Firm Relocation and Spatial Mismatch: Evidence from Natural Disasters

John-Paul Ferguson
Stanford University
jpferg@stanford.edu

Kaisa Snellman
INSEAD
kaisa.snellman@insead.edu

22 June 2016

Please do not cite or circulate without permission

Abstract

The spatial mismatch hypothesis is that large differences in black/white employment patterns could be produced by race-neutral employers, were there discrimination in the housing market combined with the suburbanization of work. Debate over the hypothesis hinges on employer intentions: do they use relocation as a tool to alter the racial makeup of their workforces? Prior work has foundered on the endogeneity of employer decisions. We exploit natural disasters to study relocations where employer discretion is curtailed. We develop models of employer behaviors consistent with different theories of employer intentions and hypothesize how these behaviors would differ in the wake of disasters. We test these hypotheses on longitudinal workforce-composition data from every large establishment in the United States between 1971 and 2014. Our preliminary results are more consistent with “race-based” decisions by employers than with “space-based” decisions. We discuss the implications of these findings for work on reducing employment segregation.

Introduction

Any researcher or practitioner who is interested in employment segregation must eventually confront the “pipeline problem.” Firm-level diversity policies are constrained by the employees and applicant pools that they have to work with. And the mismatch between where many firms are located and where nonwhite potential employees live means that such pipelines can be very racially unbalanced indeed. Half a century ago, Kain (1965) noted the steady movement of employers from central cities to the rising suburbs, and the apparent abandonment of those cities’ nonwhite populations, and posed the spatial mismatch hypothesis: racial discrimination in the housing market, combined with the suburbanization of work, could be largely responsible for the adverse labor-market outcomes faced by nonwhites.
Certain facts that might support the hypothesis are beyond dispute. Most employment growth in recent decades has taken place in the suburban rings of large cities. In 1950, central cities accounted for 75 percent of metropolitan areas’ jobs (Mills & Lubuele 1997); by 2000 they accounted for just 47 percent (Gobilon, Selod & Zenou 2007). Many urban neighborhoods did become disproportionately black, and disproportionately impoverished, as both firms and white workers moved to the suburbs (Wilson 1987, Wilson 1996). And nonwhite workers who live close to suburban jobs do tend to have better labor-market outcomes than those isolated in central cities (Ellwood 1986, Kasarda 1988). Yet many researchers and policymakers dispute the spatial mismatch hypothesis. The disagreement centers around whether whitening of the workplace is an unintended byproduct of other employer decisions or a goal in and of itself. Many researchers who were initially sympathetic to the idea of spatial mismatch (and, implicitly, the race-neutrality of employer’s relocation decisions) ultimately concluded that they could not explain differences in black/white employment patterns solely through skills, infrastructure, commuting patterns, or other individual or spatial characteristics: “The problem isn’t space, it’s race” (Leonard 1986, p. 20).

This disagreement is very much a going concern, and not just for labor economists. Most work on employment segregation takes the pipeline for granted and focuses on the firm’s treatment of workers in the pipeline. If employers factor in race when choosing where to locate, though—that is, if employers discriminate among applicant pools—then theories and policies that focus on organizational routines and ignore organizational location miss much of the action. Furthermore, spatial mismatch has not gone away; rather, it has transformed. Even as minorities have increasingly, if haltingly, relocated to the suburbs (Glæser, Hanushek & Quigley 2004, Bader & Warkentien 2006), white workers and employers have increasingly chosen to move back into central cities (Fischer 2016):

[B]etween 1990 and 2012 the percentage of residents living in the very center of the city who had bachelor’s degrees roughly doubled, while the percentage who were poor dropped. Ten to fifteen miles out, the presence of poor residents increased substantially. Low-wage downtown workers who were once close to their jobs have to drive much longer distances from their new suburban neighborhoods....Just as city centers are starting to become safer, nicer places, the poor can no longer afford them.

Said poor, it may go without saying, are disproportionately nonwhite. Spatial mismatch remains
with us, and we seem little closer to understanding or addressing the problem than we were decades ago.

Part of the lack of progress, at least in intellectual terms, has been empirical. We know that many workers self-select into living in cities or suburbs, and employers’ intentions in moving are usually opaque to researchers. Even in work that has exploited natural experiments that control for worker self-selection (Zax 1989, Fernandez 1994, Rosenbaum & Harris 2001, Katz, Kling & Liebman 2001, Clampet-Lundquist & Massey 2008), whether employers move for “space” or “race” almost always remains a mystery.

In this paper, we try to get clearer evidence of employer intentions toward relocation than was possible in prior work. To untangle the endogenous elements in employers’ location choices, we exploit a natural experiment. Specifically, we study natural disasters that force establishments to relocate, and compare those establishments to ones that relocate under normal conditions. Events like floods, hurricanes, earthquakes, and tornadoes sharply limit employers’ discretion about whether and when to move. We sketch out the behavioral implications of models that assume that employers move for reasons of space and for reasons of race, and theorize how a natural disaster would alter those behaviors. We develop a set of hypotheses that together give us some ability to differentiate between employer intentions.

We test our hypotheses using 43 years of data on workforce composition from every workplace with more than 100 employees in the United States. We construct workforce-composition histories of said establishments using the EEO-1 surveys that they have filed annually with the Equal Employment Opportunity Commission since 1971. These records contain unique identifiers that follow establishments when they move. We draw upon the Federal Emergency Management Agency’s records of major disasters to identify events that would cause spikes in firm relocation. We identify more than 12,000 establishment relocations over the period, of which more than 4,700 occurred in the wake of a natural disaster.

We examine differences across the “disaster-treatment” condition in how predictive the racial composition of an establishment is of relocation from the central city to the suburbs; in how much relocating firms prefer to move to places with whiter workforces; and in the speed with which relocated establishments’ workforces come to resemble the (typically whiter) population of their new locations. We find results that are more consistent with the idea that employers explicitly
consider race when moving than with the idea that employers only consider spatial factors that are at most correlated with race. These results are preliminary, and in any case they represent a central tendency rather than characterize the behavior of all employers. But they do suggest that employers pay more attention to race in their (re)location decisions than most theory and research on workplace diversity usually assumes. We discuss ways to advance this research and its implications for the study of organizational stratification more generally.

