The Impact of City Contracting Set-Asides on Black Self-Employment and Employment*

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February 2013

Forthcoming, Journal of Labor Economics

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ABSTRACT

In the 1980s, many U.S. cities initiated programs reserving a proportion of government contracts for minority-owned businesses. The staggered introduction of these set-aside programs is used to estimate their impacts on the self-employment and employment rates of African-American men. Black business ownership rates increased significantly after program initiation, with the black-white gap falling three percentage points. The evidence that the racial gap in employment also fell is less clear as it is depends on assumptions about the continuation of pre-existing trends. The black gains were concentrated in industries heavily affected by set-asides and mostly benefited the better educated.

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I. Introduction

Since the early 1970s hundreds of billions of dollars have been spent on programs that set aside or set percentage goals for government contracts for minority-owned firms and subcontractors. In 2008 alone, $28.2 billion of federal procurement contracts were awarded to disadvantaged business enterprises, representing 6.3 percent of the total value of all federal contract awards (SBA 2008). The amount spent on affirmative action contracting programs for minority-owned firms dwarfs the total amount spent by the federal government on enforcement of statutes legislating antidiscrimination and affirmative action in employment, such as Title VII of the 1964 Civil Rights Act and Executive Order 11246.[1] The 1980s, in particular, witnessed substantial growth in the value of *city* government contracts reserved for minority-owned business. The stated purpose of these set-aside programs was to develop minority enterprise, counter the effects of past discrimination, and reduce the high unemployment rates among urban minorities during the 1980s.

Over the last two decades, the city programs established in the 1980s have been judicially and legislatively challenged and dismantled. The *City of Richmond v Croson Co.* Supreme Court decision in January 1989 invalidated the use of such programs unless they were used as narrowly tailored remedies for identified discrimination. The 1995 *Adarand Constructors, Inc. v. Peña* Supreme Court decision, and voter referenda passed in California (1996), Washington (1998) and other states, further brought into question the future of government-sponsored set-asides.

Given the legal and political controversy surrounding these programs, there is surprisingly little quantitative evidence on their effects, especially on the formation of minority-owned businesses. We evaluate the impact of the numerous affirmative action contracting programs passed in large cities during the 1980s on business ownership and employment of African-American men. The impact on black self-employment is of special interest since blacks continue to have substantially lower rates of business

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[1] For example, in fiscal year 2001, the Equal Employment Opportunity Commission (EEOC) obtained $247.8 million for charges filed and resolved under all statutes enforced by the EEOC (Title VII, ADA, ADEA, and EPA); while the Office of Federal Contract Compliance Programs (OFCCP) – charged with enforcing Executive Order 11246, Section 503 of the 1973 Rehabilitation Act, and the 1974 Vietnam Era Veterans' Readjustment Assistance Act – obtained $29.0 million in financial remedies. Procurement of city government contracts to minority-owned firms located just in Washington D.C. was $170 million in 1985 alone.

ownership than whites even in light of the major gains made in education, earnings, civil rights and political representation (Fairlie and Robb 2008). As the main alternative to wage and salary employment, business ownership has important implications for earnings and wealth inequality. Prior work suggests that the self-employed accumulate more wealth than wage and salary workers and have higher savings rates (Borjas 1999, Bradford 2003).[2] While business owners represent roughly one-tenth of the workforce, they hold nearly 40 percent of total U.S. wealth (Bucks, Kennickell, and Moore 2006). Further, since black firms are more likely to hire minorities, it has been argued that promoting black business growth is a more effective approach to reducing black unemployment than overall economic development (Bates 1993, Boston 1999, 2006, and U.S. Census Bureau 1997).

A growing number of studies indicate that limited financial and social capital and discrimination are partly responsible for low rates of business formation among blacks.[3] Blacks have been found to face discrimination in credit markets, which will constrain business formation even when the return of the business exceeds the market borrowing rate. Low levels of personal wealth and liquidity constraints may also reduce opportunities to start and operate successful black businesses. Previous research further finds that black firms face consumer discrimination and have limited opportunities to penetrate networks, such as those in construction. Each of these barriers to black business creation suggests a potential for affirmative action contracting programs to address an efficiency loss.

With Current Population Survey data on black and white men from 1979 to 1989, we use the staggered timing of set-aside programs across U.S. cities to estimate their impacts on the self-employment and employment rates of African-Americans. We use "difference-in-differences-in-differences" (DDD) and an "event study" methodology, based on the exact dates of program implementation, to estimate the program effects. The latter approach allows us to examine trends in black and white rates, before and after program initiation, and test for non-randomness in the timing and location of the programs.

[2] Hamilton (2000) finds that earnings differentials between the self-employed and wage and salary workers cannot be explained by negative selection into self-employment and concludes that the nonpecuniary benefits of self-employment are large.
[3] For example, see Bates (1997), Fairlie (1999), Hout and Rosen (2000), Cavalluzzo, Cavalluzzo, and Wolken (2002), Blanchflower, Levine and Zimmerman (2003), and Fairlie and Robb (2008). There is little evidence that blacks have less preference for business ownership (Koellinger and Minniti 2006, Walstad and Kourilsky 1998).

Remarkably, there is little consensus in the literature on the exact start dates of city programs. We construct a new database of affirmative action program dates from four sources – in addition to two existing sources that often disagree, we conducted an extensive search of federal and state court cases and interviewed numerous government employees and officials involved with city programs. The resulting information allows us to cross-validate the program dates and investigate the impact of set-asides more comprehensively and reliably than previous studies.

Black business ownership rates increased significantly after program initiation, with the black-white self-employment gap falling by three percentage points (35-40 percent). The evidence is clear, as the self-employment gap is stable in the years preceding set-asides, and the magnitudes correspond with information from other sources on the growth in, and the set-aside amounts awarded to, black-owned businesses. The racial gap in employment fell by roughly four percentage points after program initiation, but these results are sensitive to assumptions about the continuation of pre-existing trends. City programs were preceded by several years of declining black employment in the industries that did not benefit from the programs (e.g., manufacturing and government). Black employment rates in the sectors most affected by set-asides, however, are more stable in the pre-program period.

The black gains are concentrated in industries heavily affected by set-asides and accrued mostly to the better educated. It appears that city programs led to a reallocation of self-employment from white to black men, as there is little change in the aggregate rates. This finding is sensible as overall spending by city governments did not change – that is, set-asides appear to have impacted the share of contracts awarded to minority firms but not the total amount of awards. The employment effects in the most affected sector imply that set-aside programs increased black employment more than affirmative action programs that explicitly set goals for minority hiring in firms that receive federal contracts.

The next section describes the set-aside programs and discusses the few studies exploring their impacts. Section III details the data on set-aside program dates. Section IV discusses the CPS data and presents black self-employment and employment trends during the 1980s. Section V presents the econometric framework, and Section VI contains the empirical results. Section VII concludes.

II. City Set-Aside Programs

Background

There are two types of set-aside programs. In one variant, a specified percentage of the number or total dollar value of government contracts is allotted to minority-owned businesses. In the other, prime contractors are required to allot a specified percentage of the total amount of government contracts to minority-owned subcontractors and/or suppliers (Rice 1991 and Myers 1997).[4] The percentage goals vary across programs and sometimes within programs for different purchases, such as construction contracts, procurement of goods and services, and professional services. Data on local set-aside programs listed in MBELDEF (1988) indicate that these goals range from 1 to 50 percent, with most programs having goals of 5 to 15 percent. Set-aside programs are often complemented with procurement officials who provide more general assistance to minority-owned businesses (Bates and Williams 1993).

Set-aside programs exist at the federal, state, city, county, and special district (e.g., airport, water, sanitary, park, and school) levels. At the federal level, the Small Business Administration's (SBA) 8(a) program – established in 1968 as an amendment to section 8 of the Small Business Administration Act of 1953 – is the best-known. In 8(a), the SBA serves as the prime contractor for goods and services to various federal agencies and provides subcontracts to firms that are owned by socially and economically disadvantaged individuals.[5] In 1983, 8(a) contracts totaled $2.3 billion (Bates 1985). Another federal program is the 1977 Public Works Employment Act, which required that 10 percent of all federal public works contracts be given to minority-owned businesses (Bates 1985). This program's constitutionality was soon challenged leading to the U.S. Supreme Court's ruling in *Fullilove v. Klutznick*, which upheld the federal government's use of these programs. The federal government reported $4.4 billion in total contract awards to minority and disadvantaged firms in FY 1986 (Rice 1991).

Fullilove v. Klutznick sparked the creation of set-aside provisions among other federal agencies, and state and local governments. Most states created set-aside programs for minority-owned businesses

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[4] The constitutionality of this type of set-aside was challenged in the 1995 *Adarand v. Peña* Supreme Court case.
[5] The SBA considers blacks, Hispanics, Native Americans, and Asian Pacific Americans as socially disadvantaged. In 1978, 96 percent of 8(a) firms were owned by minorities (Bates 1985).

as a direct response to requirements that state departments of transportation administering *federal* highway grants and contracts oversee implementation of the federal set-aside provisions (Myers 1997).[6] Over 200 local governments created minority business set-aside programs in response to the *Fullilove v. Klutznick* ruling (Myers 1997). Most of these programs were created in the early to mid-1980s, and many were quite substantial, especially in large central cities (Bates 1985, MBELDEF 1988). For example, Atlanta's set-aside program provided minority- and white female-owned businesses with $191 million between 1979 and 1989 (Boston 1999), and procurement to minority-owned firms in Washington, D.C. was $170 million in 1985 alone (Rice, 1991).

Potential Effects of Programs

The primary justification for affirmative action in government contracting is that it addresses discrimination against black firms. Set-aside programs may counteract the effects of consumer discrimination against black-owned businesses (Borjas and Bronars 1989, Meyer 1990 and Kawaguchi 2005) and alleviate impediments to penetrating networks, such as those found in construction (Bates 1993, Feagin and Imani 1994, Bates and Howell 1997). Discrimination among potential suppliers, prime contractors, bonding firms and governments can also create barriers to black business entry and growth.

Set-aside programs may reduce the well-documented liquidity constraints faced by black business-owners. Blacks have substantially lower levels of wealth than whites – the median net worth of black households ($5,500) is fourteen times lower than that of white households (U.S. Census Bureau 2008). Previous research finds that low levels of black wealth are associated with lower levels of business ownership, and less startup capital and worse outcomes for black-owned businesses (Fairlie 1999, Bates 1997, Fairlie and Robb 2008, Bates and Lofstrom 2008).[7] Recent work also shows that black-owned businesses experience higher loan denial probabilities and pay higher interest rates than

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[6] Rice (1991) reports that 36 states had set-aside programs in place by the late 1980s. As an example of their size, Myers and Chan (1996) report that the state of New Jersey awarded $93 million (or 3.2 percent of the total amount awarded) of public procurement and construction prime contracts to minority-owned firms in 1988.

