

Bureaucracy and Employment Segregation: Evidence from Union-Representation Elections

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Abstract

Does bureaucracy limit organizations' scope for discrimination? Considerable research has shown that certain formal, bureaucratic employment practices aimed at promoting workforce diversity have been associated with the increased presence of women and minorities in managerial positions, though occupational segregation has remained largely unchanged. While they are encouraging, it is hard to draw causal inferences from such studies because it is possible that the adoption of such policies is endogenous to the firms' wish to hire and promote more women and minorities. This study exploits the features of union organizing drives, particularly their representation elections, to conduct a regression-discontinuity investigation into unionization's effect on workforce composition. I find that while unionization is associated with more representative workplaces and more women and minorities in management, these effects disappear when considering the most similar cases closest to the discontinuity threshold. On balance, most of the apparent benefits of unionization may be attributable to the unobserved features of establishments where organizing succeeds. This has similar implications for the causal effects of diversity policies adopted by managers.

Introduction

Does bureaucracy limit organizations' scope for discrimination? Nearly a century ago, Weber argued that a defining feature of bureaucratic organization was how it "separates the bureau from the private domicile of the individual, and, in general...segregates official activity as something distinct from the sphere of private life" (Weber 1968, p. 197). Formal organizations have goals and institutional rationales

that are distinct from the familial, ethnic, religious and other loyalties in which organizational members are otherwise embedded. The ideal bureaucracy is one in which members are recruited and rewarded based not on those external loyalties but rather on their ability to carry out the organization's functions. An organizational staff member who rises on meritocratic criteria "functions more exactly, from a technical point of view, because, all other circumstances being equal, it is more likely that purely functional points of consideration and qualities will determine his selection and career" (Weber 1968, p. 201). Among the social sciences, sociology has perhaps the strongest *ex ante* sympathy for bureaucracy, precisely because sociologists recognize the often pernicious influence that informal ties and ascriptive homophily can have on social life, in the absence of formal structures. Thus for example in his unromantic defense of bureaucracy, Perrow (1986, pp. 6f) emphasized its role in "Purging Particularism":

Particularism means that criteria irrelevant for efficient production (e.g., only relatives of the boss have a chance at top positions), in contrast to universalistic criteria (e.g., competence is all that counts), are used to choose employees. The criteria of efficiency and particularism are likely to clash, since the most efficient workers may lack the particular social characteristics desired.

Enlisting bureaucracy against particularism is no mere theoretical concern. For two generations, as the United States has worked to overcome centuries of racial discrimination and segregation, bureaucratic systems have been elaborated time and again to limit authorities' personal discretion in evaluating candidates for employment, education, public office and more. The lion's share of such efforts have been focused on the employment relationship, and correspondingly, considerable research has documented both the spread (Jacoby 1985, Baron, Jennings & Dobbin 1988, Dobbin & Sutton 1998, Edelman, Uggen & Erlanger 1999, Kelly & Dobbin 1999) and the impact of diversity offices (Kalev, Dobbin & Kelly 2006), performance evaluations (Castilla 2008), merit-based pay and more. On balance, it seems, bureaucracy has

the effects sociologists would predict. It is not enough simply to tell people that their actions should be meritocratic (Castilla 2010), and some best practices are better than others (Kalev, Dobbin & Kelly 2006); but formal operating procedures that limit managers' discretion in hiring, retention and promotion seem to be associated with greater opportunities for women and ethnic minorities in firms.

Such associations are heartening, but our enthusiasm is necessarily tempered by the difficulty of drawing *causal* inferences from them about the effects of adopting bureaucratic employment procedures on employment segregation. Firms that adopt formal diversity practices may have increased employee diversity in the future, but can we say that the practices caused the change? Or rather might executives' interest in increasing diversity have caused both the adoption of such practices *and* the focus on hiring and promoting women and minorities? Even in research designs that use fixed effects to control for unobserved differences among firms, the endogeneity of policies to executives' preferences is difficult to control for empirically, because those preferences can change over time (see Kalev (2009) for a discussion). To make stronger causal claims about bureaucracy's effects on workforce composition, we would prefer policies whose imposition orthogonal to firms' preferences.

This study employs a research design that makes such causal inferences more plausible. I focus on the effects of unionization on workforce composition. Like most explicit diversity policies, union contracts typically limit employers' discretion over hiring, discipline, promotion and termination. While the relationship between labor unions and minorities has historically been complicated (Botsch 1980, Nelson 2001), unions remain among the most integrated institutions in American society, and minorities frequently express support for unions because they reduce employers' arbitrary authority (Freeman & Rogers 1999, Milkman 2006, Rosenfeld & Kleykamp 2012). More importantly for the research design, unions, considered as a type of bureaucratic employment practice, allow for stronger causal claims than policies pro-

mulgated by management. First, almost by definition, employers do not choose to be unionized. Second, American workplaces are most often organized through representation elections overseen by the National Labor Relations Board (NLRB).¹ Organizing through elections allows for a regression-discontinuity approach: by focusing on close elections, ones often decided by one or two votes, I examine two groups of organizations that differ almost solely on the establishment of a labor union. Changes in workforce composition between these two groups over time can more plausibly be attributed to the union's presence.

I focus on establishments with more than 100 employees that were targeted by union-organizing drives between 1984 and 2008. Establishments of this size are required to file annual employment surveys with the Equal Employment Opportunity Commission (EEOC) that detail the racial and sexual composition of their workforce in several broad occupational categories, which means that I can track their workforce composition over time, in the wake of union organizing or its failure. Following the make-up of different occupations lets me examine segregation *within* the firm. By comparing the racial composition of the establishments' workforces to the relevant Metropolitan Statistical Area (MSA), I also track the establishment-level segregation of unionized and non-unionized sites and explore how they evolve.

I find contradictory effects of unionization. Consistent with sociological theories of bureaucracy, the workforces of establishments where unions are formed come to better reflect their larger labor markets over time. Unionized establishments also tend to increase the proportion of women and minorities filling managerial functions over time, relative to establishments where unionization failed. These effects are visible in the larger population of establishments targeted by union-organizing drives, but

¹The National Labor Relations Act (NLRA) also allows for "voluntary recognition" of a union by an employer. In recent years, unions have increasingly chosen to forego the NLRB election procedure and instead engage in tactics that end in some sort of voluntary recognition by the employer. While such drives have increased (Brudney 2005) and attracted considerable scholarly attention (Eaton & Kriesky 2001), basic information such as the number of voluntary drives per year is still difficult to collect.

they largely reflect differences in the two populations of firms. Union organizing is more successful in more segregated establishments and in firms that already have more female and minority managers, which suggests that the relationship between the union and the workforce's make-up may be spurious. Indeed when I focus on close elections, these apparent effects of unionization disappear. Furthermore, there is no evidence that unionization increases or reduces occupational segregation within establishments. Taken together, these trends suggest that unions' apparent effects on workforce diversity are better understood as a product of the establishments that are receptive to unions, rather than a direct impact of unionization itself. They also suggest that while unions have been successful at recruiting more minorities into many establishments, those new workers have typically found work in the same occupations where they were already over-represented.

This study thus makes two contributions to our understanding of employment segregation. First, substantively, it investigates the causal link between bureaucratization and the purging of particularism more directly than has heretofore been possible. It suggests caution in interpreting the effects of formal employment policies. If some of the effects on diversity of unionization, which is not chosen by the firm, are endogenous, then it is even more likely that practices chosen by management will be as well. Second, methodologically, it demonstrates a way to bring techniques associated with the "new causal analysis" (Pearl 2009), techniques that have largely been associated with economic analysis, to bear on classical sociological questions. In so doing, it reinforces on two fronts sociologists' defense of bureaucracy, flaws and all, as often our best defense against informal and ascriptive discrimination.