**Firm relocation and spatial mismatch**

In the decades after World War II, Americans moved from cities to suburbs in increasing numbers. Several factors drove this relocation. Fifteen years of depression and war had throttled housing construction in the cities. New Deal policy privileged suburban construction and home ownership, at least for whites (Jackson 1985). Many firms also relocated to the suburbs, for diverse reasons of their own: to be closer to their markets (Greenwood 1974), for example, or to build more efficient production facilities on greenfield sites (Field 2003). Black Americans were systematically excluded from the new suburbs (Katznelson 2005, Loewen 2005). The millions of black workers who had migrated north to find industrial work now found themselves residentially isolated in central cities where work was increasingly scarce (Sugrue 1996, Wilson 1996). Surveying rising rates of black joblessness as the economy boomed elsewhere in the early 1960s, Kain (1965) proposed the spatial mismatch hypothesis: racial differences in employment patterns could exist despite race-neutral employers, if there was relocation of work combined with discrimination in the housing market.

In the strongest version of the spatial mismatch hypothesis, employers do not discriminate on race. Firms move for orthogonal reasons, such as access to cheaper land or better infrastructure, and any changes to the firms’ workforces reflect resulting changes in the composition of the firms’ applicant pools (Kain 1968). A weaker version of the hypothesis is that firms relocate for reasons that are correlated with race, such as education or skills (Ihlanfeldt & Sjoquist 1998). The chief alternative hypothesis is that firms relocate with an eye toward changing their workforce, that is, to “run away” from black and other minority workers (Leonard 1986, Cowie 1999). The differences in these perspectives has been summarized as “Space, or race?” (Kain 1992). At issue is the central tendency in employers’ intentions regarding relocation.
For those interested in reducing employment segregation, this space-versus-race issue is important. It has policy implications for how we should try to integrate workplaces. If firm relocations are race-neutral, then we might focus on residential integration or transport policies, confident that workforce integration will then take care of itself. If relocations are race-based, then such policies may be necessary but insufficient; we must also target employer practices directly (Fernandez 2006). It also matters for how we theorize the demand- and supply-side drivers of segregation. Much work on stratification takes the composition of the applicant pool for granted, and presumes that the employer has little control over the pipeline (Petersen & Saporta 2004, Rubineau & Fernandez 2013, Fernandez-Mateo & Fernandez 2016). But if employers’ relocation decisions are often based on race then the pipeline itself reflects demand-side discrimination. Diversity policies that operate inside the firm and take the pipeline for granted (e.g., Bielby (2000); Reskin (2003); Kalev, Dobbin & Kelly (2006)) will be less effective if biased employers can alter the pipelines to which those policies apply. A better understanding of employer intentions is important for understanding the limits and possibilities of a wide range of proposed diversity initiatives.

Prior research on the spatial-mismatch hypothesis has made little progress on divining employer intentions. We will not review all of that work here; Jencks & Mayer (1990), Kain (1992), Ihlanfeldt & Sjoquist (1998), and Fernandez & Su (2004) give excellent summaries. As Fernandez (2006) notes, the two main empirical issues that have bedeviled this work are the self-selected migration of employees and the intentions of employers.

Most early work on the hypothesis studied cross-sectional variation in employment access for African Americans in suburbs and inner cities. The limitation of such research is that workers’ relocation decisions are nonrandom, so better employment outcomes for black suburbanites may simply reflect different skills, abilities, and employment histories, relative to black urbanites. Later work included more elaborate sets of controls (Ihlanfeldt & Young 1994), eventually moving to fixed-effects approaches (Mouw 2000) that examine within-neighborhood changes in employment access and their relationship with black unemployment rates. Some of the most sophisticated attempts to control for employee self-selection rely on natural experiments that move workers from the inner cities to the suburbs. The Moving to Opportunity studies, for example, have found mixed results of relocation on workers’ employment prospects (Katz, Kling & Liebman 2001, Rosenbaum & Harris 2001), though some of the ambiguity in those findings may reflect treatment non-compliance
Perhaps the strongest tests of the spatial mismatch hypotheses have been those that exploit specific firm relocations and can either rule out or control for turnover in the workforce. Zax's (1989) analysis of a firm relocation in Detroit and Fernandez's (1994) study of a plant relocation in Milwaukee both demonstrate that relocation disproportionately increases commute distances and times for blacks and other nonwhites, as well as residential moves and quits. Fernandez's (1994) study is particularly important because the employer studied was explicitly committed to retaining its workers. This meant that the employer's using relocation as a tool for shedding nonwhite workers could be ruled out, and so the negative impacts on black employment could be cleanly attributed to spatial mismatch. These studies constitute an existence proof of the hypothesis: negative employment outcomes for nonwhites can be produced by race-neutral employers.

The current state of research, then, is that spatial mismatch can exist and that it can be produced by employers who do not intend to whiten their workforces. Yet the research-design choices necessary to rule out worker self-selection, such as the explicit no-layoff guarantee that Fernandez (1994) exploited, make it impossible to use the same data to clear the hypothesis's second empirical obstacle: “While studies often find spatial effects, they are often unclear on the particular mechanisms producing racial disparities. Specifically, there is debate about the role that employers play in producing spatial mismatch” (Fernandez 2006). It is useful to know that spatial mismatch can be produced by race-neutral employers. But we do not know how often it is produced under such conditions, and how often it is produced by race-conscious employers. While we are more confident than ever about the existence of spatial mismatch, we are no closer to knowing the best way to attack the problem, because we do not understand the main generative mechanisms, particularly employer intentions behind relocating.