[7] Low levels of black wealth may limit entrepreneurship due to the importance of personal and family equity invested in the business and the common use of personal assets for collateral for business liabilities and guarantees (Avery, Bostic and Samolyk 1998, U.S. Census Bureau 2006). Cavalluzzo and Wolken (2005) find that personal wealth, primarily through home ownership, reduces the probability of loan denial among business owners.

white-owned businesses, even after controlling for differences in size and credit history (Blanchflower, Levine and Zimmerman 2003; Cavalluzzo, Cavalluzzo, and Wolken 2002; Blanchard, Zhao and Yinger 2008). After adjusting for credit scores, wealth and owner and firm characteristics, a loan application filed by a black business owner is twice as likely to be denied as an application from a white owner. Set-aside programs may have an especially large effect on liquidity constraints in industries, such as construction, in which working capital is difficult to obtain and the posting of bonds is required (Blanchflower and Wainright 2005).

These constraints can lead to a suboptimal level of black business formation, growth and employment. Set-aside programs may mitigate these barriers to entry for black entrepreneurs, allowing them to launch new ventures and grow to a sustainable size. They may also circumvent some of the barriers to raising capital by providing a steady expected revenue source that allows black firms to expand. The creation of new black businesses and the growth of existing businesses due to set-aside programs may also create a disproportionate number of jobs for blacks (U.S. Census Bureau 1997, Boston 1999, 2006). For example, to the extent that black-owned businesses tend to locate in predominantly black neighborhoods, these programs can reduce the contribution of spatial mismatch to black unemployment. If barriers for minority entrepreneurs are nonexistent, however, then affirmative action in the award of government contracts can result in inefficient (self-) employment and growth.

Previous Literature

Minority business set-asides represent a multi-billion dollar, annual expenditure by governments and are controversial politically (e.g., Proposition 209 in California) and judicially (e.g., the 1995 *Adarand Constructors, Inc. v. Peña* Supreme Court decision). Little is known, however, about their actual effectiveness in promoting growth in the number of minority-owned businesses and in alleviating unemployment among blacks. Only a handful of studies have attempted to analyze whether these individual programs have met their goals.

The first substantive question is whether set-aside programs actually increase the number and total dollar amount of government contracts received by minority-owned businesses. In a study of federal

and state transportation contracting, Blanchflower and Wainwright (2005) find that set-aside programs increase the value of contracts awarded to minority-owned businesses. After a program in Minnesota was eliminated in 1999, contract awards to minority men fell from $6.5 million to less than $1 million. After the cessation of the Chicago Water Department set aside program in 1989, contracts awarded to minority businesses fell from $19.6 million to $6.9 million. Blanchflower and Wainwright conclude that set-aside program removal reduces minority business participation by 80-99 percent, on average.

Myers and Chan (1996) examine the award of public procurement and construction contracts to minority- and non-minority-owned firms before, during, and after the implementation of the state of New Jersey's set-aside program. They find that the total number of minority-owned firms receiving contracts increased, as did the volume of contracts and the number of minority-owned firms bidding for contracts. Boston (1999) finds that minority participation in government contracts in Atlanta grew from 0.13 percent in 1973 to over 38 percent in 1978 after the enactment of set-asides in 1975. During the 1980s, minority-owned firms received 15 percent ($191 million) of the total value of all contracts awarded in Atlanta. Black-owned firms also obtained $72 million of the $163 million in subcontracts awarded in Atlanta between 1992 and 1995.[8]

The existing literature provides little evidence on the next, natural question of whether set-aside programs impacted the number of minority-owned firms. Boston (1998) uses published data from the Survey of Minority-Owned Business Enterprises (SMOBE) to contrast the growth rate in the number of black-owned businesses in cities that implemented affirmative action programs in the 1980s relative to cities that did not. The information on the cities and dates in which affirmative action programs were initiated comes from MBELDEF (1988). He finds that from 1982 to 1992 the number of black-owned businesses grew 65 percent in cities with programs and 61 percent in cities without programs, and that this difference is not statistically significant. Unfortunately, the SMOBE data provide no information on the growth rates of white-owned firms in either group of cities.

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[8] Additional evidence on the relationship between set-aside programs and contract awards comes from a review of 58 disparity studies conducted in response to the *Richmond v. Croson* decision (Enchautegui, et al., 1996). It finds greater disparity in contract awards (relative to predicted levels) in jurisdictions without affirmative action programs, implying that such programs increased the amount of government contracts received by minority-owned firms.

Bates and Williams (1993) find that between 1982 and 1987 the number of black firms and total

sales by black businesses increased more in cities with black mayors than in cities without. They argue

that the black mayor effects are partly due to their support of minority business set-aside programs. Using

data collected by the Joint Center for Political and Economic Studies (JCPES) on set-aside programs in

28 cities (JCPES 1994), Bates and Williams (1995) explore whether the characteristics of preferential

procurement programs affect the survival of minority-owned businesses.[9] They find higher survival rates

for minority businesses that derive 1-24 percent of their sales from state and local governments in cities

with programs with specific characteristics.[10] The results are mixed for minority firms that derive at least

25 percent of their sales from state and local governments.

The evidence on whether set-aside programs have increased the number of minority-owned

businesses is inconclusive. Also, there is no research on the effects of these programs on black

employment rates. To address these gaps in the literature, we examine whether the set-aside programs

initiated in many of the largest U.S. cities during the 1980s affected business ownership among black men

and employment rates.[11]

III. Data on City-Level Set-Aside Programs

A major limitation for previous research on set-aside programs has been finding accurate data on

program implementation dates. Because these programs are administered at the city level, there is no

centralized source of information. Further, the focus of the programs and offices running them have

changed over time, especially due to *Croson* (1989), making it difficult to determine when programs were

originally created. We conducted an extensive analysis of information from several sources to create a

novel dataset of city program initiation dates.

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[9] JCPES collected information on program dates, program assistance staffing, provision of capital assistance, bonding, downsizing of large procurement contracts, certification of minority business enterprises, penalties for violation of certification or program regulations, and treatment of brokers.

[10] Survival increases with programs that have a rigorous certification process; have a staff assigned to assist minority firms; routinely wave bonding requirements or provide bonding; or provide working capital assistance to minority firms receiving contracts.

[11] State-run programs are much smaller than city programs and most county programs follow city programs since targeted minorities live primarily in central cities (from discussions with Thomas Boston and Timothy Bates).

Our data on the years in which city programs were enacted come from two previously used

sources and two new sources. The first is the 1988 <u>Report on the Minority Business Enterprise Programs</u>

<u>of State and Local Governments</u> by the Minority Business Enterprise Legal Defense and Education Fund

(MBELDEF). Intended to represent a complete list of local affirmative action programs in existence as of

1988, the report had great difficulty finding information for all cities.[12] It contains program initiation

dates, authority, coverage, and percentage goals for most programs. These data were previously used in

Boston (1998). A second source is the report to the U.S. Department of Commerce Minority Business

Development Agency, <u>Assessment of Minority Business Development Programs</u>, by the Joint Center for

Political and Economic Studies (JCPES) in 1994. It contains detailed profiles on minority-business set-

aside programs in 28 large cities, including program initiation dates.[13]

Table A1 in the Appendix shows the program dates provided in MBELDEF (1988) and JCPES

(1994) for the 44 cities identifiable in the Current Population Survey (CPS) during our period of analysis.

Combined, these two sources provide program start dates for 33 of the 44 MSAs. For the eleven cities

without listed dates, we cannot rule out the presence of set-aside programs since these two sources are

incomplete. According to both sources, most city-level programs were created in the early to mid-1980s.

Fourteen of the twenty-one dates from MBELDEF are in the 1983-1985 period. Though more dispersed

throughout the 1980s, the JCPES dates are similarly concentrated.

The MBELDEF and JCPES program dates correspond for only four of the thirteen cities for

which both sources provide information, and in some cases there is a difference of several years. Due to

concerns over the reliability of these dates, we conducted an exhaustive search for additional information

on when set-aside programs were initiated in major U.S. cities.[14] First, we used Westlaw to search all

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[12] Data from other sources reveal that some cities with programs existing in 1988 are not listed in the MBELDEF report and that the listed starting dates for several programs are incorrect. A personal communication with Franklin M. Lee, Chief Counsel of MBELDEF, confirmed that the report is not a complete list of programs.

[13] Timothy Bates informed us that the original goal of JCPES (1994) was to get data for 50 cities, but some cities refused to answer, whereas others did not have programs.

[14] Additional complications exist with respect to the timing of the programs listed in the two sources. For the MBELDEF data, the date of the administrative order or resolution can often be an inaccurate measure of the date that the program started. In some cases, the actual program did not start for several years after the order/resolution. Thomas Boston found that the MBELDEF dates were wrong for Atlanta. Timothy Bates suggested similar problems with the JCPES data.

federal and state court cases that involved minority business programs in the 44 cities in our sample. From the court cases, which typically involved a lawsuit by a white-owned construction firm against the city's minority contracting program following *Croson*, we found program start dates for 14 cities. Information on other program aspects was limited and not consistent across cities.

Our final source came from personal correspondence with city officials who were involved in the set-aside programs or worked in the successor agencies to promote small business. We attempted to contact every city in our sample, and, over a two-year period, we discussed the programs with key administrators in more than 30 different cities either by phone or email. In each case, we spoke with current city employees or affiliates of a related business outreach program and conducted semi-structured interviews. During these conversations, the program dates from the other sources were cross-checked and additional documentation was requested when applicable.

Given the disagreement on program initiation dates between the two sources used in previous studies, these new data are essential to conducting a comprehensive and reliable study of city set-asides. With four sources of data, we can verify the dates with greater confidence, and the personal interviews provided valuable institutional detail on the programs. This allows us to conduct the first rigorous empirical investigation into the impact of contracting set-asides.