Enlisting bureaucracy against discrimination

Limiting favoritism is one of the basic functions of bureaucracies, alongside limiting *ad hoc* decision-making. Many of the earliest identifiable bureaucratic administrations began in organizations where nominal superiors faced the task of separating their subordinates' positions from the subordinates themselves. Being able to exchange subordinates is particularly important in any situation where the subordinate, through control of his or her own subordinates, has the potential to rival the power and authority of the nominal superior. Such attempts to "impose transitivity," in Martin's (2009) phrase, characterize initiatives as diverse as Pope Gregory's edict on clerical celibacy (Brooke 1956), the movement away from administration through household retainers under the Holy Roman Empire (Bloch 1967) and the development of written civil-service exams under the Tang Dynasty (Elman 1991). In the United States, such bureaucracies were elaborated during the growth of the first large industrial corporations (Chandler 1977, Perrow 2002) and in the waves of civil-service reform, where they were explicitly adopted to break the power of urban political machines (Tolbert & Zucker 1983). In all such cases a key feature of bureaucratic advocacy was a rationale for why the expertise or autonomy from influence required to do the job well should be more important than the influence or sympathy that other forms of inter-personal loyalty could provide. In this sense all bureaucracies reflect and enact the instrumental rationality that motivates setting them up. Many of the initial rationales for specific bureaucracies are vague indeed (Tolbert & Zucker 1983, Edelman 1990, Dobbin & Sutton 1998), but organizational actors have repeatedly been willing to institutionalize vague rationales as normative and ultimately positive descriptions of how their organizations should work (Meyer & Rowan 1977, Fligstein 1990, Dobbin 2004). This plasticity of justification has both made bureaucracy a popular answer to many intra- and inter-organizational problems (Pfeffer 1981, Meyer, Boli & Thomas 1987) and simultaneously undermined the grounds for believing that

any given bureaucratic solution would deliver (or even try to deliver) on its stated purpose.

Within the United States, the Civil Rights revolution is often noted as a turning point in which increasingly regulated and legalistic, bureaucratic management was imported into the employment relationship (Lichtenstein 2002, Piore & Safford 2006, Dobbin 2009). There is no doubt that the elaboration of case law in the last half century has eroded the at-will employment doctrine (Budd 2004). The government's outlawing of discrimination provided an opportunity for human-resource professionals to develop a new rational account about why diversity was not only meritocratic but also a source of new and different ideas for the firm (Dobbin & Sutton 1998). Furthermore, bureaucratic employment practices, which relied upon "the files" that Weber (1968) thought so characteristic, through their regular operations generated detailed records of their own logic and operations—precisely the kind of legible (Scott 1998) accounts that regulatory bodies would like to see. Interest in the impact of such bureaucratic practices on employment segregation has waxed accordingly.

Of course, modern corporate diversity policies were not adopted in a vacuum. For a generation before the Civil Rights movement, managers and trade unions had been developing their own systems of industrial jurisprudence (Jacoby 1985), and elements of the labor-relations regime often served as templates for new practices. Edelman (1990) for example studied how civil-rights grievance procedures in firms were often explicitly modeled on union grievance procedures. Thirty years before, Slichter, Healy & Livernash (1960) had popularized a theory of how, in an era of mass unionization, "rational" employment practices that were developed during collective bargaining in unionized firms diffused into non-unionized ones, partly as those firms sought to avoid unionization by offering specific employment practices and thus taking arrows from the unions' quiver.

Many of the contract provisions that unions negotiated had direct implications for

the composition of firms' workforces. While closed shops, in which unions controlled hiring, were always an exception (limited mostly to shipping and the building trades), seniority systems often dictated the order in which employers could promote and lay off workers. For women and minorities, such practices were a double-edged sword: seniority systems meant that they were often the first to be laid off, but if they could accumulate seniority then the protections were considerable (Milkman 1987). More generally, unions' presence reduced employers' use of casual labor, to which women and minorities had always been disproportionately assigned (Zeiger 1986, Nelson 1992). The industrial unions, which brought together skilled and more-replaceable unskilled workers in the same bargaining units, gave positions typically held by minorities more power to determine their own employment conditions (Boyle 1995, Nelson 2001). And of course labor has long had a codependent if not always cooperative with the Civil Rights movement, one in which unions have often mobilized their memberships around issues relating to racial or sexual discrimination (Clawson 2003, Fantasia & Voss 2004).

All such policies suggest that unions would naturally be a force for increased employment integration. But of course policies that protect the status of insiders, current employees, can easily be turned into instruments of exclusion, and many unions historically were willing to do just that. One of the earliest uses of affirmative action was against the exclusionary hiring practices of the building-trades unions in Philadelphia (Schuwerk 1972), and the AFL-CIO's position on immigration only switched from favoring restrictions and stronger policing to favoring amnesty in 1995. Employers in turn have been happy to exploit ethnic, racial and sexual divisions in their workforces to divide and rule (Botsch 1980, Griffith 1988, Cohen 1990, Nelson 2001, Lichtenstein 2002).

It is therefore an empirical question precisely what effects unionization will tend to have on workplace composition. It is a sad irony that, in the same period when

unions have grown more diverse and inclusive, their influence has shrunk. Nonetheless the unique characteristics of unionized establishments—workplaces where elements of formal, bureaucratic employment policies are in place but where those policies were not endogenously chosen by management—makes them an excellent place to look for bureaucracy’s effects on segregation.

Data and methods

Data sources

I track changes in establishment workforce composition over time, across the firm and relative to the larger labor market, in the wake of a union-organizing campaign. The primary data for this study correspondingly come from three sources. Data on union-organizing drives is drawn from the AFL-CIO and the NLRB. Data on establishments’ racial composition comes from firms’ EEO-1 surveys filed with the EEOC. The racial composition of MSAs is built using results from the Current Population Survey (CPS).

Union organizing at an establishment begins with a so-called card drive, in which the union² tries to get employees in the proposed bargaining unit to sign cards showing their support for the NLRB to hold a secret-ballot election, in which employees can vote whether the union should be their representative for collective bargaining with their employer. Once 30 percent or more of the employees have signed such cards, the union can file a petition with the NLRB to hold such an election. A simple majority of votes cast determines the outcome. If the union wins then the NLRB certifies the union as the employees’ representative, and the employer is mandated to bargain “in good faith” with the union for one year. The goal during this period is typically a contract between the union and the employer that covers various terms and conditions

²Under the NLRA, an individual or an employer can also petition the NLRB to hold an election. In practice, the overwhelming majority of petitions are filed by unions.

of employment (McGuinness & Norris (1986) review the legal procedure in detail).

The NLRB opens a representation case when it receives such an election petition. It closes the case upon the election's result, or if the petition is withdrawn at any stage.³ This means that the NLRB's records contain failed organizing attempts as well as successful ones. This case information has been stored in several databases over time. Since 1999, the NLRB has used the Case Activity Tracking System, or CATS database, and since 2009 has made all CATS records available in XML format through <http://data.gov>, the federal government's digital clearinghouse. From 1984 through 1998 NLRB data was saved in the Case Handling Information Processing System, or CHIPS database. While the NLRB no longer makes CHIPS data available, the AFL-CIO maintains copies of older election cases as part of a data-sharing agreement it signed with the NLRB in the 1960s.⁴ For this analysis I use data from both the CHIPS and the CATS databases. NLRB records from before 1984 often lack the detailed address information necessary to link organizing-drive records to the EEOC's establishment surveys. Thus my population of firms that were targeted by organizing drives runs from 1984 through 2008; I stop analysis at 2008 so as to have a few years' window in which to observe any changes in workforce composition.

The Civil Rights Act (amended) of 1964 requires employers with more than 100 employees (or 50 employees if the firm has government contracts worth at least \$50,000) to file annual EEO-1 reports with the EEOC. These reports include a matrix that details the race and sex of employees across nine broad occupational categories. These are the most detailed establishment-level data on workforce composition avail-

³Up to a third of all petitions are withdrawn before an election takes place. This can introduce substantial bias when trying to estimate effects on whether unions are formed (Ferguson 2008). Because this study is designed to compare the effects of unionization among establishments that held elections, the kind of survivor bias introduced by, e.g., employer opposition is less of an issue here.