Identifying employer intentions

Prior work cleared the obstacle of worker self-selection by exploiting natural experiments involving worker relocation. To help clarify employer intentions, we take a parallel approach and exploit

---

1It is striking that the two best firm-relocation studies of this question involve opposite employer intentions. Fernandez (1994) recounts that the employer in his study was committed to retaining its largely minority workforce. Zax (1989) notes that “black employees filed suit” against the company he studied, “alleging discrimination against black employees in the general terms and conditions of employment, and discriminatory intent in the relocation” (p. 473; emphasis added).
natural experiments involving firm relocation. We go beyond the quantitative case-study approach used by Zax (1989) and Fernandez (1994) by exploiting an exogenous driver of firm relocation that appears frequently in nationally representative data: natural disasters.

Employer intentions are hard to uncover because the decisions of whether, when, and where to relocate are usually made simultaneously, and can all be seen as endogenous. By examining instances when disasters oblige firms to move, we exogenize the “whether” and “when” decisions. To a lesser extent, we can also compare where employers relocated, in the presence and absence of such disasters. Natural disasters have been used elsewhere as natural experiments; Baker & Bloom (2013) review the literature. Kirk (2009) used forced relocations in the wake of Hurricane Katrina to study recidivism among released criminals, with a logic similar to ours, namely that the disaster reduced the individuals’ choice about when and where to relocate. Our identification strategy is based on the assumption that some firms that move in the wake of a natural disaster would not have moved otherwise. We can compare the pattern of relocation decisions made by such firms to those made by firms in non-disaster years to explore whether there are significant differences in the patterns around voluntary and involuntary relocation.

To construct testable hypotheses in this context, we first have to describe the pattern of behaviors that different stylized theories of employer intentions would predict, then discuss how these behaviors might change in the wake of a natural disaster. There is no single test that can be used to differentiate between employer stances, but the pattern of results from several tests can be informative.

The spatial mismatch hypothesis comes in differing strengths. These hinge on the role that race plays in employers’ decisions to move. At one extreme, which we could call “space choice,” employers do not consider race at all. Instead their relocation decisions are based on factors like the cost of land, the quality of infrastructure, and the nearness to markets, which in theory are orthogonal to race. Thus the typical employer, when choosing to move, would not consider the race of its workforce; within a given locality, race would not be predictive of whether an establishment relocates. For similar reasons, race in a given location would not predict whether an employer would move to it. Net of other factors, the whiteness of a location would not be a particular draw. We would expect the establishment’s workforce to change after relocation, as the typical forces of turnover gradually cause the original urban employees to be replaced by workers from the new
location, but we have no a priori prediction of what the replacement rate will be.

At the other extreme, which we could call “race choice,” employers move because of race—their intention is to replace a non-white labor force with a white one. Thus the typical employer, when choosing to move, would consider the race of its workforce. Measures of racial composition, such as the share of black workers or (alternatively) the establishment’s dissimilarity to the larger labor market, would be predictive of whether an establishment relocates. For similar reasons, race in a given location would be predictive of whether an employer would relocate to it. Net of other factors, the whiteness of a location would be a particular draw. As in the space-choice model, we would expect the establishment’s workforce to change over time to come to resemble the new location, but we would also expect that adjustment process to happen faster. This is because such employers would make no special effort, either in their choice of the new site or their policies after the move, to ease the transition for their urban workforce.

At the level of theory, these different views of the spatial mismatch hypothesis yield crisp predictions. At the level of empirics, they are confounded. The problem is that we have no counterfactual. Employers might factor in the whiteness of a locality when considering a move, net of all other factors. Unless we can control for all other factors, though, we cannot take an observed correlation between whiteness and relocation as evidence that employers factor in race. That correlation may be spurious. Similarly, we only have one observed rate of workforce adjustment to the local labor market. Is this observed rate higher than what we might expect, as the race-choice model would predict? We lack a baseline against which to compare it. Testing between these theories using observational data on relocations is not possible. This is the obstacle that prior work has faced.

The endogeneity of the employer’s relocation decision is what poses the obstacle. Because employers are typically observed moving when they have already decided that conditions are right and when they have found a satisfactory site, these considerations are collapsed together. To gain any empirical traction, we need a baseline to which we can compare these observed relocations. This is where we leverage the impact of natural disasters. When there is a disaster, employers have less control over whether, when, and where they should relocate. Furthermore, we can imagine

---

2We can imagine, and explore, different measurement strategies based on whether employers try to shed nonwhite workers, or try to avoid taking nonwhite workers on in the first place.
employers responding to a disaster differently under the space-choice and race-choice models.

Space-choice employers who are struck by disasters would have a higher hazard of relocation, but there is no reason why the race of their workforce would suddenly predict whether they move. For race-choice employers, the picture is more complicated. It is possible that disasters force relocation by race-choice employers who were otherwise happy with their situation, i.e., who had workforces with a racial composition of which they approved. In this case, the added “noise” of such relocations would reduce the correlation between workforce composition and the probability of relocation. Alternatively, if the typical employer would prefer to have a whiter workforce but has felt constrained in relocation because of things like sunk investments in plant and facilities, then the sudden depreciation caused by a natural disaster could spur them to make the jump. In this case, disasters would increase the correlation between workforce composition and the probability of relocation. Taken together, this yields two hypotheses, support for which would be evidence for the race-choice model, and the null for which would be evidence for the space-choice model:

**Hypothesis 1a** Establishments with more black workers will be less likely to relocate after disasters than at other times.

**Hypothesis 1b** Establishments with more black workers will be more likely to relocate after disasters than at other times.

Note that these hypotheses are framed in such a way as to suggest an interaction effect—they are about moderating a main effect. This befits the research design, where we use an exogenous shock to compare differences in pre-existing social relationships.

We can apply similar reasoning to the choice of destination. Space-choice employers do not take into account the whiteness of possible destinations, net of other factors. But because race is correlated with so many other things in American society, such as educational attainment and infrastructure investments, there could be spurious correlation between race and destination choices. In a disaster situation, we would expect employers to be constrained in their choice set, such that they may not be able to pick the ideal site to which to relocate. If race is correlated with site features that employers prefer, then constraint on their choice of sites will weaken correlation between their choices and race. By contrast, race-choice employers would still try to avoid less racially desirable locations, and might even use the disaster as an excuse to move to whiter areas.
**Hypothesis 2a** The correlation between the white share of a destination and the probability of a firm’s relocating to it will be weaker after disasters than at other times.