Table A1 also reports the program dates from these two new sources. While for the majority of cities there is agreement in the program dates across more than one source, there is disagreement for several others. Overall, most cities are reported as implementing set-aside programs in the early-to-mid-1980s. In the analysis that follows, we focus on two different estimation samples – the 17 MSAs with cross-validated dates and the 25 MSAs with either cross-validated dates or dates from only one source.[15] We drop the cities that we could not obtain program dates for or that had conflicting dates in multiple sources. The 17 cities with non-conflicting dates comprise the majority of the population in the 25-city

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[15] The results from the two different estimation samples are very similar. For the sake of brevity, in some tables we report only the results for the 25 MSA sample. The comprehensive results for the 17 MSA sample are shown in Chatterji, Chay and Fairlie (2012).

sample. In some analyses, we include the three additional MSAs in which city records indicate that no program was ever initiated and no other source suggests otherwise.

IV. The Current Population Survey and Trends in Self-Employment

The primary data source on self-employment and employment comes from the 1979 to 1989 Current Population Survey (CPS) Merged Outgoing Rotation Group (ORG) files. The CPS is conducted monthly by the U.S. Census Bureau and the Bureau of Labor Statistics, and represents the entire U.S. population with observations on over 130,000 people each month. The ORG files contain annual samples that are roughly three times larger than those from a monthly CPS. The large sample sizes are important since the group of interest, self-employed black men, is small relative to the population, and our analyses are disaggregated to the MSA-level. From these data we construct annual information on patterns of minority and non-minority self-employed business ownership rates from 1979 to 1989.

The main limitation of the ORG files is that they do not contain information on the performance of the business – for example, they contain information on workers' weekly wages, but not for the self-employed. The only alternative data source on minority businesses, the Survey of Minority-Owned Business Enterprises (SMOBE), contains information on annual sales, but it is only available every five years (1977, 1982, 1987), underwent major changes in sample definitions over time, and does not include a comparison group of white-owned firms.

The self-employment rate is defined as the ratio of the number of self-employed to the adult population. Self-employed workers are individuals who identify themselves as self-employed in their own not incorporated or incorporated business on the class of worker question, which refers to the job with the most hours during the reference week in the ORG files. This definition of self-employment includes all business owners, both employers and non-employers. We focus on the years 1979 to 1989 since they encompass the period when most major cities in the United States created set-aside programs. The period after 1989 is not studied since the 1989 *Croson* decision led to the suspension or dismantling of set-aside programs in several cities. We focus on the impact of the creation of the original set-aside programs prior to the numerous changes that occurred to programs after *Croson*. We also do not examine

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the more recent state initiatives, such as those in California and Washington, due to their potentially confounding effects on government employment (Fairlie and Marion 2010).

Our sample is restricted to non-Hispanic white and black men aged 20 to 64. We do not study women since they have substantially lower business ownership rates, and female business owners are less concentrated in the industries that are affected the most by contracting set-aside programs, such as construction.[16] Our reliance on self-reported business ownership status lessens concerns over the potential inclusion of "fronts" in which white, male-owned businesses include minority owners in name only to secure government contracts.

Since we analyze city-level changes, it is important to consistently match cities over time. In the ORG files, the 44 largest Metropolitan Statistical Areas (SMSAs) can be identified from 1979 to September 1985 based on their 1970 Census population size ranking (MSA identifiers). After 1985 the city coding scheme changed to include 252 Consolidated MSA (CMSA) ranking identifiers, some subdivided into as many as 12 Primary MSA (PMSA) ranking codes. The match of the 1986-1989 and 1979-1985 city codes were based on making the CMSA and PMSA rankings compatible with the MSA rankings. While the codes for later years represent smaller geographical units, they capture the same general areas, and we examined population totals to gauge the quality of the match. The resulting sample includes the 44 MSAs that can be consistently matched over the entire decade.

Table 1 presents summary information on the characteristics of all black and white men (Panel A) and self-employed men (Panel B) in the 1979 to 1989 ORG data. In the United States as a whole (columns 1a and b) and in the 44-MSA sample (columns 2a and 2b), black men (relative to white men) are much less likely to be self-employed, more likely to be unemployed or out of the labor force, and much more likely to reside in the central city. Relative to the full population, the self-employed are older, slightly better educated, and less likely to be central city residents. These patterns hold for the 25- and 17-MSA samples that are used in the analysis below. The 44-MSA sample captures about 50 percent of

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[16] For example, only 1.8 percent of black women are self-employed, and only 1.1 percent of self-employed black women are in the construction industry.

the total black population and 30 percent of the white population in the United States. The 17- and 25-MSA samples contain 60 and 70 percent of all black men in the 44-MSA sample.

Table 2 shows the distribution of self-employment across industries, by race. We categorize industries into two groups – those heavily affected (Panel A) and less affected (Panel B) by city government contracts, which is an important distinction in the analysis. To classify industries into most- and least-affected, we used three sources of information: Bates (1993, 1997) and MBELDEF (1988). Bates uses confidential Census business-level data to tabulate the fraction of firms in each industry selling goods and services to the government. He also details the industries targeted by minority city contracting according to his own surveys of programs and other sources. The MBELDEF report provides information on the targeted sectors for many cities.[17] Complemented by our conversations with program officers, we identified construction, professional services, transportation-communications-utilities, business services, repair services, and retail trade as the "most-affected" industries.

In Panels A and B, the industries are roughly ordered from top to bottom by how impacted by minority business set-asides they are likely to be. For both races, pluralities of self-employed business owners are in construction (18-20 percent). The industries containing the next most self-employed are professional services for whites and retail trade for blacks. Relative to blacks, the white self-employed are much more likely to be in professional services, finance-insurance-real estate, wholesale trade, and manufacturing. The black self-employed are more likely to be in transportation-communications-utilities, repair services, personal services, and entertainment-household services.

While fewer of the self-employed are in the "less affected" sector for both races, the black-white gaps in self-employment rates are large in both sectors, as are the employment rate gaps. For both races, the self-employed in the "less affected" sector are better educated than their counterparts in the "more affected" sector, and the employed are less likely to be in their twenties. In the more affected sector, the employed tend to be better educated than business owners (especially among blacks), while this pattern is

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[17] In the report, a large percentage of program coverage appears to target the construction sector.

reversed in less affected industries. Black business owners and employees in both sectors are much more likely to reside in the central city, and all of these patterns hold across the three MSA samples.

Before proceeding, we discuss self-employment and employment trends between 1979 and 1989. Panel A of Figure 1 plots the self-employment rates of white and black men in the entire United States and in the 44-MSA sample. While black self-employment rates in MSAs were stable from 1979 to 1983, they increased from 4.5 percent to over 5 percent in 1984 and remained at this higher level for the rest of the decade. The black self-employment gains in the mid-1980s in the country as a whole were driven entirely by the gains in the 44 MSAs. In the MSA-sample white self-employment rates also rose during the 1980s, but most of the increase had occurred by 1983.

Panel B presents employment-population rates by race. The employment rates of black and white men declined sharply between 1979 and 1983 in response to the two recessions and rebounded over the rest of the 1980s. The fall and rebound are much sharper for blacks, implying larger business cycle effects on black men, and most of the black rebound in the 44 MSAs occurred between 1985 and 1988, while the white rebound was largest from 1983 to 1984 in both samples. In the MSA-sample, white men experience both employment declines and self-employment growth between 1979 and 1983, suggesting that some of the self-employment increase was driven by worsening job prospects. This pattern does not hold for black men in the MSA sample, as their self-employment growth occurs after the employment losses and just precedes the largest employment gains.

The increase in black self-employment rates in MSAs roughly corresponds with when many cities initiated set-aside programs, while the white rate changes little after the early 1980s. Further, between 1984 and 1987 there is a significant narrowing of the black-white employment gap in U.S. cities, though this may be partially due to racial differences in cyclical sensitivity. Next, we rigorously analyze the correspondence in the location and timing of the black, relative self-employment (and employment) changes and the implementation of city-level minority business set-aside programs.

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V. Econometric Framework

We use two approaches to estimate the impact of the city set-aside programs. First, we contrast before-and-after changes in the black-white gaps in self-employment and employment rates between MSAs that initiate a program and those that do not. This is commonly referred to as difference-in-differences-in-differences (DDD) and requires the assumption that changes in the racial gaps in the cities without programs provide a valid counterfactual for what would have occurred in the program cities in the program's absence. The second approach uses the staggered timing of set-asides across cities in greater detail to implement an "event study" analysis, which estimates the precise timing of the changes in black relative outcomes. On one hand, this framework also allows us to examine pre-program trends in relative outcomes and test for potential endogeneity in the programs' timing, which is assumed away in the DDD framework. On the other hand, it does not allow for comparisons to cities that did not initiate a program.

For the DDD analysis, we estimate the following equation:

$$
\begin{aligned}
(1) \quad y_{ijt} = {}& \lambda_t^r + \alpha_j^r + X_{ijt}{}' \beta^r + \pi_1 Black_{ijt} + \pi_2 Treat_{ijt} + \pi_3 Black_{ijt} \cdot Treat_{ijt} + \pi_4 Post_{ijt} \\
& + \pi_5 Black_{ijt} \cdot Post_{ijt} + \pi_6 Treat_{ijt} \cdot Post_{ijt} + \theta \cdot Black_{ijt} \cdot Treat_{ijt} \cdot Post_{ijt} + u_{ijt}
\end{aligned}
$$

where y_{ijt} is an indicator variable equal to one if person i in city j is self-employed (or employed) in survey year t; λ_t^r are race-specific (r) year fixed effects that capture racial differences in sensitivity to the cycle; α_j^r are MSA-level fixed effects that vary by race; X_{ijt} is a vector of unrestricted dummy variables for age and education whose effects can vary by race (β^r); and u_{ijt} is an error term that is allowed to be heteroskedastic and clustered over time at the MSA-level. *Black*, *Treat* and *Post* are indicators equal to one if the observation is respectively black, located in an MSA with a set-aside program, and surveyed in a year after set-aside initiation. OLS and probit estimation of equation (1) lead to similar marginal probability effects, and the analyses use the CPS sampling weights.

The DDD parameter of interest in (1) is θ, which measures the difference in changes in black-white gaps between MSAs with and without set-aside programs, before-and-after program initiation. It provides a valid estimate of the set-asides' impact if there are no unmeasured factors that affected black men differently than whites, in the program cities relative to those without programs, before-and-after set-asides. The parameters π_4 and π_5 measure the before-and-after changes for white levels and black-white

differences in the cities without programs, and the parameter π_6 measures the before-and-after change for

white men in program cities relative to their counterparts in MSAs without programs. We examine the

sensitivity of the estimates to the inclusion of the covariates (X_{ijt}) as an indirect test of the underlying

identification assumption for DDD.