⁴I thank Gordon Pavy, Sheldon Friedman and Alfonso Nevarez at the AFL-CIO for access to and help with these data.

able (Robinson, Taylor, Tomaskovic-Devey, Zimmer & Irvine Jr. 2005), and have been used in multiple studies in particular of women and minorities' progress into management (Kalev, Dobbin & Kelly 2006, Cohen, Huffman & Knauer 2009, Huffman, Cohen & Pearlman 2010). The focus of this study differs in that I focus on the *non*-managerial workforce described in the reports, though I do also examine changes in management composition for comparison to other work. I obtained EEO-1 data from the EEOC through an Intergovernmental Personnel Act (IPA) agreement.

To link the two data sources, I match the NLRB's representation cases with the EEO-1 surveys. Unions filed 66,097 election petitions with the NLRB between 1984 and 2008. Election petitions can be filed at workplaces that have already been unionized, for example to decertify one union and certify another. Nearly eighteen thousand of the petitions had some sort of incumbent listed on the ballot. Because my focus is on the introduction of bureaucracy through unionization, I exclude such establishments. The EEOC's surveys only cover large establishments; thus many NLRB records will have no match in the EEOC data because the corresponding establishment is below the latter's employment threshold. For example, only 7,223 of the NLRB's 48,251 first-election cases involve more than 100 eligible voters. I used the approximate-string-matching algorithm in Stata's `reclink` package to match EEOC and NLRB records based on establishment name and street address. I kept matches with a match confidence of .9 or higher (see the `reclink` documentation for more details); hand-checking this group and removing some false positives left 6,223 matched cases.

Taken together, the NLRB and EEOC's data allow both fine-grained and representative exploration of the effects of bureaucracy on employment segregation, since they cover establishments across the United States. A further advantage when linking these data is that the EEOC's broad occupational categories correspond closely to the "bargaining unit type" categories tracked by the NLRB: both for example

break out “Craft Workers” and “Professionals” from other job types. This is an advantage because many organizing drives cover a subset of the employees in the large establishments monitored by the EEOC. The racial demography of work groups can vary considerably by job types within an establishment, and so relying solely on establishment-wide measures of workforce composition would introduce considerable measurement error (Tomaskovic-Devey, Zimmer, Stainback, Robinson, Taylor & McTague 2006). By focusing on the relevant job types, it is possible to construct more precise measures of workforce diversity.

To measure divergence between an establishment’s workforce and its larger labor market, I use racial information given by annual respondents to the Current Population Survey. The CPS has long asked respondents to identify their race, as well as whether they identify as Hispanic.⁵ To build an annual demographic profile for each Metropolitan Statistical Area, I use all respondents between age 18 and 65 who identify themselves as in the labor force. Some organizing drives happen at large facilities that are outside MSAs, such as large meatpacking plants in rural Iowa or auto-parts plants in the North Carolina piedmont. Additionally, the CPS’s sampling procedure can cause small MSAs to be poorly represented in some survey waves; drawing inferences about metropolitan populations from very small samples risks overstating the homogeneity of the population and thus biasing results. Excluding rural and small-city cases leaves 4,687 organizing drives that inform the analysis here.

Such a sample is not perfectly representative of American workplaces. Most obviously, only certain industries are common targets of union organizing. By construction, these are larger establishments. The data are also disproportionately urban,

⁵Mapping respondents to races is often arbitrary and always fraught. The EEOC’s survey forms break race into white, black, Hispanic, Asian American/Pacific Islander, and American Indian/Alaskan native. CPS respondents are asked about their race and their Hispanic identity in separate questions. Respondents can also list multiple races. Nonetheless, 99.5 percent of respondents in the years in question listed a single race or “black-white,” and more than 65 percent of the latter group identified as Hispanic. I therefore focused on single racial groups here. I classified black-white respondents as black, on the presumption that this most closely reflects how American employers have historically classified mixed-race workers.

though this bias is not large because MSAs are defined even on relatively small population clusters. Finally, the hostility toward unions in certain parts of the country means that pro-union regions like the northeast, the Great Lakes and the west coast are over-represented. Yet two factors should mitigate concerns of this sort. First and most importantly, the regression-discontinuity design described below estimates a “local average treatment effect,” or the effect of treatment on the treated. The trade-off in such a design is that it sacrifices some measure of generalizability to better estimate causal effects on a well-specified sub-population. Second, while the design over-samples large establishments, most Americans *work* in large establishments. Indeed the idea of bureaucratic procedures guiding employment and the principle of a “representative” workforce both presume some degree of organizational scale. Previous research on bureaucratic employment practices in large organizations has relied on similar justifications for studying this organizational population (Fernandez & Weinberg 1997, Petersen & Saporta 2004, Kalev, Dobbin & Kelly 2006, Castilla 2008).

The Regression-discontinuity framework

Regression discontinuity is at root an attempt to establish a believable counterfactual against which to compare the effect of an intervention. In this it resembles propensity-score matching (Peikes, Moreno & Michael 2008), coarsened-exact matching (Iacus, King & Porro 2011) and other quasi-experimental designs that have been introduced over the last twenty years to support stronger causal identification. As with these other techniques, we conceive of the independent variable of interest—in this case, unionization—as a “treatment” that one group receives and the other does not; we can then examine the treatment effect by comparing the differences in group outcomes.

For this approach to work, the treatment should be as exogenous as possible. In the ideal case, firms would be randomly ordered to set up bureaucratic employment policies. Outcomes in these firms could then be compared to those in firms that had

not set up such policies; and because the order was exogenous, we could attribute differences in outcomes to the treatment. Random assignment is not a realistic option in most organizational contexts, but we can approximate it with careful study design as long as certain assumptions hold.

Regression-discontinuity analysis has two main assumptions. First, the treatment should be a known, *discontinuous* function of an observed variable. Second, treatment should be a *continuous* function of other likely predictors (observed and unobserved) around the discontinuity's threshold. Van der Klaauw (2002) provides a classic example in his study of the effects of financial aid on academic performance. The causal problem he faced was that receiving financial aid was associated with better academic performance, but students have to demonstrate greater academic ability to receive aid. Van der Klaauw exploited the test-score requirements that many states use to award financial aid. Test scores are a continuous function of academic ability, but receipt of financial aid is discontinuous at a specified threshold. Presume that the cutoff is an SAT score of 1400. A student who scores 1600 likely has different (unobserved) ability than one who scores 1200, but a student who scores 1400 likely has comparable ability to one who scores a 1399. Yet one gets financial aid while the other does not. Thus, as long as one restricts the analysis to those cases very close to the treatment threshold, one can reasonably attribute outcome differences to the treatment. Further, one can compare less-restrictive threshold cutoffs in order to test the sensitivity of the treatment effect to unobserved heterogeneity in the larger population.

Following the explication in DiNardo & Lee (2002), applying the regression-discontinuity framework to unionization is straightforward. Whether a worker votes union is a well-studied function of many variables: the legal environment (Wessels 1981, Elliott & Huffman 1984), establishment size (Olson 1965, Farber 2001), the unemployment rate (Freeman 1998), employer responses (Freeman & Kleiner 1990, Fior-

ito & Bozeman 1997), organizer tactics (Bronfenbrenner 1997, Bronfenbrenner & Hickey 2004) and so on. Voting propensity is continuous, but the election result is a discontinuous outcome of the vote share the union receives. One vote over 50 percent makes all the difference. Establishments where unions win by a single vote should be comparable on other dimensions to establishments where unions lose by a single vote, and therefore we can attribute group differences in observed outcomes to the presence of the union.

More formally, suppose that the firm's level of employment segregation, measured by an index d , depends on the function $d_i = \alpha + BEP_i\beta + X_i\gamma + \epsilon_i$, where BEP_i indicates whether a bureaucratic employment practice (diversity training for managers, for example) is in place, X_i is a vector of observed and unobservable covariates (firm size, profitability *etc.*) and ϵ_i is an error term. We want to estimate β , but a straight regression on the population will give biased results. Firms decide to adopt bureaucratic employment practices based on some unobserved threshold function, which may itself be a function of X_i :

$$\begin{aligned} BEP_i &= 1 \iff \tilde{V}_i > \tilde{V}^* \\ \tilde{V}_i &= v(X_i) + M_i + u_i \end{aligned}$$

where \tilde{V}_i is the unobserved threshold and $(v)(\cdot)$ is a function of X_i . Because X_i factors into both the decision to adopt diversity training *and* the decision to hire more minorities, the composition of X_i likely differs between firms that do and do not adopt training. Worse, because X_i includes unobservable factors, we cannot determine the extent to which such unobserved heterogeneity biases our estimate of β .

