.003)	(0.003)	(0.003)
Zero-Population N'hood	0.670***	0.669***	0.719***
	(0.077)	(0.077)	(0.077)
% College Grad (N'hood)	−0.014	−0.010	0.067*
	(0.034)	(0.034)	(0.034)
% < High School Degree (N'hood)	0.266***	0.252***	0.224***
	(0.036)	(0.036)	(0.036)
% Poverty (N'hood)	−0.640***	−0.622***	−0.626***
	(0.041)	(0.041)	(0.041)
Avg Household Income	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
% Owner-Occupied (N'hood)	−0.025	−0.028	−0.035
	(0.019)	(0.019)	(0.019)
% Hispanic Residents (N'hood)	0.118***	0.133***	0.112***
	(0.029)	(0.029)	(0.029)
% Black Workers (Firm)	0.632***	0.890***	0.711***
	(0.027)	(0.036)	(0.037)
% Black Residents (N'hood)	−0.258***	−0.509***	−0.507***
	(0.022)	(0.028)	(0.033)
Neighborhood % Black		1.196***	1.208***
× Firm % Black		(0.082)	(0.084)
% Blue-Collar Workers (Firm)			0.410***
			(0.015)
Neighborhood % Black			0.059
× Firm % Blue-Collar			(0.053)

***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

Table D.3: *Part 3 of model estimates, showing pull effects.*

	(1)	(2)	(3)
Push Effects (Continued):			
Distance from Current Location	-0.249^{***}	-0.248^{***}	-0.248^{***}
	(0.001)	(0.001)	(0.001)
log(Job Density)	1.240^{***}	1.244^{***}	1.244^{***}
	(0.006)	(0.006)	(0.006)
log(Population Density)	-0.396^{***}	-0.395^{***}	-0.396^{***}
	(0.002)	(0.002)	(0.002)
Zero-Population N'hood	-3.054^{***}	-3.041^{***}	-3.053^{***}
	(0.071)	(0.071)	(0.071)
% College Grad (N'hood)	0.035	0.053	0.033
	(0.028)	(0.028)	(0.028)
% < High School Degree (N'hood)	-0.827^{***}	-0.816^{***}	-0.823^{***}
	(0.025)	(0.025)	(0.025)
% Poverty (N'hood)	0.165^{***}	0.175^{***}	0.181^{***}
	(0.034)	(0.035)	(0.035)
Avg Household Income	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
% Owner-Occupied (N'hood)	-0.030	-0.048^{**}	-0.046^{**}
	(0.016)	(0.016)	(0.016)
% Hispanic Residents (N'hood)	-0.365^{***}	-0.347^{***}	-0.349^{***}
	(0.030)	(0.030)	(0.030)
% Black Residents (N'hood)	-0.409^{***}	-0.983^{***}	-1.183^{***}
	(0.022)	(0.029)	(0.033)
Neighborhood % Black		2.791^{***}	2.504^{***}
× Firm % Black		(0.082)	(0.085)
Neighborhood % Black			0.683^{***}
× Firm % Blue-Collar			(0.054)

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

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