The key assumption of DDD is that there were no race-specific changes that differed between

cities with and without programs after program initiation. One test of this assumption is to examine

black-white trends in the outcomes preceding program initiation. The existence of systematic, pre-

program patterns could indicate that the programs' timing was endogenous to factors affecting post-

program outcomes. Since the programs were staggered across cities instead of starting in the same year, a

natural way to examine this type of non-randomness is to use an "event study" framework.

Here, we begin by estimating equations of the form:

$$(2) \qquad y_{ijt} = \lambda_t^r + \alpha_j + X_{ijt}'\beta^r + \sum_{t-s=-13}^{12} \gamma_{t-s} \cdot 1\big(t_{ij} - s_j = t - s\big) + \sum_{t-s=-13}^{12} \theta_{t-s} \cdot Black_{ijt} \cdot 1\big(t_{ij} - s_j = t - s\big) + u_{ijt}$$

where the indicator variables, $1(t_{ij} - s_j = t - s)$, equal one if the survey year for an individual in city j (t_{ij})

minus the year of program initiation in city j (s_j) equals a value between -13 and 12, which is the full

support of survey years relative to program initiation years in our sample. For example, 1989 is 12 years

after Washington, D.C. began its program, and 1979 is 13 years before New York City started its

program.[18] The event study year ($t - s$) is zero when the survey year equals the program initiation year

for the MSA that the person lives in. The race-by-year effects adjust for the fact that cities initiated

programs at different points in the business cycle and that cyclical effects may vary by race, while the

MSA effects adjusts for the composition of cities in the event study year.

Estimation of equation (2) provides an unrestricted description of the racial gaps in self-

employment (and employment), θ_{t-s}, in relation to the timing of city-specific set-aside programs. While

the estimates of θ_{t-s} will be based on a greater number of MSAs in event years near zero, they give

precise pictures of: i) the exact timing of any changes in black-white outcomes in relation to the year of

---

[18] Dropping New York City and Washington, D.C. from the analyses has little effect on the results. An MSA with set-asides initiation in 1985 contributes to the periods -6 to +4; an MSA with initiation in 1982 contributes to -3 to +7. The three MSAs that never had programs are not included in the primary event-study samples.

program initiation; and ii) whether there are systematic trends in racial gaps preceding program initiation. The former uses the specific timing of changes to test for program causality; the latter provides a test of endogeneity in the timing of the set-aside programs.

By estimating equation (2) we can plot a response function of program effects. This motivates fitting a more restrictive model that provides simple summaries of the magnitudes and statistical significance of the set-asides' impacts:

$$
\begin{aligned}
(3) \quad y_{ijt} &= \lambda_t^r + \alpha_j + X_{ijt}'\beta^r + \delta_1 \cdot (t-s) + \delta_2 \cdot Black_{ijt} \cdot (t-s) + \sum_{t-s=0}^{7} \gamma_{t-s} \cdot 1(t_{ij} - s_j = t-s) + \\
&\quad \sum_{t-s=0}^{7} \theta_{t-s} \cdot Black_{ijt} \cdot 1(t_{ij} - s_j = t-s) + u_{ijt}
\end{aligned}
$$

where $(t-s)$ is a trend variable in the event study years, and the "program effects" begin in event year zero. When we estimate (3) below, we restrict the sample of event years to be between -8 and $+7$, and instead of using unrestricted dummies for the post-program years, we use indicators for event periods of 0, (1, 2), (3, 4) and (5, 6, 7).[19] The error term (u_{ijt}) is allowed to be heteroskedastic and correlated over time at the MSA-level.

The parameter δ_1 measures the pre-program trend in white outcomes, while δ_2 measures the black-white difference in pre-program trends and provides a test for systematic changes in racial gaps before set-asides implementation. The program parameters θ_{t-s} measure the deviation of post-program racial gaps from their pre-program trends. The pre-program trends allow us to analyze whether the stationarity assumption implied by difference-in-differences (DD) holds. For example, if the racial gap is systematically narrowing before program initiation, then DD could estimate a positive program effect when the gap would have continued narrowing even in the program's absence. Similarly, if the gap is widening before program initiation, then DD could provide negative program effects simply due to a continuation of the pre-program trend toward greater racial inequality.

At the same time, the estimated program effects are based on the assumption that the pre-program trend in the racial gap would continue through the entire post-program period associated with the

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[19] These restrictions allow for a simpler summary of the results and do not affect the conclusions.

indicator variables. While this may be reasonable for the years soon after set-asides initiation, this is less likely to hold the further the period is from event year zero – for example, event years (5, 6, 7). These issues are discussed in detail when we present the results. The θ_{t-s} parameters provide "detrended" DD estimates of the program impacts – before-and-after comparisons of the black-white gap deviated from pre-program trends in the gap. Here, causality is assigned based on the exact timing of set-asides initiation and by using white men as the control group. Since cities that never had a program have unknown "event years", they cannot be used as control groups, which is a disadvantage of the event study approach relative to the DDD framework.

VI. Empirical Results on the Impact of Set-Asides

To start, we examine trends between 1979 and 1989 in the black-white gap in self-employment for three types of MSAs: i) those that initiated a set-aside program in 1985, a common start year; ii) those that never had a program or did not start one until after 1989; and iii) those that started a program by 1980. The last two groups are MSAs that were, respectively, never and always "treated" during the 1980s. Panel A of Figure 2 presents differences in the black-white self-employment gap for the three potential contrasts of these groups. The dashed line shows that MSAs with a program throughout the 1980s had black relative self-employment rates that were consistently 4-6 percentage points higher than MSAs without a program. Compared to cities that were "always treated", MSAs that started a program in 1985 had relative self-employment rates that were 4-6 points lower between 1979 and 1985 (bottom line). After 1985 there is a striking improvement in their relative rates, with no difference in the racial gaps between the two groups by 1988. The comparison of the 1985 initiators to the "never treated" cities yields similar findings – while the differences between the two groups are relatively small before 1985, the 1985 initiators experience a large narrowing of their gap after 1985.

The patterns strongly imply that set-asides initiation in 1985 resulted in significant gains in black self-employment. Panel B tests this further by analyzing these gains by industrial sector. The MSAs that started a program in 1985 are contrasted with those that always or never had a program during the 1980s. The middle line, for example, is roughly the population-weighted average of the bottom two lines in Panel

A. The plots are from regressions that adjust for race-by-MSA fixed effects, race-by-year fixed effects, year effects interacted with a 1985 program dummy, and unrestricted education and age indicators. The other lines decompose the overall self-employment DDs into the two industry groups discussed in Table 2 – industries likely to be most affected and least affected by city contracting set-asides (outcomes are indicators for self-employment in the respective sector). The estimated standard error bands are also shown and are corrected for heteroskedasticity and over-time clustering.

Nearly all of the black self-employment gains after 1985 occurred in the more affected sectors. The gains are significant in magnitude and statistically and are consistent with the hypothesis that set-aside programs were the cause.[20] While the DDs exhibit a downward trend between 1979 and 1982, they are relatively stable in the three years preceding set-asides initiation. At least for self-employment, the DDD framework in equation (1) may be appropriate for estimating the impact of set-asides.

Difference-in-Differences-in-Differences (DDD) Results

Table 3A presents the results of fitting equation (1) to the combination of two sets of DDD contrasts for self-employment: i) MSAs starting a program in 1982 compared to cities with no program change between 1979 and 1985 (never had a program or started one before 1979 or after 1985); and ii) MSAs initiating set-asides in 1985 compared to cities with no program change between 1982 and 1988 (never had one or started one before 1982 or after 1988). The sample pools three years each of before-and-after program data – 1979-1981 and 1983-1985 for the 1982 program contrasts and 1982-1984 and 1986-1988 for the 1985 program contrasts – with data from the program year excluded. Thus, the DDD estimates are based on before-and-after comparisons of three-year periods, and the inference is corrected for over-time clustering at the MSA-level. The pooled 1982 and 1985 set-asides sample provides before-and-after windows that span the 1980s, with the program cities never serving as control cities. The results are similar when we include likewise constructed 1983 and 1984 program samples, and the subsequent

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[20] We fit a DDD model to the 1982-1984 and 1986-1988 DDs using the same regression specification as in Panel B. The DDD estimates [t-ratios] for self-employment (per 100 men) are: 3.71 [2.55] for all industries; 3.00 [2.66] for the more affected sector; 0.40 [0.58] for the less affected sector.

event-study analysis uses all of the set-aside program dates.

Panels A and B of the table correspond to the 17-MSA and 25-MSA samples, and the three sets of columns present the results for all industries and the more and less affected sectors. For both samples, the DDD estimates ("B-W, treat-c" rows) show that the black self-employment rate rose three percentage points more in cities that initiated a program than in the control cities. These effects are highly significant statistically and imply that set-asides reduced the black-white self-employment gap by roughly one-third. White self-employment fell by 1.7 percentage points after program initiation ("White, treat-c"), suggesting that much of the black gain coincided with white losses – e.g., the before-and-after change in the overall self-employment rate is similar in the treatment and control cities. This seems plausible since, as we discuss below, set-aside programs did not change the total amount of city contract awards, but instead shifted a fixed proportion of them to black entrepreneurs and sub-contractors.

In the control cities, there are no before-and-after changes in white self-employment ("White, control") or in the black-white gap ("B-W, control"). Thus, the DDD effects are driven entirely by what is occurring in cities that initiated set-asides. Columns 2a-2d and 3a-3d show that nearly all of the black self-employment gains, and white losses, occurred in the most affected industries, with no changes occurring in the control cities.[21] The estimated effects are insensitive to controls for race-by-MSA fixed effects and unrestricted education and age indicators interacted with race.

The effect sizes are plausible. While data on contracts awarded to minority firms are unavailable for our full sample of MSAs, program information for Atlanta (Boston 1999) and Washington, D.C. (Rice 1991) is. Minority-owned businesses received $191 million between 1979 and 1989 from Atlanta's set-aside program, while procurement to minority-owned firms in Washington, D.C. was $170 million in 1985 alone. We apply these numbers to the DDD estimates to calculate an average contract amount for each new black business – the Atlanta and Washington, D.C. figures imply annual amounts between

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[21] The outcome is an indicator for being self-employed in the respective sector. The DDD effects in columns 2 and 3 will not add up to those in the first set of columns since this approach allows the effects of the additional controls to vary by sector. Another approach is to run the regressions in columns 1a-1d and include interactions of all of the effects of interest with sector dummies, which constrains the effects of the controls to be the same by sector. This leads to similar results.