Unlike many such bureaucratic employment practices, unionization is a discontinuous function of vote share, which is observed:

$$\begin{aligned} UNION_i &= 1 \iff V_i > \frac{1}{2} \\ V_i &= v(X_i) + M_i + u_i \end{aligned}$$

where V_i is the vote share for the union. We still do not observe all of X_i , so solving $d_i = \alpha + UNION_i\beta + X_i\gamma + \epsilon_i$ will still yield biased estimates of β . If M_i is continuously distributed though then the composition of X_i conditional on a win will be closer to the composition of X_i conditional on a loss, the smaller the conditional vote margin δ is:

$$\lim_{\delta \rightarrow 0} \Pr(X_i = x | V_i = \frac{1}{2} + \delta) - \Pr(X_i = x | V_i = \frac{1}{2} - \delta) = 0$$

Prior work has used this fact to examine unions' impact on plant-closure rates (DiNardo & Lee 2002, DiNardo & Lee 2004) and pay dispersion (Frandsen 2012). DiNardo & Lee (2002) for example found that while unionized establishments closed at higher rates than non-unionized ones, most of this effect was due to unionized plants' being older, more based in the manufacturing and blue-collar service sectors, and so on. Older plants were both more pro-union and more likely to fail. Examining close elections let them observe plants of comparable age and industrial composition—and showed no effects of unionization on plant closure. In practice regression-discontinuity studies almost never specify functional forms and estimates of $X_i\gamma$. Instead β is measured directly as the between-group difference in d , the outcome of interest. I employ such an approach here.

Calculating segregation

I calculate three different measures of segregation: the share of black employees in managerial positions, an index of black-white dissimilarity (to measure occupational segregation) and a χ^2 test of racial employment shares (to measure establishment

segregation). Studies that use EEO-1 data to study bureaucracy’s effect on employment segregation have tracked changes in the share of women and minorities in management (Kalev, Dobbin & Kelly (2006), Kalev (2009); see also Huffman, Cohen & Pearlman (2010)). Employment shares are easy and intuitive to understand yet only make sense if, at the start of the study period, the group in question is exclusively white and male. This holds well for managers but not for the rest of the workforce. Research that studies the racial make-up of entire establishments, such as Tomaskovic-Devey et al.’s (2006) documentation of trends in occupational segregation and Cohen, Huffman & Knauer’s (2009) study of sex segregation in management, instead uses dissimilarity indices. Such an index reflects segregation or integration across the sub-units of a larger organization, but is independent of establishment segregation, or how representative the organization is of its larger environment.

These measures, then, are calculated differently and reflect separate but related concepts. Shares in management tell us something about the promotion prospects of specific groups but nothing about their isolation or integration in the broader firm. By comparison, a dissimilarity index considers groups that are clustered in sub-units that are nested in larger units, such as blacks and whites in census tracts in a city, or in departments in a firm. For groups a and b , dissimilarity at time t , d_t , is defined as follows:

$$d_t \equiv \frac{1}{2} \sum_i \left| \frac{a_{it}}{A_{it}} - \frac{b_{it}}{B_{it}} \right|$$

where a_{it} is the share of group a in sub-unit i at time t , A_{it} is the share of group a in the larger unit at time t , and so on. Intuitively, the index measures how many people would have to swap places in order to make the composition of the sub-units equal to the composition of the larger unit. When $d_t = 0$, the racial make-up of any sub-unit matches the racial make-up of the organization as a whole. This does not mean though that the organization reflects *its* environment: if in a firm black workers

make up exactly five percent of each department, but the firm operates in an urban area that is 40 percent black, then we might balk at calling the firm integrated. To measure establishment segregation, I generate expected frequencies for each racial group (white, black, Hispanic and Asian) based on the establishment's size and the racial composition of the firm's MSA. I then calculate $\frac{(O-E)^2}{E}$, the χ^2 score for each racial group, and sum across races to produce an establishment-level χ^2 score.

These three measures need not track in the same direction. Fundamentally the index of dissimilarity is changed by moving around existing workers within the firm, while the χ^2 score is changed by moving workers into and out of the firm. An establishment can become more representative while remaining occupationally segregated and vice versa. We know that formal HR policies can have very different effects on hiring, retention and promotion (Sørensen 2004, Fernandez & Mors 2008, Kalev 2009). It therefore makes sense to consider the impact of a workplace change like unionization on all of these dimensions.

Like most other employment practices, unionization mandates no immediate hiring or firing. Any impacts will happen over time, as workers enter or leave the establishment. I therefore track changes over time in these measures. For women and minority shares in management and occupational segregation, I calculate the mean statistics across establishments for units where the union organizing drive succeeded and for units where it failed. For establishment segregation, I calculate the mean score across MSAs for units where the drive succeeded and failed. I follow changes in these means for successive years after unionization for the full sample, and then contrast trends in the full sample with sub-samples restricted more and more closely around the 50-percent vote threshold.

Results

Descriptive statistics

Table 1 reports the average share of blacks in managerial positions, black-white⁶ index of dissimilarity and χ^2 score of establishment segregation for the full sample of matched establishments. I group establishment-year observations by the number of years that have passed since the organizing drive, since it makes more sense to consider for example all firms that have been organized for two years than all firms in 1997. We would not expect common effects across establishments in a calendar year, because establishments will have had unions in place (or not) for different lengths of time.

[Table 1 about here.]

Three broad patterns are visible in table 1. First, there is an upward trend in the share of black managers over time in unionized establishments, while the trend in unsuccessfully organized establishments is basically flat. This trend, which also exists for women managers in unionized establishments, is similar to the relationship that Kalev, Dobbin & Kelly (2006) found between formal policies that establish organizational responsibility for diversity and female and minority representation. It is slightly surprising to see this effect associated with unionization, since collective bargaining agreements do not cover managerial personnel. One possible explanation is of course that unionized firms are more likely to put in place the sort of policies that Kalev, Dobbin & Kelly (2006) explored. It should be noted that the presence of unions in their models showed a small, negative and insignificant effect on women and minority presence in management once those policies were controlled for. Another

⁶Both shares of managerial employment and indexes of occupational segregation are based on two-group comparisons. Comparisons between whites and other groups, available on request, are qualitatively similar.

explanation is that firms that are more receptive to promoting managerial diversity are also relatively more receptive to unionization. It is notable for example that there is an initial difference in the average share of black managers in the two groups.

Second, establishment segregation of unionized establishments tends to decline sharply, relative to non-unionized ones. This integration of unionized establishments, relative to their larger labor markets, consists mostly of firms that are less diverse than the MSA becoming more heterogeneous, rather than “excessively” diverse firms becoming less heterogeneous; and the trend exists for firms that were unionized both with exceptionally high and exceptionally low shares of minority workers. Yet this decline in establishment segregation should be interpreted with caution, precisely because the places where organizing drives succeed appear to have quite different racial make-ups, on average, from firms where drives fail (as reflected in the shrinking differences between the groups over time). Differences in starting conditions like these augur that differences between the two groups of firms may predict both unionization and changes in workforce composition—the endogeneity described above.

Third, unionization seems to have no effect on occupational segregation. The index of black-white dissimilarity for both groups of establishments is stagnant over time. This reflects Tomaskovic-Devey et al.’s (2006) finding that, after making considerable progress in the 1960s and 1970s, occupational segregation stalled in the 1980s and 1990s. In a context like this, where establishment segregation is declining over time, unchanging occupational segregation implies that while such firms were hiring a more representative workforce, they tended to place blacks and whites into occupations where each group had already been over-represented.