**Hypothesis 2b** The correlation between the white share of a destination and the probability of a firm’s relocating to it will be stronger after disasters than at other times.

Because disasters affect whether and when employers have to move but less where they must move to, employers may use the same decision-making process regarding destination in the wake of disasters as they would at other times. Thus while disasters increase the relocation rate, they will not necessarily alter the destination patterns. There is a compelling null for hypotheses 2a and 2b, in other words, but failing to reject the null hypothesis here gives no support for one model or the other.

Finally, we can consider how the establishment’s workforce would adjust to such a relocation. Space-choice employers who relocate have no particular commitment to keeping or shedding their existing workforce. Employers who do not intend to move, though, probably value at least some of their current workers. Having been forced to move by a natural disaster, such employers would make some effort to retain current workers post-relocation, such as commuting assistance, more flexible hours, or even short-term credit to help with moves. This would imply a slower adjustment rate of the establishment’s workforce composition to local conditions.

**Hypothesis 3** Employment composition will adjust more slowly to local conditions after disasters than at other times.

By contrast, race-choice employers who move actively want to shed their existing workforce. We certainly would not expect such employers to make efforts to retain their workforces after disasters. Indeed they might be happy with faster turnover. However, the circumstances surrounding disaster relocations make it harder for workers to follow their jobs regardless of employer intentions. By itself, the vicissitudes of life after a natural disaster could increase worker turnover and thus increase the adjustment rate. Thus while a slower adjustment rate would tend to support the space-choice model, we would not take the null hypotheses (or indeed faster adjustment rates) to constitute support for the race-choice model. Asymmetric conclusions like this one are why we have proposed multiple tests to differentiate between such theoretical models.

We use these hypotheses, taken together, to try to characterize the behavioral patterns we would observe for different types of employer intentions. We hasten to add that empirical support
for, say, the race-choice model does not imply that all employers hinge their relocation decisions on their desire to avoid nonwhite workers. In practice, employers exist all along the spectrum between space-choice and race-choice that we have delineated, and outcomes we observe here reflect the aggregation of many such different people’s decisions. Our goal here is to examine which types of decisions are more common. Given the robust debate about how important spatial mismatch, rather than covert or overt bias, is in producing the starkly different employment outcomes of white and nonwhite Americans, such examination seems warranted.

Method

Our empirical strategy is to study firm relocations from urban cores to suburbia over several decades, and to compare patterns in those relocations when they were and were not preceded by natural disasters. We focus on urban-to-suburban relocations because these were the original fuel for the spatial mismatch hypothesis. While natural disasters increase the likelihood of relocation between suburbs, or even from suburbs to cities, we do not have theoretical priors about such moves.

We study establishments in the fifty largest core-based statistical areas (CBSAs) of the United States. These collectively contain 58 percent of all establishments in our data, and more than half of the American population. As defined by the Office of Management and Budget, a CBSA consists of one or more counties anchored by an urban center of at least 10,000 people, plus adjacent counties that are socioeconomically tied to the urban center by commuting (United States Census Bureau 2010). The National Center for Health Statistics in turn classifies U.S. counties via a six-level urban-rural scheme. We treat the NCHS’s level-1 or “large central metro” counties as urban cores for our analysis, and the level-2 or “large fringe metro” counties as suburbia. Figure 1 shows these counties.

[Figure 1 about here.]

This classification scheme is imperfect. The county designation is ideal for cities like New York, Boston, or San Francisco, where the city and county are coterminous. It works less well for cities like Miami or Las Vegas, whose counties contain other municipalities that might be the target of firm relocations. By requiring an establishment to cross a county line to count as a relocation, we
understate the amount of relocation in our data. We are cleaning the town and city information for the establishments in our data and plan to reproduce our analyses using a more fine-grained geographical coding. In the interim, we think that studying the county level is a conservative but still useful approach.

Data sources

To track establishments over time, we use data from the EEO-1 surveys compiled by the Equal Employment Opportunity Commission. To monitor compliance with Title VII of the Civil Rights Act, the EEOC requires all establishments with more than 100 employees\(^3\) to annually complete an EEO-1 form. The key data on the form is a matrix wherein the firm lists counts of employees of different races, broken out across nine occupational categories. Robinson, Taylor, Tomaskovic-Devey, Zimmer & Irvine Jr. (2005) discuss the mechanics of the EEO-1 survey in detail. The EEOC assigns each establishment a unique identifier that, crucially, follows the establishment if and when it moves. Together these forms constitute longitudinal data on the workforce composition of every large workplace in the United States, beginning in the early 1970s. We obtained access to the EEO-1 survey forms through an Intergovernmental Personnel Act agreement.

To code natural disasters, we rely on declarations by the Federal Emergency Management Agency. FEMA’s National Emergency Management Information System contains basic information on all disaster declarations since the late 1950s. In the wake of any natural catastrophe, being declared a disaster area is the first step for any state or local government to access federal funds and other assistance in reconstruction. Declarations are made at the county level.

Care is needed when using these FEMA data. State and local governments vary in how quick they are to take advantage of FEMA assistance, and the declarations can sometimes tell us more about how funds were spent than about how a disaster damaged an area. In the wake of Hurricane Katrina, for example, most U.S. counties were declared disaster areas, to ease dispensing money for evacuees. For similar reasons, every county in a coastal state might be declared a disaster area in the wake of “coastal flooding.” Finally, FEMA can recognize non-natural disasters, such as the 1992 Los Angeles riots, that are theoretically endogenous to processes studied here. Fortunately,

\(^3\)An executive order requires firms with more than 50 employees to also complete EEO-1 surveys, if they have more than $50,000 in federal contract work. We focus on the 100-employee threshold here.
FEMA also usually includes a short text description of the disaster in question, which helps in determining the actual counties damaged.