$4,000 and $24,000, respectively. Average revenue for black-owned firms was $31,204 in 1982, with the majority coming from non-governmental sources (U.S. Census Bureau 1985, Bates and Williams 1995). Our approximate set-asides amounts per each new firm are within a range to provide black-owned businesses with a foothold in the market.

Growth in the number of black-owned businesses in the 1980s measured by the U.S. Census Bureau is also consistent with our estimated magnitudes from the CPS. The Survey of Minority Owned Businesses (SMOBE) indicates that the number of black-owned firms grew by 38 percent between 1982 and 1987 (from 308,260 to 424,165), and that the number of black-owned construction firms increased by 64 percent.

Table 3B is identical to Table 3A, except the outcome of interest is an employment indicator. The DDD estimates are similar in the two MSA-samples and insensitive to the set of controls included in the analysis. In columns 1a-1d, there is no relative employment gain for blacks in program cities relative to the control cities, implying that set-aside programs did not increase black employment rates even as their self-employment increased. This could happen if, for example, all of the newly self-employed black men were employed before program initiation and either hired blacks who were already employed or hired unemployed and out-of-the-labor-force blacks and whites at rates similar to their representation among the employed.

The other columns show, however, that this masks large differences in black employment changes between the industries most and least affected by set-asides. There is a significant, three percentage point gain in black relative employment in the more affected sector and a similar sized loss in the less affected sector. There are no before-and-after changes in either sector among whites in program or control cities or for blacks in the control cities – that is, the DDD employment estimates are driven entirely by changes among black men in program cities.

The new black businesses created in the more affected sector apparently increased the employment rate of blacks in more affected industries, but there was a corresponding decline in black employment in less affected industries. This could happen if, for example, the black employment gain in more affected industries came from hiring black men who were already employed in other industries.

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Evidence suggests, though, that this is not the case. In particular, the relative employment of black men was declining *before* these programs were initiated, and this decline was driven mostly by a growing racial gap in the *least affected* sector.

While the event study framework allows us to examine this in detail, DDD contrasts for the 1985 program cities are illustrative. In addition to constructing 1982-1984 and 1986-1988 before-and-after comparisons, we constructed 1979-1981 and 1983-1985 DDD contrasts. The latter tests whether outcomes were changing differently between the 1985 program and control cities before set-asides were adopted by shifting the DDD backwards – DDD models imply that there will be no pre-program effects.

We applied the regression specification in columns 1c, 2c, and 3c of Table 3 to the 1985 program sample and estimated both post-program and pre-program DDD effects. For self-employment, the post-program DDD effect is positive and significant (see footnote 20), and the pre-program DDD is small, negative and insignificant at the 10-percent level. For employment, the post-program DDD effects are zero, significant positive and significant negative for all industries, the more affected sectors, and less affected sectors, respectively (estimates similar to Table 3B). However, the pre-program DDD for all employment is negative (-3.31 percentage points) and significant (t-ratio of 2.32), and 70 percent of this decline occurred in the least affected sector (-2.36 points).

In the 1985 program cities, black relative employment fell in the less affected sector before the programs were initiated. This implies: i) the program timing was not exogenous with respect to the black employment changes occurring in less affected industries; and ii) using DDD models to estimate the employment effects in the less affected sector (and across all industries) may be inappropriate. We next implement an event study analysis that incorporates all of the set-aside program dates and estimates program effects that adjust for the race-specific trends that preceded program adoption.

Event Study Results

Figure 3 plots the black-white gaps (θ_{t-s}) in self-employment and employment from estimation of equation (2), along with their standard error bands. Panels A and B are based on the 17- and 25-MSA samples, respectively, and the estimates are from regressions that include race-by-year effects, MSA

effects and unrestricted indicators for education and age. The patterns are insensitive to excluding the education and age indictors or allowing their effects to vary by race. Recall that the x-axis (event year) is zero in the year the set-aside program started, with negative (positive) event years occurring before (after) program initiation. Also, the MSAs that never had a program are excluded from the sample since we cannot construct their event years.

In both samples, the self-employment gap is stable in the six years preceding program initiation (at about 6 percentage points), which suggests that the timing of the programs was exogenous with respect to racial trends in self-employment. The gap immediately falls by three percentage points after program adoption, and remains smaller in the years afterward. These patterns imply that set-asides caused a sharp increase in black-owned businesses and that a difference-in-differences (DD) framework is appropriate for estimating the self-employment program effects.

The employment gap, by contrast, systematically widened in the eight years preceding the programs; more so in the 17-MSA sample and in the four years before program adoption. Thus, the program dates are not exogenous vis-à-vis racial trends in employment – cities initiated programs after years of declining black employment. This makes sense as city set-asides were partly a response to the growing problem of black unemployment. However, this trend is immediately reversed after set-asides initiation, with the black-white employment gap narrowing over the rest of the post-program period. The timing of the trend reversal suggests that set-aside programs were the cause.

If the trend toward greater employment inequality would have continued in the absence of set-asides, then the estimated employment effects should be adjusted for the pre-program trends. This is not appropriate if one does not believe that the pre-program trends would have continued. In any case, the magnitude of the estimated employment effects will depend on how one deals with the relative black trend before program initiation.

We use equation (3) to estimate the magnitude and statistical significance of the program effects while adjusting for racial differences in the pre-program trends. Table 4 presents the results separately for self-employment and employment (first and second set of columns) and for the 17- and 25-MSA samples (Panels A and B). Recall that the sample contains the event period from eight years before to seven years

after program implementation, and therefore the pre-program trend is based on the eight years preceding the program.[22] There are highly significant gains in black self-employment after set-asides adoption of 2.5 to 3.5 percentage points, depending on the sample and post-program period. This implies that set-asides caused a 40-percent decline in the black-white gap. There is no racial difference in the pre-program trends, confirming the visual impression left by Figure 3, and the estimates are insensitive to controlling for education and age indicators or allowing their effects to differ by race. The programs' timing does not appear to be a response to changing conditions for black business ownership.

For employment, there are significant racial differences in the pre-program trend in both samples, with black men systematically losing ground relative to whites before program adoption. The estimated effects imply that, deviated from the race-specific trends, the employment gap narrowed by 3.5 and 6.5 percentage points in the periods 1-2 and 3-4 years after adoption in the 17-MSA sample. These figures are 3 and 4.5 points in the 25-MSA sample. As the qualitative findings are similar, we focus mostly on the larger 25-MSA sample from this point forward.[23]

It seems plausible that the trend toward greater employment inequality would not have immediately reversed itself in the absence of set-asides. Indeed, we show below that the racial difference in pre-program trends was driven primarily by an employment decline in the *least affected industries* and that this decline continued after program adoption. It is more dubious to expect that the pre-program trends would have continued for many years after adoption. In this case the employment estimates for 5-7 (and possibly 3-4) years after initiation could exaggerate the black employment gains from set-asides.[24]

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[22] The results are not sensitive to basing the pre-program trends on the six or four years preceding the program (instead of eight). See Chatterji, Chay and Fairlie (2012) for these results.

[23] We also performed the analyses in Tables 4 and 5 without CPS sample weights. The magnitudes change slightly, but the statistical significance and qualitative conclusions are nearly identical to the weighted results. Based on personal conversations with BLS staff, the use of CPS sample weights is preferred to make the results representative of the U.S. population (e.g., adjusts for oversampling of smaller states). The unweighted results for Table 4 are shown in Chatterji, Chay and Fairlie (2012).

[24] Not adjusting for pre-program trends has no impact on the self-employment results, but, unsurprisingly, a large effect on the employment results. For example, we fit DD specifications to the event study data using the periods one to four years before and after program initiation. In the 25-MSA sample, the DD estimates [t-ratio] for self-employment were 3.03 [4.20], 3.02 [3.73], 3.01 [3.49] in the three specifications. For employment, they were 0.73 [0.49], 0.73 [0.49], 0.55 [0.43]. If the year just before adoption is used as the baseline year, the employment effects increase to roughly 3 percentage points in the period 2-to-4 years after set-asides adoption.

Table A2 shows the robustness of the results across different samples of MSAs. We estimate equation (3) – controlling for race-by-year and MSA effects and race-specific education and age effects – but allow for unrestricted post-program effects instead of grouping them. Panel A shows the self-employment results for only the adoption year and the year after since the effect magnitudes are similar in the later years and for brevity. Panel B shows the employment effects for each post-program year. Column 1 contains the results from our primary sample of 25 MSAs, which can be compared to column 2c in Table 4. To this sample we cumulatively added: three MSAs that we are confident did not have programs (column 2); four MSAs with no evidence of a program, though this could not be confirmed (column 3); five MSAs with conflicting program dates across sources, so we assigned the dates from court cases or city records (column 4). The largest sample contains 38 out of the 44 MSAs identified in the CPS.[25] In column 5, we drop the four cities that initiated programs either before or after the 1979 to 1989 sample period (e.g., Washington, D.C., New York City) from the original 25-MSA sample.

In four of the five samples, the self-employment effect ranges from 3 to 3.5 percentage points. It falls to 2.2 points in the sample that includes MSAs with disagreement in their program dates, which is perhaps to be expected given the potential error in these dates. The employment effects are also similar across the samples. In four of the five samples, the black-white employment gap narrows by 3.9 to 5.5 points in years 2-to-4 after program initiation. The effects in the sample that includes the most MSAs are again smaller in magnitude (2.8-3.2 points).[26]

As noted above, the self-employment effects are plausible in magnitude and insensitive to controlling for pre-program trends. The employment effects fall in magnitude if pre-program trends are ignored (see footnote 22). To gauge the plausibility of the employment effect sizes, we presume that while the downward trend in black employment would have continued in the absence of set-asides, it would have stopped within three to four years. In Tables 4 and A2, this puts the black employment gain

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[25] The remaining six cities either have several conflicting dates or conflicting dates and no information from a court case or city record. For the MSAs without programs, we assigned 1979 to be most negative event year (-13).
[26] We also examined the sensitivity of the results to central city representation within MSAs. The results are unchanged in specifications that include only central city observations and in specifications where observations are weighted by the percentage of the MSA sample residing in the central city area.

at roughly 4 to 4.5 percentage points, while the self-employment gain is 3 to 3.3 points. This implies that in aggregate the new black employers hired four black men for every white who otherwise would not have been employed.[27]

These magnitudes seem plausible as: i) the non-employment rate of black men in the 25-MSA sample was 32 percent before the programs; and ii) we find below that the aggregate employment effects are small – i.e., since the 25-MSA sample is 15-percent black, the gain in the black employment rate coincided with a small decrease in the white rate. Also, the Characteristics of Business Owners (CBO) survey shows that new black business owners were vastly more likely to hire blacks than whites (U.S. Census Bureau 1997). For example, two-thirds of black-owned businesses with employees had workforces that were over 90 percent minority. Finally, from the information on set-aside amounts in Atlanta and Washington, D.C., we can calculate the contract dollars awarded for each new job created using an employment effect of 4.5 percentage points. These figures are $2,500 and $15,700 in city contracts per new job created in Atlanta and Washington, D.C., respectively. In 1982, the average payroll per employee in black-owned firms was $7,812 (U.S. Census Bureau 1985).