Overall, table 1 suggests that establishments’ racial make-up follows a different trend in the wake of a successful organizing drive than in the wake of a failed one. These are similar trends to what prior research into the effects of bureaucratic employment practices on workforce composition has found. Yet table 1 also shows that

the establishments targeted by successful and unsuccessful organizing drives differ in their composition at the time they are targeted. This leaves open the possibility that some unobserved differences in the two groups of establishments affect both the success of union organizing and the make-up of the workforce over time. To investigate the possibility further, I concentrate on cases near the 50-percent threshold.

Validating regression discontinuity

Figure 1 shows the distribution of vote shares among the 4,687 elections studied here. The presence of many elections wherein the union received 100 percent of the vote (these tend to be small elections within large establishments) biases the distribution slightly away from normal, but otherwise the histogram is unimodal with a peak near 50 percent. Such a distribution helps the analysis because there are many cases close to the threshold itself, which allows for narrower standard errors. More importantly, it is easier to generalize from threshold cases to the larger population when the threshold lies near the data's center of mass.

[Figure 1 about here.]

The regression-discontinuity design used here relies on the assumption that establishments that have very close union victories are similar on observed *and unobserved* characteristics to establishments that have very close union defeats. By definition, assessing balance on unobservables is impossible, but we can examine whether there are any discontinuities on relevant observable characteristics near the 50-percent vote threshold. Figures 2 and 3 present analyses for eight possible confounding variables. For each variable examined here, I estimate the local average for that variable among establishments where the union won 5 percent of the vote, 10 percent of the vote and so on, up to 100 percent. I also fit two cubic regressions to each variable, as a function of vote share: one regression is fit on cases where the vote share was less

than 50 percent, while the other is fit on cases where the share was greater. Each panel plots the local averages and polynomial regression lines for one variable. If the two regression lines approach one another as the vote share approaches .5, then we can assume continuity around the threshold.

[Figure 2 about here.]

[Figure 3 about here.]

The first three panels of figure 2 show the average values of the outcome variables among different groups of elections. In each case, the observed and predicted values for each variable are very similar near the 50-percent threshold. The one possible exception is occupational segregation, where the fitted curves approach one another but from sharply different directions. This effect stems from the narrow range of occupational segregation observed in these data, which exaggerates the slope of the plotted curves. The difference in slopes is not statistically significant. The fourth panel of figure 2 shows that similar continuity exists in the racial composition of the areas where elections are held. Figure 3 compares other observable characteristics: the union density of the MSA, the size of the bargaining unit, the proportion of cases in manufacturing establishments, and the proportion in right-to-work states. Studies have found all of these factors correlated with election outcomes (see Heneman & Sandver (1983), Riddell (2004) and Ferguson (2008) for reviews), as indeed the curves in figures 2 and 3 suggest. In each of these cases though crossing the 50-percent threshold is not associated with a striking change in the level of the variable concerned—unlike unionization itself, which is a step function at 50 percent support. Other variables related to election success, such as the local unemployment rate, the party in control of state government and the distribution of unions involved all follow the same pattern (further analyses are available upon request). Together, these

findings lend empirical support to the assumption that close union elections can be considered comparable on their observable and unobservable characteristics.

Consequences of unionization

Figure 4 explores the relationship between unionization and establishment segregation in greater detail. The top panel reproduces the findings from table 1: unionized establishments reduce their deviance from the larger labor market faster than non-unionized establishments do. Yet as figure 4 makes obvious, newly unionized establishments *start out* less representative on average. Unionized establishments are not bounding ahead so much as catching up. Each panel shows time series for the same variable calculated on the subset of all cases where the vote share was within the indicated margin. Thus for example the bottom panel of figure 4 is based on the 283 cases where the union's share of the vote was between 48 and 52 percent (in a 50-person election, this equals a single vote). Standard errors are necessarily wider in a smaller group like this, but it is nonetheless striking how close the two group means are to one another over time. This sameness holds if we relax the bounds of comparison to a 6-percent margin, or even a 10-percent one (i.e., vote shares between 40 and 60 percent). It is difficult not to conclude that the apparent effects of unionization on establishment segregation in the top panel of figure 4 are due as much to the unrepresentative make-up of many places targeted by unions as the formalization of employment itself.

[Figure 4 about here.]

A similar result emerges from figure 5, which similarly explores blacks' shares of managerial positions. In the full sample unionization increases the share of black managers, albeit from a higher starting point. This effect also vanishes when we focus on stricter sub-samples. Occupational segregation (figure 6) shows even smaller

effects: while unionized firms present less occupational segregation in the full sample, this effect is not significant, and is reduced even further in the restricted samples.

[Figure 5 about here.]

[Figure 6 about here.]

Discussion

Taken together, these results should give pause to proponents of bureaucracy as a means to reduce employment segregation. Unions have multiple goals, and increasing the diversity of a given workforce is only one of those. It is tempting to assume that unions do not affect workplace composition because, while unions are one way to formalize the employment relationship, they do so in a fashion that directs managerial attention to wages, working conditions and other concerns that are not directly tied to diversity. It nonetheless remain true that, as a group, unionized establishments have experienced some of the same changes in workforce and managerial composition as firms with explicit practices meant to encourage equal opportunity (Dobbin 2009). This study shows that those changes have more to do with unobserved heterogeneity among unionized and non-unionized firms than with unionization *per se*. It is important to stress that a strong alternative hypothesis is possible: that diversity practices adopted by management are *even more likely* to be endogenous to the interests and goals of the firm than unionization is, and therefore if we could jointly model the adoption and effects of such practices we could explain away their apparent causal effects as well.

Such a model seems conceptually possible and indeed would help tie together the two main strains of research on diversity policies: their adoption and their impact. Studies of the diffusion of bureaucratic employment policies have a long tradition in neo-institutionalist research (Baron, Dobbin & Jennings 1986, Edelman 1990). An

obvious next step for such studies would be to build a detailed model of the adoption of diversity policies, then perform a matched-pair analysis of firms that could explore the effects of policies conditional on adoption. While such an approach cannot fully control for endogeneity, it can offer a good estimate of the likely size of any bias. Similarly, future research on unionization should incorporate information about the other diversity practices that establishments have in place. The assumption in this study is that establishments where elections were close will be similar in their other HR practices, but this could be investigated and such practices' interactions with unionization farther from the discontinuity could be explored.

A final note about measuring unionization should be made, lest this study's findings seem entirely pessimistic. The goal of unionization is almost always a contract with one's employer specifying things like the terms and conditions of employment. Yet many newly organized bargaining units do not manage to reach a first contract with their employers. By the turn of the century, only 61 percent of new bargaining units, on average, were managing to negotiate a contract with their employer within the one-year period stipulated by the Wagner Act (Ferguson 2008). Obviously, bargaining units without contracts will have much less of an effect on all dimensions of the workplace. In this study, this inconvenient fact acts like measurement error, treating establishments as unionized and thus "expecting" impact from that unionization when in fact the lack of a contract makes impact unlikely. This is admittedly akin to blaming the victim, and the urgent empirical issue is to gather better data on unions' first contracts and incorporate such data into future analyses.

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Table 1: Demographic outcomes in the wake of union organizing drives. Cohorts represent years since the organizing drive.