We hand-cleaned FEMA’s list of county-level declarations. We removed false-positive counties like those described above, where disasters were declared to help dispense funds for evacuees. We removed all wildfires from the data. By definition, most of these happen outside city centers, so removing them had very little effect on our count. We also removed disasters like blizzards, which often require federal funds to deal with but rarely if ever force firms to move. We focused on disasters caused by floods, hurricanes,\(^4\) tornadoes, and earthquakes. The raw FEMA file has 39,835 disaster declarations since 1971. After cleaning, we are left with 13,830 declarations, of which 2,508 apply to a county within one of the 50 largest CBSAs.

Such cleaning does not address false-negative counties, those that were hit by a natural disaster but where FEMA made no declaration. In most cases this is an issue of political jurisdiction. In figure 2, which codes counties by their number of disaster declarations over the time period, some boundaries like the state of Georgia or the northern border of Louisiana are clearly discernible. To try to ensure that we do not classify too many county-year observations as not having a disaster when they did, we changed the flag for a disaster from zero to one if four of a county’s neighbors reported a disaster, and at least one of those neighbors was in the same state.\(^5\) This recoding alters 92 county-year observations and does not significantly effect our main results.

![Figure 2 about here.]

The resulting dataset consists of just over 2.5 million firm-year observations. We observe just over 400,000 establishments in the 50 largest CBSAs between 1971 and 2014. Establishment relocation is a very rare event, since most workplaces never move. Roughly one half of one percent of our firm-year observations involves a move. The sheer size of the underlying dataset though means that we observe 12,106 establishment relocations, 4,749 of which happen in the wake of disasters.

For disasters to be a useful exogenous shock, they must exogenously increase the rate of establishment relocations. We calculated the conditional probabilities that an establishment would relocate in the next year based on there being a disaster in the current year, one year ago, two

\(^4\)before 1992, FEMA classified hurricanes as floods
\(^5\)Requiring four neighbors prevents this alteration from simply “marching inland” from the coasts; requiring one county to be in the same state prevents similar naïve spillover across state lines.
years ago and so on. Figure 3 plots logistic regression coefficients, which summarize the same information. A disaster in the current year increases the chances of firm relocation in the next year by about 36 percent. There is a smaller positive effect (about 27 percent) for relocations two years after the disaster; the coefficient then turns negative before returning to baseline. As a placebo test, we also estimated whether the occurrence of a disaster next year is correlated with relocation in this year. This is important to check, because it could be that disasters happen to occur at times and places where relocation rates were already rising, in which case we would doubt the technical exogeneity of the treatment. Figure 3 shows this is not the case. There is no upward trend in relocations before disasters strike.

[Figure 3 about here.]

Models and variables

In the ideal experimental design, every firm in a county struck by a disaster would relocate, and they would do so because of the disaster. This would let us cleanly delineate treatment and control groups for comparison. Instead, our data contain considerable treatment non-compliance. Many firms observed relocating after a disaster might have already planned to relocate. And some firms that are affected by a disaster choose to remain in place and rebuild. While assignment to treatment is ignorable due to the quasi-random distribution of natural disasters, non-compliance means that the receipt of treatment is not ignorable (Angrist, Imbens & Rubin 1996). Simply computing differences in variables like relocation and worker-turnover rates between establishments exposed and not exposed to disasters would yield biased and inefficient coefficients of causal effects (Kirk 2009). Furthermore, although whether a firm in a given location is hit by a disaster in a given year can be thought of as random, whether a given location is hit by a disaster, or a disaster lands in a given year, is not itself random. Coastal cities are more prone to hurricanes, for example, and climate change has increased the frequency of severe weather over time (Rosenzweig, Iglesias, Yang, Epstein & Chivian 2001). Furthermore, industries cluster non-randomly, and differ markedly in their racial composition (Tomaskovic-Devey, Zimmer, Stainback, Robinson, Taylor & McTague 2006). To deal with these concerns, we avoid a straight differences-in-differences approach (Card & Krueger 1994, Bertrand, Duflo & Mullainathan 2004) in favor of an instrumental-variables
approach. The key assumption of an IV approach is that the instrument is not directly correlated with the outcome variable except through its effects on the input variable. This is a reasonable assumption in the case of quasi-random extreme weather events, provided that we account for the different exposure probabilities of different establishments given their city, industry, and year. Thus we incorporate fixed effects for year, two-digit SIC code, and CBSA into our analyses. Because social processes like workforce turnover, commuting, and indeed employer intentions can also vary on these dimensions, we interact the disaster treatment with the fixed effects as well as with the variables of interest, to purge the former associations.

To test hypotheses 1a and 1b, which ask whether disasters affect the relationship between workforce composition and firm relocation, we estimate discrete-time, single-failure event-history models. The risk set comprises all firms from their appearance in the dataset. We begin observing firms when they pass the 100-employee threshold and fall under the EEOC’s reporting requirement. Thus these observations are left-censored. However, we have no reason to believe that firm growth rates are correlated with the racial composition of the workforce in a way that would also be correlated with relocation. Formally, we define the discrete-time hazard of relocation for establishment \( i \) as

\[
R_{it} = \Pr[T_i = t | T_i \geq t, X_{it}, \Psi],
\]

where \( T \) indexes the time the event occurred, \( X_{it} \) is a vector of time-varying establishment controls, and \( \Psi \) is an optional vector of fixed effects. We specify the hazard rate as a logistic function of time and the explanatory variables:

\[
\ln \left[ \frac{R_{it}}{1 - R_{it}} \right] = \alpha_t + \beta X_{it},
\]

which can be estimated using a traditional logit that includes a count for each establishment-year observation of the number of years it has been at risk of relocation (Allison 1982, Beck, Katz & Tucker 1998). In these models, the key independent variables are the black share of the workforce,
which is calculated based on the establishment’s EEO-1 surveys, and a flag for whether a disaster occurred in the establishment’s county in the prior year, which is constructed from the FEMA declarations described above. We use the main effects and the interaction of these two terms to test the hypotheses. As discussed above, we also present comparable results from fixed-effects models.