Industry Results

We next apply the event study framework to examine how the estimated effects differ by industrial sector. If the programs are the cause, there should be greater gains in black self-employment and employment in the industries that are most affected by city government contracts. Figure 4 displays the racial gaps (θ_{t-s}) in self-employment (Panel A) and employment (Panel B) from estimating equation (2) separately for the least and most affected industries. The analysis is based on the 25-MSA sample and includes the entire range of event years, though the plotted effects are for the period eight years before and after program initiation.[28] The regressions include race-by-year effects, MSA effects and unrestricted

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[27] This calculation assumes that 25 percent of the newly self-employed blacks were unemployed before. This would be true if, for example, the estimated black employment gain in the year of program initiation was driven by men who were not employed and became self-employed in response to the program.

[28] Eight years before (after) initiation is 1979 (1989) for set-asides started in 1987 (1981). Outcomes are indicators equal to one if the person is self-employed (or employed) in more affected (or less affected) industries.

education and age indicators. Recall that the most affected industries are dominated by construction, professional services, transportation, business services, repair services and retail trade.

In Panel A, nearly all of the post-program gains in black self-employment are in the most affected industries. In this sector, there is no trend in the racial gap before adoption and an immediate decrease in the gap of 2-2.5 percentage points after. In the less affected sector, there is little trend in the gap before adoption and a small reduction after. After set-asides initiation, the self-employment gap in the more affected sector converges to the smaller gap in the less affected sector.

In Panel B, the majority of post-program gains in black employment also are in the most affected sector. The employment gap widened in both sectors preceding program initiation, but the black losses are noticeably larger in the less affected sector. If set-asides were an endogenous response to declining black employment, these losses were mostly occurring in the industries least affected by city contracts. In the less affected sector, the black losses continue for two years after program adoption. By contrast, in the most affected industries black employment rises by three or more percentage points in the period 1-to-4 years after set-asides. At the same time the racial gap in less affected industries stabilizes and even narrows somewhat. This suggests that the black gains in the more affected sector are driven by those who would have been unemployed in the programs' absence and not simply by a reallocation of employed blacks from the least to most affected sectors. On net, the employment gap in the more affected sector converges to the smaller gap in the less affected sector after set-asides.

The patterns suggest: i) the finding of a black employment loss in less affected industries in Table 3B is due to trends that preceded set-asides; ii) it is appropriate to control for pre-program trends since they are driven mostly by employment changes in the least affected sector; and iii) set-asides allowed blacks who lost jobs in the less affected sector to find employment in the more affected sector, where there was significant growth in black-owned businesses.

Table 5 presents the "detrended" DD results from estimating equation (3) for self-employment (Panel A) and employment (Panel B) in the more affected (columns 1a-1e) and less affected (columns 2a-2e) sectors. We focus on the 25-MSA sample, and, as in Table 4, we include the event period from eight years before to seven years after program initiation. The post-program effect is based on grouping years

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1-to-4 after program adoption – an appropriate simplification given the patterns in Figure 4 – and the pre-program trend is again based on the eight years preceding the program.[29]

For self-employment, the post-program black gains occur entirely in the most affected sector, with the racial gap narrowing by 50 percent (2.5-2.7 percentage points). The estimates are insensitive to the controls, including the addition of race-specific MSA effects and race-specific time trends that vary across MSAs (columns c and e). There is little change in white self-employment rates in either sector, so the relative within-sector change is driven by black gains and not white losses. The evidence that set-asides increased the relative self-employment of black men is decisive, and these estimates are very similar to the DDD estimates in Table 3A.

The evidence on employment in the most affected sector is similarly robust. The black-white gap narrowed by four percentage points after set-asides, and this effect is insensitive to including race-specific time trends that vary by MSA, as well as the other controls. These gains coincided with white losses, as the white employment effects are negative, though not statistically significant. In the less affected sector, the employment effects depend on the specification. In columns 2a, b and d there are no employment losses for blacks once one adjusts for the pre-program trends. However, if race-by-MSA fixed effects and race-by-MSA time trends are included, the estimated black employment losses become significant and similar in magnitude to the DDD estimates in Table 3B.

It appears that there are complex, city-specific employment changes for blacks in less affected industries (e.g., manufacturing and government) that coincide with set-asides' adoption. Some of the black gains in the more affected sector may have come from those who were previously in less affected industries. In estimating the overall black employment effects, it is valid to control for pre-program trends if the blacks previously in the less affected sector would have been unemployed without set-asides, but instead found jobs in the more affected sector due to the increase in black-owned enterprises.

In analysis reported in Chatterji, Chay and Fairlie (2012), we probed the robustness of these findings to ignoring pre-program trends. Specifically, we used the event study data to construct contrasts

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[29] Separate indicators and interactions are included for event years 0, +5, +6, +7.

of the periods 1-to-4 years before and after program initiation without adjusting for pre-existing trends (i.e., DD estimates). In the more affected sector, the estimates for both outcomes were similar to those in Table 5 – i.e., controlling for pre-program trends has little effect on the results in the most affected sector. In the less affected sector, while the black self-employment effects are also similar to those in Table 5, the black employment effects become negative and economically large, though not statistically significant.

We also examined the sensitivity of the results to reducing the number of years used to estimate the pre-program trends and to constraining the post-program effect to be for years 1-to-3 after set-asides initiation. The estimated effects (shown in Chatterji, Chay and Fairlie 2012) are similar to those in Table 5 for both industrial sectors. In summary, the estimates in Tables 4 and 5 are similar to those in Table 3 with the exception that controlling for pre-program trends leads to net positive employment gains for blacks. This is because the black employment losses in the *least affected industries* after set-asides were just a continuation of pre-existing trends.

Black Gains by Skill Group and Central City Residence

Black youth unemployment reached crisis levels in the 1980s, especially in urban neighborhoods (Freeman and Holzer 1986). One goal of set-aside programs was to increase the number of black employers in central city areas in order to reduce joblessness among urban black men. We examine how the black gains from set-asides vary by skill group and central city residence using the DDD equations and samples underlying Table 3 – as we have shown, DDD seems appropriate for each outcome except for employment in the least affected sector. The outcome variables are indicators equal to one if the person is self-employed (or employed) and in the group of interest.

Table 6 shows the DDD estimates for self-employment (Panel A) and employment (Panel B) in the *most affected sector* for the 25-MSA sample. Each set of columns relates to different "skill" categories: i) at least 12 years of completed education and at least 30 years of age (older-skilled); ii) 12-or-more years of education and 20-to-29 years-old (young-skilled); iii) less than 12 years of education and 20-to-29 years-old (young-unskilled); and iv) below 12 years of education and 30-or-older (older-unskilled). The specifications are the same as those in columns c and d of Table 3.

The vast majority of the black self-employment gains in more affected industries are for the older-skilled, with the remainder accruing to the young-skilled. Among white men, there are significant self-employment losses for the older-skilled and no changes for any other group. The self-employment gains among better-educated black men (young and older) apparently came at the expense of losses for older, well-educated whites. In the control cities, there are no before-and-after changes in self-employment for either white or black men in any skill group. This supports the interpretation that set-asides caused a reallocation of self-employment between better-educated whites and blacks.

Table 6 also shows that, in the most affected sector, virtually all of the black employment gains are for the young-skilled. This evidence seems decisive since, in this skill group, there are no before-and-after changes for whites in the program cities or for either race in the control cities – i.e., the entire, highly significant, DDD effect is driven by increased hiring of young-skilled blacks in program cities. However, the gains among less-educated blacks in their twenties – an important focus of the interventions – are small; the changes in program cities are statistically similar to the marginally significant gains in the control cities. There is no evidence of a reallocation of employment between whites and blacks within the most affected sector.

Table 7 contains the DDD estimates in the most affected industries disaggregated by whether the person resides in the MSA's central city area or not. We show the results for the skill groups that dominate the black gains in Table 6 (for the specification in the b columns). For self-employment (columns 1a to 2b), the majority of the black gains among the older-skilled are for men who live outside of the central city, while most of the gains for the young-skilled are for central city residents. The white self-employment losses for the older-skilled are driven by those residing outside of the central city. Keep in mind, though, that the residence of the self-employed may not match where the business is located, particularly for the older self-employed.

For young-skilled employment (columns 3a, b), most of the gains are for black men who reside in the central city. For self-employment and employment, there are no differences in the estimated effects by central city status in any other skill group (results available from the authors). Set-asides appear to have mostly benefitted the self-employment of older-skilled black men living outside the central city and

the (self-employment and) employment prospects of better-educated blacks in their twenties living in the central city. The implication is that the new black-owned businesses in the most affected industries hired young, educated blacks residing in the central city.

To better understand the black employment losses occurring in the *least affected industries*, Table 8 presents the DDD employment estimates by skill group and central city residence in this sector. For the most part, the black employment losses are evenly distributed by age and education (columns 1a-1d) and largest in magnitude for the older-skilled. Columns 2a and 2b show, however, that there were large employment losses for blacks living in the central city area, and columns 3a-3d show that these losses were concentrated among the better-educated, both young and older.

One interpretation of these results is that set-asides simply shifted the employment of young-skilled blacks from the least affected to the most affected industries, as the gains and losses involve central city residents. Two patterns, though, imply that this is not the case: i) in the less affected sector there were large employment losses for older-skilled blacks in the central city, but no corresponding gains for this group in the more affected sector; and ii) in the less affected sector, there were large losses for better-educated, central-city blacks in the years *before* set-asides' adoption. We estimated DDD models of the "pre-program" changes (between 1979-1981 and 1983-1985) for the 1985 program cities and the MSAs that never had a program or did not adopt one until after the 1980s. Black employment in the least affected sector declined by four percentage points more in the program cities; and the entire loss occurred among the better-educated, with two-thirds of it going to black men in their twenties.