Cohort	Black share of managers			Occupational segregation			Establishment segregation			Observations				
	All	Won	Lost	All	Won	Lost	All	Won	Lost	All	Won	Lost		
0	0.066	0.080	0.055	0.335	0.333	0.337	-0.004	20187.5	26672.4	14982.4	11690.1	4631	2062	2569
1	0.070	0.085	0.058	0.332	0.332	0.332	0.000	19618.8	26974.1	13827.2	13147.0	4347	1915	2432
2	0.074	0.090	0.062	0.332	0.332	0.332	-0.001	17908.7	24861.7	12454.8	12406.9	4097	1801	2296
3	0.074	0.090	0.061	0.334	0.329	0.338	-0.010	16496.1	21000.8	13023.5	7977.3	3857	1679	2178
4	0.075	0.094	0.061	0.334	0.328	0.338	-0.009	16323.8	20372.4	13194.6	7177.8	3654	1593	2061
5	0.077	0.095	0.063	0.330	0.324	0.334	-0.010	15772.8	20834.8	11855.3	8979.5	3452	1506	1946
6	0.077	0.098	0.062	0.331	0.323	0.338	-0.015	15201.4	20458.4	11194.5	9263.9	3283	1420	1863
7	0.077	0.097	0.061	0.331	0.326	0.336	-0.010	16223.5	23281.5	10825.2	12456.3	3122	1353	1769
8	0.079	0.103	0.061	0.331	0.325	0.336	-0.011	13591.5	19195.4	9376.1	9819.3	2970	1275	1695
9	0.076	0.100	0.058	0.329	0.320	0.335	-0.015	14673.7	17042.2	12925.8	4116.4	2819	1197	1622
10	0.076	0.103	0.057	0.328	0.321	0.333	-0.012	12685.3	17077.4	9486.0	7591.4	2667	1124	1543
11	0.075	0.101	0.056	0.325	0.324	0.326	-0.002	12042.3	11733.0	12265.3	-532.4	2339	980	1359
12	0.077	0.104	0.057	0.320	0.316	0.323	-0.007	9482.8	12684.4	7194.0	5490.4	2015	840	1175
13	0.076	0.103	0.056	0.323	0.320	0.325	-0.005	7857.4	10730.4	5756.5	4973.9	1733	732	1001
14	0.081	0.110	0.058	0.323	0.318	0.326	-0.008	7665.5	11261.0	4924.2	6336.8	1491	645	846
15	0.083	0.115	0.060	0.319	0.317	0.320	-0.003	4664.4	5204.7	4277.6	927.1	1244	519	725
16	0.082	0.110	0.061	0.321	0.316	0.325	-0.010	5254.4	6257.1	4520.8	1736.3	1039	439	600
17	0.082	0.113	0.060	0.318	0.316	0.320	-0.004	4395.8	6200.7	3092.9	3107.7	873	366	507
18	0.078	0.107	0.056	0.319	0.317	0.320	-0.003	4354.9	6525.6	2768.4	3757.2	701	296	405
19	0.076	0.110	0.050	0.319	0.322	0.317	0.005	3680.6	4308.4	3217.8	1090.6	542	230	312
Total	0.075	0.096	0.059	0.037	0.330	0.325	-0.007	14514.7	19147.2	10946.7	8200.5	50876	21972	28904

Overall differences in bold are significant at $p < .05$. “Black share of managers” reports the share of black employees among the total listed in occupational category 1, “Officers and Managers,” of the establishment’s EEO-1 survey. “Occupational segregation” reports the index of black-white dissimilarity, calculated across the nine occupational categories of the EEO-1 survey. Lower values represent less occupational segregation. “Establishment segregation” reports a χ^2 test of the divergence of the racial distribution of employees in the establishment from the marginal distribution of the establishment’s metropolitan statistical area, calculated from the CPS. Lower values represent less divergence.

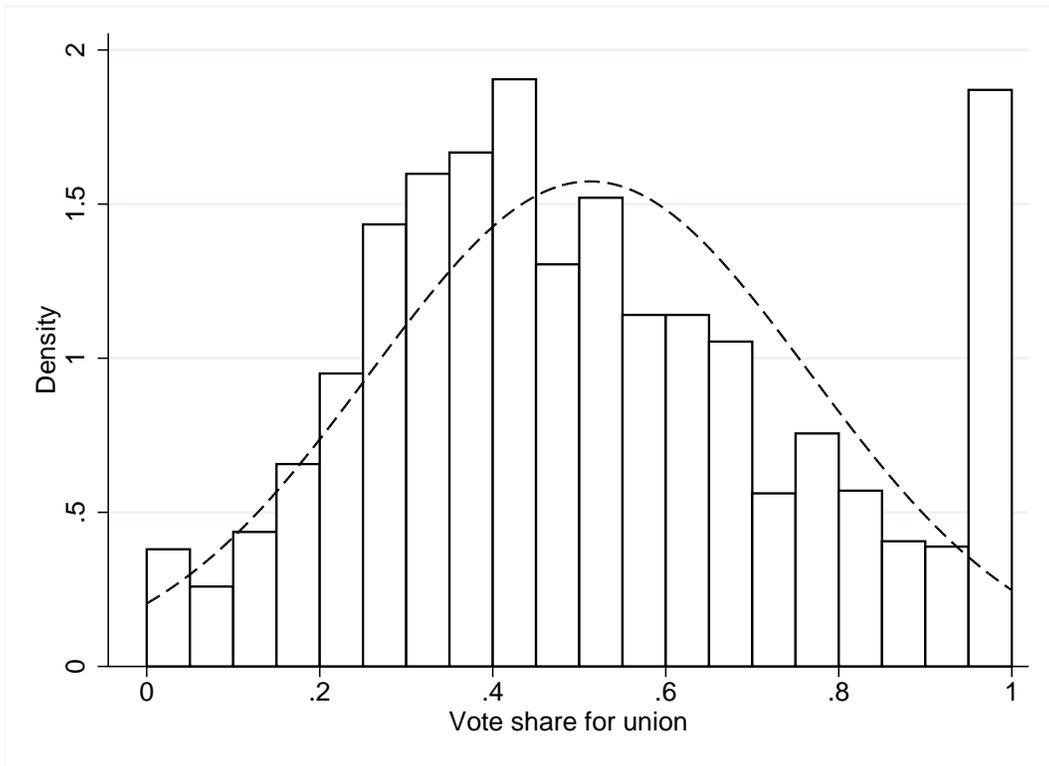


Figure 1: Distribution of vote shares in large establishment union-representation elections, 1984–2008.

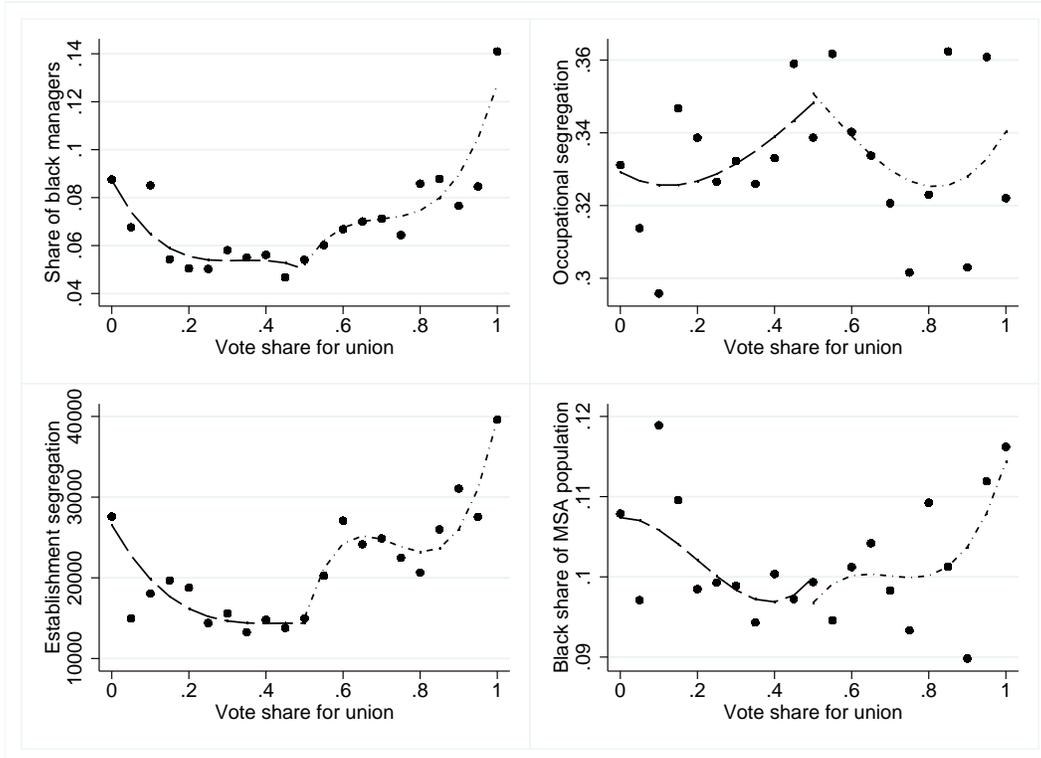


Figure 2: Inspecting the validity of the regression-discontinuity assumption. Each panel plots local averages for the response variable at 5-percent intervals of union vote share, as well as cubic regression lines fitted to the data above and below the 50-percent threshold. Continuity can be assumed when the regression lines approach one another near the threshold, as they do in each case here.