To test hypotheses 2a and 2b, which relate to how predictive the racial composition of a suburban community is of an establishment’s relocating there, we ultimately plan to estimate conditional logit models of location choice (Dahl & Sorenson 2010a, Dahl & Sorenson 2010b). Such models require considerable fine-grained controls for all destination counties, for which we are still gathering data. In this draft, therefore, we present descriptive comparisons of the racial distribution of establishments’ destinations across the treatment condition. Such descriptives are only suggestive but are a useful first step, at least insofar as they agree or conflict with the other statistical tests presented.

To test hypothesis 3, which is concerned with the speed with which establishments’ workforces come to resemble their new surroundings after a relocation, we fit linear partial-adjustment models (Tuma & Hannan 1984). The intuition here is that the racial composition of an establishment’s workforce has a long-run steady state that it will tend toward, that relocation to a demographically different location will change that long-run steady state, and that the organization will adjust to the new steady state at some rate \( \rho \). Partial-adjustment models have previously been used in employment-segregation research to study gender integration (Baron, Mittman & Newman 1991). Here, the adjustment rate has the empirical interpretation of summarizing the speed with which turnover causes a formerly urban establishment’s workforce to come to resemble that of its new suburban neighbors. More formally, we measure changes in the white share of an establishment’s workforce during the five years after a relocation,\(^6\) and compare the adjustment rate for establishments moving after disasters to that for other establishments. We assume that the adjustment rate is a function of the distance of the establishment’s composition at time \( t \) from its steady state:

\(^6\)We have annual measurements of workforce composition, and so need to measure several years to estimate adjustment trends. As time since the disaster increases though it becomes less plausible to attribute differences in trends to the disaster itself. We have chosen five years here, but using three- to seven-year windows produces substantively similar results.
\[
\frac{dW_t}{dt} = \rho(W_t^* - W_t).
\] (4)

We set the establishment’s steady state to be a function of the *white share of the county’s workforce*, which we calculate from the U.S. Census Bureau’s annual intercensal estimates; *establishment age* and *establishment size* in year \( t \), which we calculate from the EEO-1 surveys; and fixed effects for the county, year, and industry of the establishment.

\[
W_t^* = \beta_0 + \beta \cdot (\text{countyWhite}_t, \text{estAge}_t, \text{estSize}_t) + \kappa_c + \tau_t + \iota_t + u_t.
\] (5)

Substituting 5 into 4:

\[
\frac{dW_t}{dt} = \rho(\beta_0 + \beta X_t + \Psi + u_t - W_t),
\] (6)

where \( X_t \) represents the time-varying components of \( W_t^* \) and \( \Psi \) represents the vectors of fixed effects. Equation 6 cannot be directly estimated; it must be integrated to yield a model whose terms are directly observable. We follow Haveman (1993) in assuming that changes in \( X_t \) are linear in time, which case 6 can be integrated as follows:

\[
W_t = \alpha W_{t-1} + \beta_1 X_{t-1} + \beta_2 \Delta X_{t-1} + \Psi + \nu_t,
\] (7)

And most of the parameters from 6 can be recovered. In particular, \( \rho = -\ln(\alpha)/\Delta t \).

**Results**

Table 1 presents discrete-time Cox proportional-hazard models predicting firm relocation. Model 1 demonstrates that firms with a greater share of black workers are more likely to relocate from central cities to suburbia. This coefficient must be taken with a grain of salt, though, as these first estimates do not include fixed effects. Model 2 includes the effect of a disaster in the previous year which, in line with figure 3, increases the hazard of relocation. Model 3 presents the interaction of these two terms, which is positive and significant. Firms with more black workers are more likely to flee the central cities after disasters than they are in other years. This supports hypothesis 1b.
Table 2 introduces fixed effects for year, industry, and city, as well as their interactions with the disaster flag, singly and then pairwise. Models 4, 5, and 8 demonstrate that including such fixed effects for year or industry, singly or together, have little effect on the estimated effects of workforce composition on relocation, in disaster years or otherwise. By comparison, models 6, 7, and 9 demonstrate that including fixed effects for CBSA is sufficient to flip the sign of the coefficient for black share of the workforce. The most obvious reason for this is because the other models pool observations across cities, which vary both in their racial composition and in their baseline establishment-relocation rates. Overall, firms leave “blacker” cities at higher rates, but within cities, more black workers are associated with lower hazards of relocation. This points to why multiple measures of workforce composition should ultimately be analyzed. If we theorize, per the race-choice model, that employers move to avoid nonwhite workers, then the presence of a blacker workforce could reflect employers who have accommodated themselves to integration rather than moving. Using something like an index of dissimilarity between the establishment and its home labor market (Massey & Denton 1989, Ferguson 2015), which would increase for whiter workplaces in blacker cities, may yield opposite main effects. Yet this discussion also underlines why it is important to consider the interaction of such variables with the disaster treatment, as we had formulated in our hypotheses. Whatever the baseline, the relevant research question is how employer activities differ in the wake of such disasters. And on this score, models 6, 7, and 9 look like the others: the black share of the workforce is more predictive of establishment relocation after natural disasters. Persistence of this result in the fixed-effects specifications provides further support for hypothesis 1b.

Disasters make establishments more likely to move, and this effect is greater for establishments with more black workers. We can next ask, where do these establishments go? The best initial baseline would be to assume that establishments move randomly, and that the probability that an urban firm relocates to a particular suburban county is proportional to the number of establishments already in that county. This would yield a counterfactual distribution of relocated establishments
across suburban counties, sorted by the white share of the county population. Figure 4 displays a kernel density of this distribution, labeled “Random.” That figure also plots kernel densities for the observed relocations, broken out by the treatment condition. There are few to no differences between relocations in the non-disaster years, relative to the random baseline. Given that the pre-existing number of establishments in each county is itself an outcome of prior employer decisions, this is unsurprising. What is immediately apparent, though, is that relocations in the wake of disasters are far more likely to move to whiter counties.