Set-asides were adopted during a period of falling, local labor demand in manufacturing and the public sector. These shocks disproportionately affected black men with at least a high school degree or its equivalent.[30] A question we cannot answer is what set-asides' impacts would have been if government and manufacturing employment had not fallen. Another key question is whether the better-educated,

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[30] Figure A1 presents event-study plots of the racial gaps in employment in the least affected industries and in manufacturing and government, which account for 44 and 22 percent of all black employment in the least affected sector. As was discussed, there were large black employment losses in the least affected sector before program adoption, and these losses continued for two years after adoption. Nearly all of this is driven by black losses in the public sector and manufacturing, with government employment staying permanently lower.

young blacks in the central city would have found employment in the absence of set-asides (and the resulting growth of black-owned businesses in the most affected sector) or whether their job losses in the least affected sector would have continued. If one believes the latter counterfactual, then it is appropriate to control for pre-program trends in estimating the effects on overall black employment. If not, then, at a minimum, set-asides led to an across-industry shift in the employment of young, better-educated blacks toward sectors in which black self-employment rates increased.

Aggregate Self-Employment and Employment

The evidence suggests that city set-asides led to a reallocation of self-employment from white to black men. Black self-employment gains coincided with white losses, particularly in the most affected industries (Table 3A) and among the older-skilled (Table 6). The evidence on employment reallocation is much weaker, though there may have been small losses for whites in the more affected sector (Tables 5). Set-asides may have caused a substitution of black-owned for white-owned businesses that were previously awarded city contracts. Conversely, if the black firms winning contracts were less efficient than the previous awardees, then the total number of businesses could increase as more would be needed to complete the government-financed projects. We provide suggestive evidence on these questions of substitution and efficiency by examining the city-wide impacts of set-asides.

We estimate the *aggregate* effects on self-employment and employment using equations similar to (2) and (3), but without race interactions and adjusted only for MSA fixed effects. Figure 5 plots the MSA-level, event-year effects for self-employment and employment and their standard error bands. Panel A shows that self-employment is modestly higher one-year after program adoption but then reverts to pre-program levels in event years 2-to-4. Panel B shows the self-employment effects for the most and least affected industries. Self-employment rates in the most affected sector increased slightly in the post-program period (relative to the pre-program trend), but the magnitudes are small and statistically insignificant.

In Panel A, MSA-wide employment increased after program initiation; employment rates are significantly higher 2-to-4 years after set-asides. However, since the reversal in the pre-program trend

begins a year before the programs, it is unclear whether the post-program aggregate gains are strictly due to set-asides. Panel C shows overall employment separately for the most and least affected industries. In the least affected sector, employment fell sharply in the years preceding set-asides and continued to fall after, though at a slightly slower rate. By contrast, employment is significantly higher in the most affected sector after set-asides, consistent with the possibility that the programs increased aggregate employment. But again the gains appear to start a year before program initiation, making it difficult to attribute them solely, or even primarily, to set-asides.

Set-asides seemed to have had no impact on city-wide self-employment and instead affected the racial composition of business owners. This is sensible as city records indicate that set-asides changed the share of city contracts awarded to minorities but not the total amount of procurement outlays (MBELDEF 1988). The evidence is inconsistent with the view that set-asides were grossly inefficient – if contracts went to less-productive black-owned businesses, cities would need to compensate by hiring more firms to complete the projects. Further contradicting this view, data from the *Statistical Abstracts of the United States* show that total city expenditures did not increase after set-asides (results available from authors).[31] The fact that overall employment increased also suggests that the new black business owners were no less productive than the whites they replaced, and that the (self-reported) black self-employment gains were not driven by "fronts" created to attract set-aside dollars (e.g., firms fraudulently changing the owner's identity).

Before concluding, we note that we found no evidence of other policy changes that coincided with the staggered timing of city set-aside programs. There were no conflating changes in important federal programs, such as unemployment insurance and welfare, and a detailed analysis of U.S. Census Bureau (2008) annual budget data for each city in our sample showed no significant changes in city expenditures. Kerr and Nanda (2009) find that the deregulation of branch banking led to significant growth in both entrepreneurship and business failures among new ventures. However, the timing of these

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[31] We used the annual publications of the *Statistical Abstracts of the United States* (1978, …, 1981, 1982-83, …, 1992-93) to examine city-level, general expenditures per-capita between the 1976 and 1990 fiscal years.

state-level deregulations do not match that of city-level set asides.[32] We also examined whether

nonrandom movement of blacks to the cities that created set-aside programs potentially contaminate our

results. However, we found no program effects when we estimated equations (2) and (3) using the total

black population as the dependent variable. The large and significant estimated effects of set-aside

programs on black self-employment do not appear to be driven by confounding factors such as migration,

other policy changes, or economic trends.

VII. Conclusion

This study quantified the impact of city set-aside programs – affirmative action policies that are

among the most significant, racially-based interventions since the 1960s. We construct a new database of

program dates that enables a more comprehensive and reliable analysis than previously possible. We find

that set-asides had a large and significant impact on African-American business ownership during the

1980s, with the black-white self-employment gap falling by three percentage points. These gains were

realized entirely in the industries targeted by set-asides and correspond with other information on the

growth in, and the set-aside amounts awarded to, black-owned businesses. The better-educated were the

primary beneficiaries, and the programs appear to have reallocated self-employment from white to black

men. There is no evidence that the new, black businesses were less productive than the white-owned

businesses that were replaced.

Consistent with black-owned firms hiring a disproportionate number of blacks, the racial gap in

employment fell roughly four percentage points after set-asides. These results, however, are sensitive to

beliefs about the continuation of pre-existing trends, as the programs were preceded by several years of

declining employment in the industries that *did not* benefit from the programs (e.g., manufacturing and

the public sector). Black employment rates in the sector most affected by set-asides are more stable in the

pre-program period. The effects in the most affected sector imply that set-aside programs had larger

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[32] For example, intrastate branch deregulation in Washington, D.C. and California occurred before 1970 and in 1976 for New York. Interstate deregulation occurred in 1985, 1987 and 1982, respectively. None of these years are close to the set-asides dates for any of the cities in these states.

impacts on black employment than affirmative action programs that explicitly set goals for minority

hiring in firms receiving federal contracts (as summarized in Leonard 1990).

Nationally, the relative self-employment rate of blacks declined in the 1970s but rebounded in the

1980s (Fairlie and Meyer 2000). Our estimates imply that in the absence of city set-aside programs, the

black self-employment rate would have continued to decline relative to the white rate during the 1980s.

The finding of increased black employment provides support for the view that growth in black businesses

may reduce black unemployment more than general economic development.

The results are also consistent with the existence of entry constraints that suppress black self-

employment. If liquidity constraints are especially binding for blacks – perhaps due to low levels of

wealth and lending discrimination – then set-aside programs can lead to a large increase in the number of

black-owned businesses. Black entrepreneurs facing financing constraints may have been able to more

easily borrow against the accounts receivable from government contracts or use the initial receipt of city

contracts to grow to a sustainable size. City set-asides may have also diminished other barriers arising

from consumer, supplier, and prime contractor discrimination.

There are several caveats to our conclusions and areas that merit further research. First, more

evidence is needed on the "first-stage" of set-aside programs – e.g., number and amounts of contract

awards. Similarly, an ideal analysis would exploit the variation across programs in goals, enforcement,

and contract amounts reserved for minority-owned businesses. This study implicitly treated the city

programs as being homogeneous. Also, we only estimated the programs' benefits (in terms of black self-

employment and employment) and provided no new evidence on their costs. While we found suggestive

evidence of minimal efficiency loss, future work should investigate the actual costs of projects. For

California highway contracts, Marion (2009) finds that minority entrepreneurs were no less productive

than white counterparts in the same location, but that those located in high minority areas faced higher

costs. Future analysis should also document the characteristics of the black businesses that city set-asides

facilitated – for example, revenue, size, survival rates, and employee information.

As with the recessions of the late 1970s and early 1980s, the "Great Recession" has highlighted

questions over government spending, small business development and unemployment. Our findings

imply that government contracts can have real impacts on the number of business owners and employment. Of great policy interest is the relative effectiveness of such demand-side stimulus compared to supply-side policies, such as lower tax and interest rates, in increasing employment during a downturn. With specific respect to black employment, the impact of different policies further depends on the relative importance of place (urban "enterprise zones") and race (set-asides) in business formation and hiring decisions (e.g., Hellerstein, Neumark and McInerney 2008). Finally, as underscored in some recent contexts (rebuilding after Hurricane Katrina, Iraq reconstruction), the ex-ante award of government contracts may be neither competitive nor efficient. Our study indicates that how these contracts are allocated could have significant consequences for both equity and local economic development.

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Table 1: Summary statistics for black and white men aged 20-64 in 1979 to 1989 CPS-ORG data

	Entire United States		MSA identified in CPS		25-MSA sample		17-MSA sample	
	Black	White	Black	White	Black	White	Black	White
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
A. All men								
Age	37.4	39.3	37.4	39.3	37.5	39.3	37.6	39.4
Under 30 years old (%)	33.9	27.8	33.7	28.0	33.7	28.0	33.5	27.6
Education	11.7	13.0	12.1	13.5	12.0	13.5	12.1	13.6
HS grad or more (%)	67.4	83.0	71.9	86.9	71.4	87.0	71.4	87.1
Unemployed (%)	10.91	4.66	11.11	4.24	11.25	4.24	11.57	4.25
Out of the labor force (%)	16.46	9.88	16.76	9.06	17.43	9.35	18.20	9.51
Self-employed (%)	4.64	14.98	4.83	13.56	4.69	13.17	4.67	13.02
Central city resident (%)	51.2	19.6	70.5	28.4	73.1	29.4	74.2	29.5
Number of observations	111,142	1,115,886	56,433	341,808	39,520	214,342	33,689	171,455
B. Self-employed men								
Age	42.9	43.1	42.3	42.9	42.1	42.8	42.2	42.9
Under 30 years old (%)	13.1	13.6	13.7	13.6	14.5	13.7	13.8	13.5
Education	12.0	13.4	12.5	14.1	12.5	14.1	12.6	14.1
HS grad or more (%)	67.1	84.3	74.3	88.9	74.1	88.7	75.0	89.0
Incorporated (%)	13.2	27.0	16.1	34.9	16.0	34.7	16.8	35.0
Central city resident (%)	48.3	16.5	65.6	26.8	67.5	27.9	68.0	27.9
Number of observations	5,321	177,348	2,805	46,277	1,927	28,295	1,653	22,392

Notes: Data come from the 1979 to 1989 Merged Outgoing Rotation Groups files of the Current Population Survey, and are limited to observations that are aged 20-64; are black or non-Hispanic white men; and have one or more years of potential experience (age – education – 6). The statistics are weighted by the CPS sample weights. The samples underlying each set of columns are explained in the text and in the Appendix table.