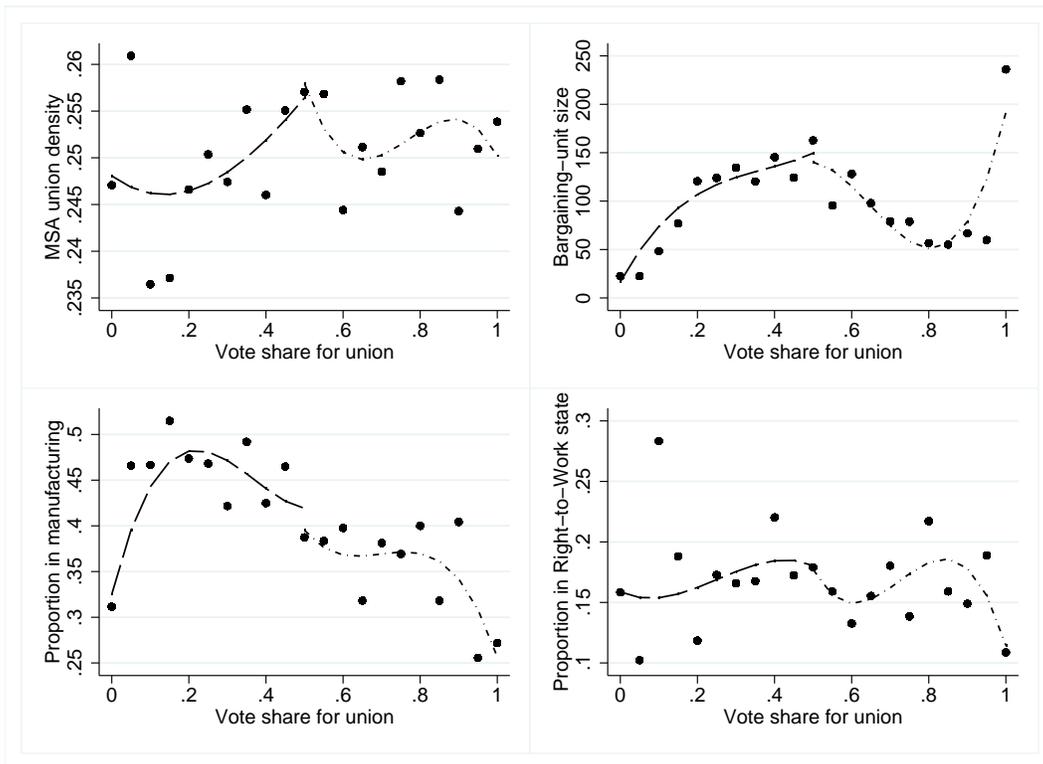


Figure 3: Inspecting the validity of the regression-discontinuity assumption, continued. Panels are constructed as in figure 2.

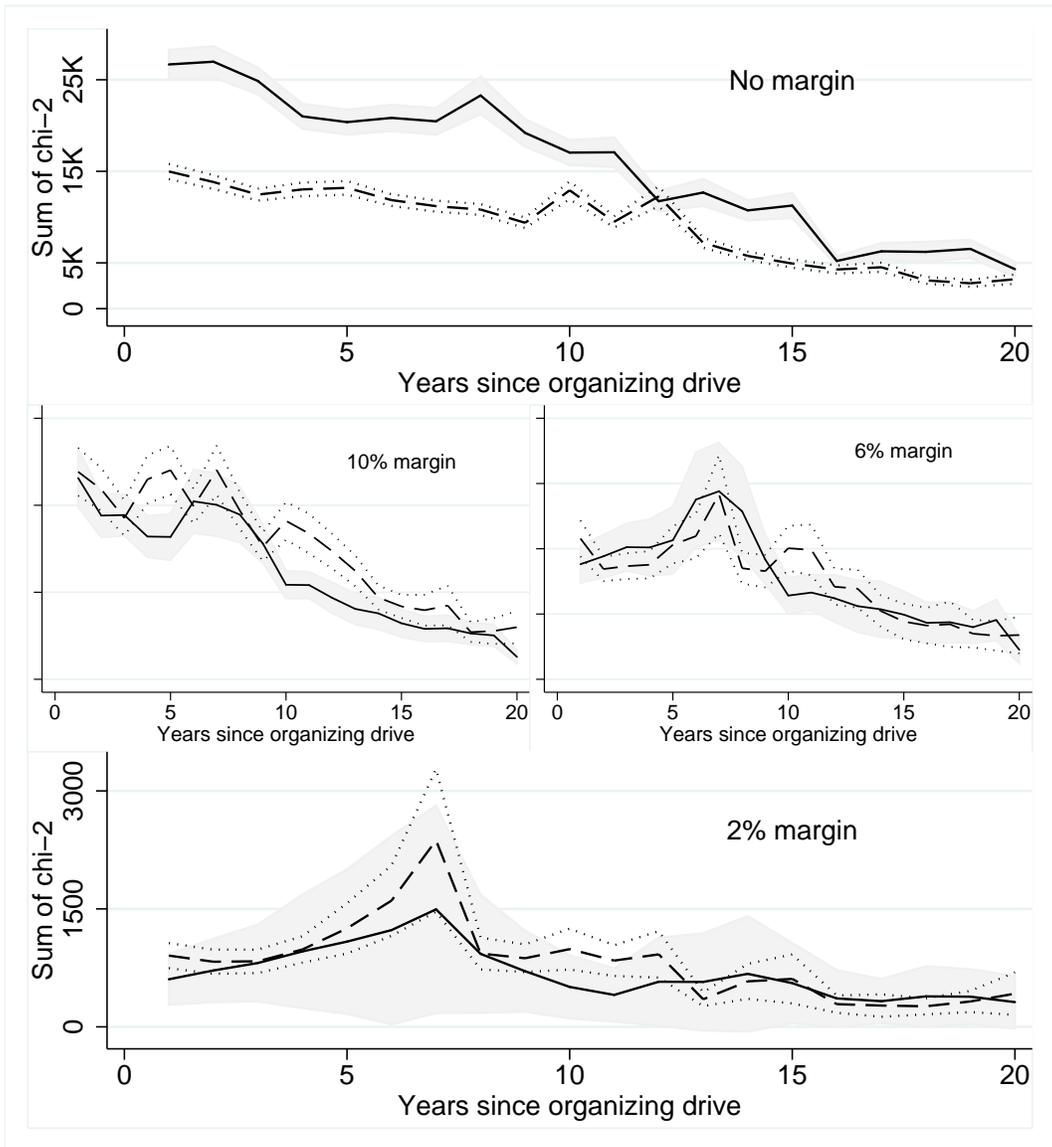


Figure 4: Comparison of unionization’s effect on establishment segregation, using increasingly strict sub-samples to exploit the regression-discontinuity design. In each panel, the solid line and corresponding confidence interval represent those establishments where a union organizing drive succeeded within the specified margin; the broken line and confidence interval are establishments where a drive failed. Sum of χ^2 is dependent on population size, so the absolute heights of the curves are not directly comparable, though slopes are.