We stress that this analysis is only descriptive. The most obvious question is whether counties with whiter populations are also counties with more slack real estate and capacity to absorb firms and workers on short notice. Such variation would be correlated with the treatment, and thus figure 4 may reflect non-trivial omitted-variable bias. As discussed above, we are gathering data on city and county characteristics that should help us control for such (as yet) unobserved differences. Nonetheless it is worth noting that the basic pattern here strongly supports hypothesis 2b, and that the goal of future analyses will be to try to account for as much of this disproportionate targeting of whiter communities after disasters as possible.

Finally, we can examine how quickly relocated establishments’ workforces turn over and come to resemble the labor market of the establishments’ new locations. Table 3 presents discrete-time linear partial-adjustment models, with the data separated by exposure to disaster. From the estimated coefficients for the white share of the establishment’s workforce in the prior period, we can calculate $\rho$, the establishment’s adjustment rate. Adjustment is relatively slow in both conditions. Recall that the EEO-1 surveys track counts of workers by race, not individual workers. Thus replacement of one white worker by another will not alter the share of white workers and will not factor into any adjustment. Only replacement of a worker of one race by one of another race (or differential growth rates of the respective groups) will factor in. That said, this is not necessarily a weakness of the design, since changes in such racial employment patterns is what the spatial mismatch hypothesis theorizes about.
Since the coefficients on share white are both close to 1 and since the discrete-time interval is one year, \( \rho = -\ln(\alpha)/\Delta t \approx 1 - \alpha \). Table 3 reports these adjustment rates. Slow though they may be in absolute terms, it is the difference across the treatment condition that is important. The adjustment rate in the disaster condition is almost 30 percent higher than in the no-disaster condition. The difference between the two adjustment rates is marginally significant \((p = .09)\). At best, workforce adjustment to relocation happens faster in the wake of disasters than in other years; more practically, there is no significant difference. We find no support for hypothesis 3.

**Discussion and conclusion**

The pattern of results that we find here is more consistent with a race-choice model of employer intentions than with a space-choice one. We find support for hypothesis 1b and tentative support for hypothesis 2b, both of which are consistent with the idea that employers take race separately into account when making relocation decisions. We find no support for hypotheses 1a, 2a, or 3, which would be more consistent with employers’ not factoring in race this way. When comparing large establishments that relocate in the wake of natural disasters to others, we find that employers with blacker workforces are more likely to move, that they move to whiter areas, and that if anything their workforces become whiter at faster rates than in more normal times.

Because this is an early draft of this study and because we have additional analyses to conduct, we do not presume throughout this discussion that these results are definitive. Instead we will discuss some limitations on these findings and our planned next steps; then we will discuss some implications for stratification research and future research directions if this pattern of results holds up to those next steps.

Beyond fixed effects, we have relatively thin controls in the models presented here. This is not a problem in and of itself. Controls are needed for causal inference when random assignment is not possible. Natural disasters are quasi-random, so we should only be concerned with omitted variables that correlate with receipt of the treatment. The main such issue with our current study design is that it assumes that all establishments in a county are equally likely to be affected by a natural disaster that affects that county. Partly this is a risk of conflating cities with their counties, but more fundamentally the racial distribution of residences and workplaces is correlated
with susceptibility to natural disasters (Cutter, Boruff & Shirley 2003). If blacker workplaces are located in more vulnerable areas then the results we have for hypotheses 1a and 1b could reflect differential treatment rather than differential response. In recent years, particularly in the wake of Hurricane Katrina and its starkly different impact on black and white residents of New Orleans, researchers have been trying to develop quantitative measures of social vulnerability to natural disasters (e.g., Flanagan, Gregory, Hallisey, Heitgerd & Lewis (2011)). The challenge for the present work is extending these measures’ underlying data series far enough back in time to be able to compute comparable metrics for as much of our time series as possible.

The converse of variation in vulnerability to disasters is variation in ability to absorb and shelter those affected by disasters. Our descriptive data around hypotheses 2a and 2b suggests that establishments that relocate after disasters go to much whiter locations. If the whiter areas within suburbia have more available land, excess building capacity, or better transport infrastructure, all of which would allow easy relocation on short notice, we would observe such a pattern of post-disaster relocations even if employers were race-neutral. While specific data series on, for example, excess office space are not available, many other factors correlated with destination areas, such as median income, population density, and miles of paved road are available. Our future plans for testing hypotheses 2a and 2b involve multivariate regressions with such controls.

We want to be as generous to alternative explanations as possible. We note these limitations and our plans to address them because, in the absence of such controls, an alternative story that matches our results is possible: employers do indeed make space-based choices, but the differential treatment of a disaster (based on the vulnerability of locations where blacker establishments are based) combined with the variation in the ability of sites to absorb relocated firms (on dimensions that are correlated with race) produce these findings. In other words, absent these controls, the hypotheses we test here do not definitively distinguish between the space-choice and race-choice models. We have no favorite between these alternative explanations, and the contribution of this study depends on the strength of the tests, not on the specific results. Thus for now we emphasize that we are continuing to gather data to strengthen these tests, and discuss what the implications are for policy and research on organizational stratification if the present results hold up.

Our dominant theories of employment stratification presume that most discrimination is due not to the explicit actions of consciously biased employers but rather to the exercise of unconscious
bias, coupled with organizational routines that lend those biased judgments weight and persistence (Reskin 2003). It follows that we should not spend much effort on reforming bias (because it is unconscious) but instead focus on reforming organizational routines: taking managerial discretion out of the loop, and using formal rules based on objective criteria for as many personnel decisions as possible. This study underlines a weakness in this approach. Employer discrimination may be more explicit than our theories assume. Explicit discrimination can take place not in the hiring process but in the location process. The location process determines the composition of the pipeline that most stratification research takes for granted. If we find little evidence of discrimination against black applicants in firms’ hiring procedures, can we infer that there is little discrimination against black applicants in the population? Not necessarily. Employers who choose to locate where black workers find it easier to apply probably do not discriminate much against black applicants. But employers who choose to locate where black workers find it hard to apply may very well discriminate against black applicants. We cannot tell, because we do not observe diversity in their pipelines. The applicant data used in much research is self-selected in a way that makes causal inference about our theories suspect.