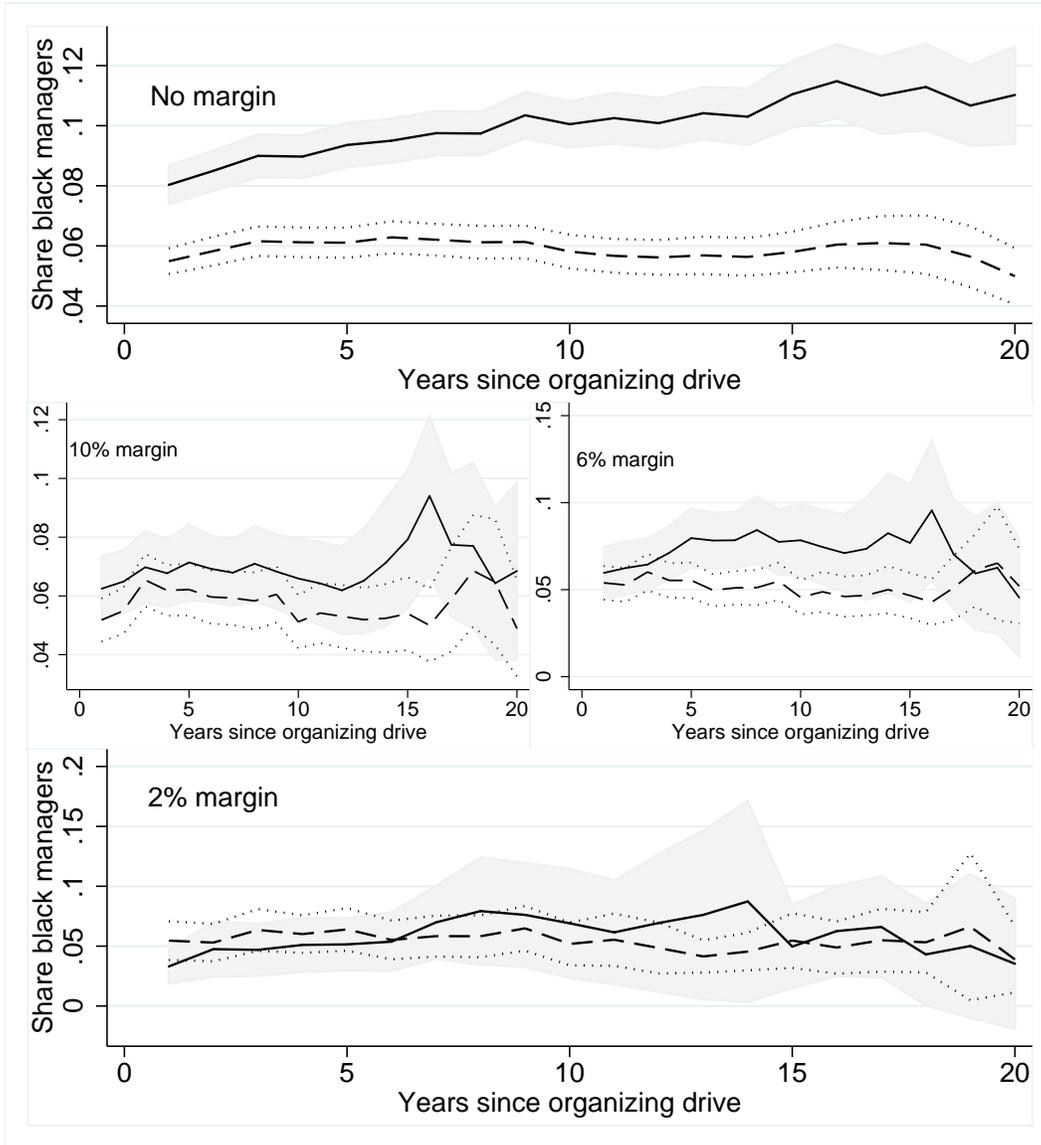


Figure 5: Comparison of unionization’s effect on blacks’ share of managerial positions, using increasingly strict sub-samples to exploit the regression-discontinuity design. In each panel, the solid line and corresponding confidence interval represent those establishments where a union organizing drive succeeded within the specified margin; the broken line and confidence interval are establishments where a drive failed.

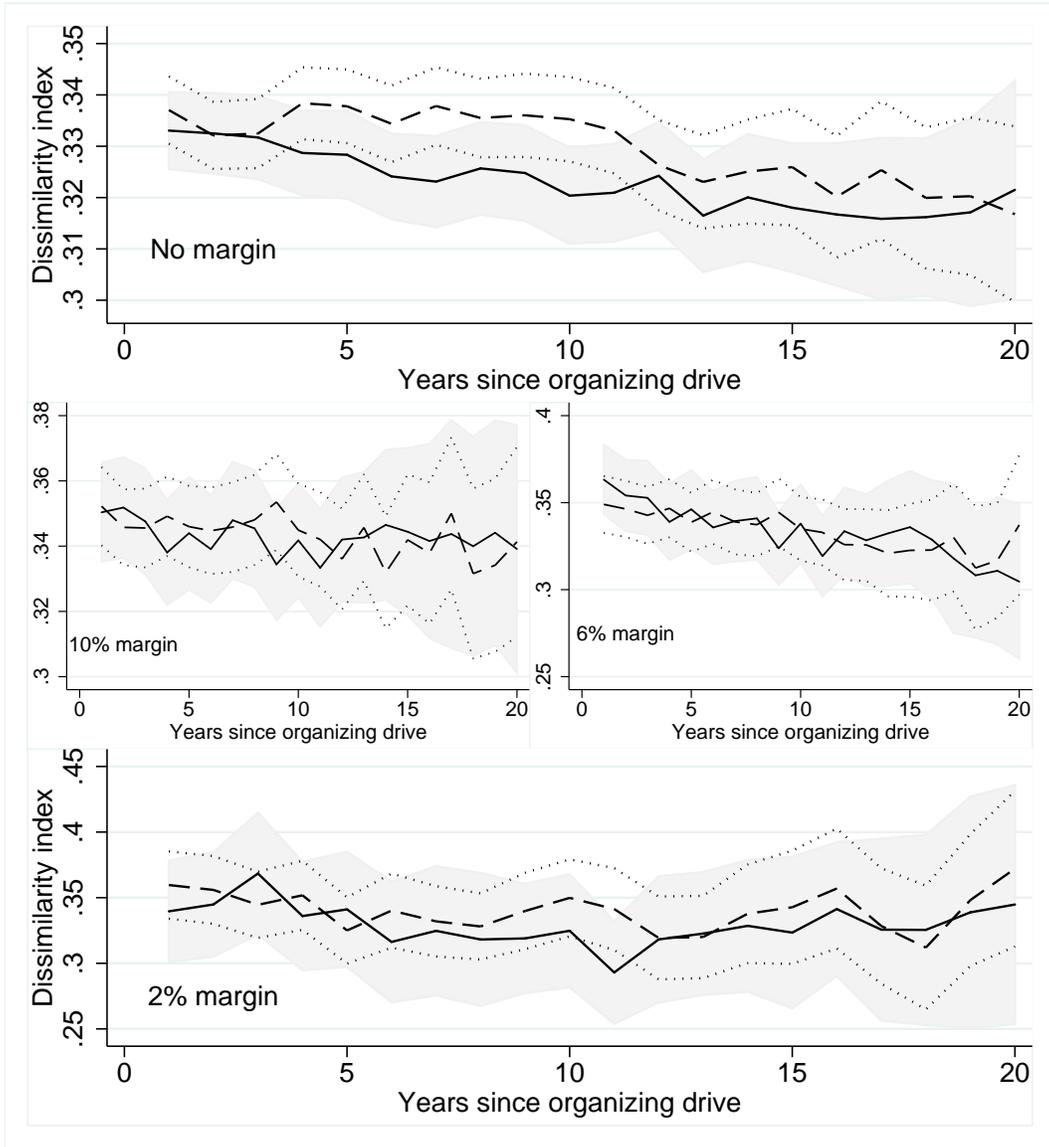


Figure 6: Comparison of unionization’s effect on occupational segregation, using increasingly strict sub-samples to exploit the regression-discontinuity design. In each panel, the solid line and corresponding confidence interval represent those establishments where a union organizing drive succeeded within the specified margin; the broken line and confidence interval are establishments where a drive failed.