Ultimately, the issue here is not whether employer bias is explicit or implicit, overt or covert. The issue is whether employers’ applicant pools should be theoretically treated as part of the supply-side generators of employment segregation (Fernandez & Friedrich 2011) and thus beyond employers’ control, or as the result of demand-side processes that could be partially remedied by employers themselves.

This is the link between the theoretical and the policy implications of this study. We noted at the start that, if employers mostly conform to a space-choice model of relocation, our policy approach to reducing spatial mismatch should also focus on spatial issues: residential segregation, transport policy, and the like. If employers mostly conform to a race-choice model, such interventions may be necessary but they will certainly be insufficient. We must also consider interventions that target employers directly. And the types of interventions that current work often recommends (Bielby 2000, Kalev, Dobbin & Kelly 2006, Dobbin, Schrage & Kalev 2015), which also largely take the pipeline for granted, could themselves be insufficient. Relying on voluntary adoption of diversity policies by employers is doubly compromised as a policy prescription: not only are such policies likely to be taken up by the firms that need them the least, but diversity itself may be a
choice variable on which firms base location decisions.

This leaves open what the right policy prescription is. We confess to having few concrete ideas, other than that whatever policies are implemented should involve universal requirements, random audits, or some combination of both. With such elements, we would at least be better positioned to determine what state policies and employer choices have which effects, and begin to find our way out of the empirical debate that has characterized work on issues like the spatial mismatch hypothesis for so long.

References


Table 1: Discrete-time Cox models predicting establishment relocation

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Black share of workforce</td>
<td>0.235***</td>
<td>0.246***</td>
<td>0.126*</td>
</tr>
<tr>
<td>Disaster(_t-1)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black share of workforce</td>
<td>0.415***</td>
<td>0.307***</td>
<td></td>
</tr>
<tr>
<td>Disaster(_t-1) × Exposure clock</td>
<td>-0.005***</td>
<td>-0.006***</td>
<td>-0.006***</td>
</tr>
<tr>
<td>Constant</td>
<td>-5.192</td>
<td>-5.244</td>
<td>-5.226</td>
</tr>
</tbody>
</table>

Observations: 2589754 2589754 2589754
Establishments: 406592 406592 406592
Log-likelihood: -88747.95 -88592.12 -88574.9

Standard errors, clustered by establishment, in parentheses
*p < 0.05, **p < 0.01, ***p < 0.001

Table 2: Discrete-time Cox models predicting establishment relocation, with fixed effects

<table>
<thead>
<tr>
<th></th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
<th>(9)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Black share of workforce</td>
<td>0.136**</td>
<td>0.153**</td>
<td>-1.591***</td>
<td>-1.773***</td>
<td>0.113*</td>
<td>-1.613***</td>
</tr>
<tr>
<td>Disaster(_t-1)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black share of workforce</td>
<td>0.352***</td>
<td>0.332***</td>
<td>0.585***</td>
<td>0.450***</td>
<td>0.390***</td>
<td>0.620***</td>
</tr>
<tr>
<td>Exposure clock</td>
<td>0.002</td>
<td>0.005***</td>
<td>0.003*</td>
<td>0.002</td>
<td>0.004**</td>
<td>0.015***</td>
</tr>
<tr>
<td>Year FE</td>
<td>Y</td>
<td>N</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
<td>N</td>
</tr>
<tr>
<td>Industry FE</td>
<td>N</td>
<td>Y</td>
<td>N</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>CBSA FE</td>
<td>N</td>
<td>N</td>
<td>Y</td>
<td>Y</td>
<td>N</td>
<td>Y</td>
</tr>
</tbody>
</table>

Observations: 2540887 2042530 2188103 2145732 1993669 1734949
Establishments: 399368 350488 346093 339805 343260 298956
Log-likelihood: -79130.11 -78571.19 -77096.95 -67637.48 -69975.17 -68197.08

Standard errors, clustered by establishment, in parentheses
*p < 0.05, **p < 0.01, ***p < 0.001
Table 3: Discrete-time linear partial-adjustment models predicting convergence of white share of establishment’s workforce to local labor market, with fixed effects

<table>
<thead>
<tr>
<th></th>
<th>Disaster</th>
<th>No disaster</th>
</tr>
</thead>
<tbody>
<tr>
<td>Share white$_{t-1}$</td>
<td>0.969***</td>
<td>0.976***</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>County share white</td>
<td>0.000</td>
<td>0.005**</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Δ County share white</td>
<td>0.033</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Establishment size</td>
<td>0.000</td>
<td>0.000*</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Δ Establishment size</td>
<td>-0.000***</td>
<td>-0.000***</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Establishment age</td>
<td>-0.000</td>
<td>0.000*</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>ρ</td>
<td>.031</td>
<td>.024</td>
</tr>
<tr>
<td>Observations</td>
<td>16402</td>
<td>42522</td>
</tr>
<tr>
<td>Establishments</td>
<td>4749</td>
<td>7357</td>
</tr>
<tr>
<td>$R^2$</td>
<td>.951</td>
<td>.954</td>
</tr>
<tr>
<td>Log-likelihood</td>
<td>24596</td>
<td>68412</td>
</tr>
</tbody>
</table>

Standard errors, clustered by establishment, in parentheses

Δ Establishment age omitted because colinear with time

* p < 0.05, ** p < 0.01, *** p < 0.001
Figure 1: Core urban and suburban counties in the largest 50 core-based statistical areas of the United States.

Figure 2: FEMA major disaster declarations by county, 1971–2014.
Figure 3: Exogeneity of the treatment: estimated probability of establishment relocation, relative to year when a natural disaster occurred.
Figure 4: Comparing establishment-relocation destinations in disaster and non-disaster years. “Random” is the expected distribution of establishments among target counties presuming random moves.