Table 2: Distribution of self-employment across industries and other characteristics,
men aged 20-64 in 1979 to 1989 CPS-ORG data

	MSA identified in CPS		25-MSA sample		17-MSA sample	
	Black	White	Black	White	Black	White
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
A. More affected industries						
Percent of all self-employment						
Construction	20.33	18.35	18.49	18.08	18.33	17.11
Professional services	8.73	16.40	8.55	16.88	9.02	17.40
Transport, communicat, and utilities	11.37	3.92	12.61	4.02	12.41	3.91
Business services	7.77	6.81	8.61	6.88	7.79	7.11
Repair services	9.88	5.51	9.87	5.43	10.09	5.39
Retail trade	13.14	14.30	14.06	14.55	14.74	14.96
Self-employed traits						
Ratio to population (%)	3.44	8.85	3.38	8.67	3.38	8.58
Under 30 years old (%)	13.3	14.3	14.2	14.4	13.5	13.8
HS grad or more (%)	73.4	88.0	73.8	87.9	73.8	88.2
Central city reside (%)	66.8	27.1	69.6	28.4	70.3	28.5
Employed traits						
Ratio to population (%)	40.0	44.8	40.0	45.1	39.4	44.7
Under 30 years old (%)	33.3	30.1	32.7	30.0	32.3	29.3
HS grad or more (%)	77.8	88.5	77.6	88.7	78.1	89.0
Central city reside (%)	67.8	28.7	70.4	30.1	70.6	30.1
B. Less affected industries						
Percent of all self-employment						
Finance, insurance, and real estate	4.27	7.49	3.50	7.38	3.67	7.43
Wholesale trade	3.14	6.91	2.88	6.80	2.78	6.81
Personal services	5.04	2.88	5.17	2.92	5.18	2.99
Entertainment and HH services	5.23	3.08	5.30	3.16	5.80	3.20
Manufacturing	2.29	7.18	2.42	6.86	2.56	6.99
Agriculture	4.10	3.64	3.68	3.56	2.72	3.29
Mining, forest, fishery	0.18	0.55	0.18	0.45	0.22	0.34
Public administration	0.00	0.03	0.00	0.04	0.00	0.06
Self-employed traits						
Ratio to population (%)	1.17	4.31	1.08	4.10	1.07	4.05
Under 30 years old (%)	14.9	12.2	15.7	12.4	15.1	12.6
HS grad or more (%)	80.0	92.0	78.7	91.6	82.5	91.7
Central city reside (%)	61.1	25.8	60.7	26.5	59.9	26.2
Employed traits						
Ratio to population (%)	32.1	41.9	31.3	41.4	30.8	41.5
Under 30 years old (%)	28.4	24.9	28.2	24.8	26.8	24.4
HS grad or more (%)	77.3	89.5	76.5	89.5	76.7	89.5
Central city reside (%)	66.8	26.2	69.5	26.7	70.9	26.7
Percent of self-employ w/ missing ind codes	4.52	2.95	4.66	2.99	4.70	3.01

Notes: See notes to Table 1. Data come from the 1979 to 1989 Merged Outgoing Rotation Groups files of the Current Population Survey, and are limited to observations that are aged 20-64; are black or non-Hispanic white men; and have one or more years of potential experience (age – education – 6). The statistics are weighted by the CPS sample weights.

Table 3A: Change in self-employment rates before-and-after set-aside program initiation
[absolute value of t-ratios]

	All industries				More affected industries				Less affected industries			
	(1a)	(1b)	(1c)	(1d)	(2a)	(2b)	(2c)	(2d)	(3a)	(3b)	(3c)	(3d)
A. 17-MSA sample												
Post-program effects												
B-W, treat-c	2.78***	3.06***	2.75***	2.84***	2.71***	2.93***	2.76***	2.80***	-0.05	0.01	-0.13	-0.07
	[2.87]	[3.37]	[2.97]	[3.11]	[3.56]	[4.04]	[3.73]	[3.87]	[0.13]	[0.04]	[0.31]	[0.16]
White, treat-c	-1.58***	-1.79***	-1.72***	-1.73***	-1.38***	-1.48***	-1.41***	-1.41***	-0.08	-0.17	-0.16	-0.16
	[3.51]	[4.48]	[4.74]	[4.71]	[3.60]	[4.10]	[4.04]	[4.00]	[0.24]	[0.59]	[0.59]	[0.57]
B-W, control	0.07	-0.12	-0.07	-0.08	0.11	0.06	0.07	0.06	-0.05	-0.19*	-0.15	-0.16*
	[0.09]	[0.54]	[0.31]	[0.36]	[0.22]	[0.29]	[0.36]	[0.32]	[0.16]	[1.97]	[1.73]	[1.93]
White, control	0.04	0.27	0.26	0.26	0.15	0.11	0.12	0.12	-0.08	0.20*	0.18*	0.17*
	[0.05]	[1.11]	[1.07]	[1.05]	[0.31]	[0.64]	[0.70]	[0.69]	[0.27]	[1.94]	[1.89]	[1.88]
B. 25-MSA sample												
Post-program effects												
B-W, treat-c	3.32***	3.39***	3.13***	3.25***	2.82***	2.93***	2.82***	2.87***	0.36	0.33	0.19	0.27
	[3.36]	[3.80]	[3.33]	[3.50]	[3.75]	[4.31]	[4.00]	[4.21]	[0.77]	[0.74]	[0.39]	[0.58]
White, treat-c	-1.67***	-1.75***	-1.76***	-1.77***	-1.28***	-1.29***	-1.28***	-1.29***	-0.28	-0.34	-0.35	-0.35
	[3.19]	[4.38]	[4.68]	[4.64]	[2.86]	[3.59]	[3.93]	[3.93]	[0.90]	[1.14]	[1.16]	[1.15]
B-W, control	-0.17	-0.08	-0.04	-0.05	-0.09	0.04	0.03	0.02	-0.09	-0.13	-0.09	-0.11
	[0.22]	[0.36]	[0.19]	[0.26]	[0.15]	[0.20]	[0.14]	[0.13]	[0.33]	[1.38]	[0.99]	[1.22]
White, control	0.15	0.15	0.16	0.16	0.24	0.04	0.07	0.07	-0.08	0.15	0.13	0.12
	[0.19]	[0.62]	[0.65]	[0.64]	[0.48]	[0.25]	[0.39]	[0.39]	[0.28]	[1.49]	[1.39]	[1.38]
Race-year FE's	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Race-MSA FE's		Y	Y	Y		Y	Y	Y		Y	Y	Y
Educ, age FE's			Y	Y			Y	Y			Y	Y
Race-educ, race-age FE's				Y				Y				Y

Notes: Dependent variable is equal to one if person reports being self-employed and zero otherwise. Estimated effects are from a linear probability model. Probability effects derived from a probit model are similar. There are two sets of before-and-after comparisons in the sample: i) cities with a set-aside program begun in 1982 compared to control cities who either had no program, a program before 1979, or a program after 1985; and ii) cities with a set-aside program begun in 1985 compared to control cities who either had no program, a program before 1982, or a program after 1988. The sample is further limited to three years before and after program initiation – 1979 to 1985 (1982 to 1988) for the 1982 (1985) set-aside sample – and excludes data from the program year. Estimated sampling errors are corrected for heteroskedasticity and over-time clustering at the MSA level. Education and age controls are unrestricted indicator variables. Results including unrestricted interactions between education and age are similar. Regressions are weighted by the CPS sample weights. The 17-MSA and 25-MSA samples contain 141,286 and 154,454 observations, respectively. See Table 2 for the listing of industries in the "more affected" and "less affected" industry groups.
***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively.

Table 3B: Change in employment rates before-and-after set-aside program initiation
[absolute value of t-ratios]

	All industries				More affected industries				Less affected industries			
	(1a)	(1b)	(1c)	(1d)	(2a)	(2b)	(2c)	(2d)	(3a)	(3b)	(3c)	(3d)
A. 17-MSA sample												
Post-program effects												
B-W, treat-c	-0.12	0.13	-0.70	-1.00	3.28**	3.36***	3.28**	3.03**	-3.40**	-3.23**	-3.98***	-4.03***
	[0.08]	[0.11]	[0.75]	[1.18]	[2.66]	[2.81]	[2.61]	[2.43]	[2.17]	[2.31]	[2.85]	[2.82]
White, treat-c	0.04	0.29	0.64	0.64	0.18	0.30	0.48	0.46	-0.14	-0.01	0.16	0.18
	[0.06]	[0.55]	[1.10]	[1.11]	[0.28]	[0.53]	[0.82]	[0.76]	[0.20]	[0.01]	[0.22]	[0.24]
B-W, control	-1.68	-0.81	-0.62	-0.59	-0.70	-0.48	-0.49	-0.47	-0.99	-0.33	-0.13	-0.12
	[1.22]	[1.29]	[1.37]	[1.35]	[0.93]	[0.88]	[0.91]	[0.89]	[0.99]	[0.58]	[0.26]	[0.25]
White, control	1.42	0.17	0.16	0.17	0.62	0.06	0.13	0.14	0.80	0.12	0.03	0.03
	[1.00]	[0.53]	[0.52]	[0.54]	[0.63]	[0.29]	[0.66]	[0.68]	[0.90]	[0.40]	[0.12]	[0.11]
B. 25-MSA sample												
Post-program effects												
B-W, treat-c	-0.10	-0.01	-0.60	-0.96	3.01**	2.92***	2.97**	2.63**	-3.11	-2.93**	-3.57**	-3.58**
	[0.06]	[0.01]	[0.66]	[1.16]	[2.60]	[2.68]	[2.63]	[2.32]	[1.70]	[2.13]	[2.59]	[2.54]
White, treat-c	0.24	0.45	0.69	0.69	-0.25	-0.15	-0.02	-0.04	0.50	0.61	0.71	0.73
	[0.36]	[0.75]	[1.13]	[1.14]	[0.36]	[0.25]	[0.03]	[0.06]	[0.54]	[0.69]	[0.84]	[0.85]
B-W, control	-1.73	-0.81	-0.66	-0.62	-0.86	-0.54	-0.59	-0.54	-0.87	-0.27	-0.07	-0.07
	[1.34]	[1.41]	[1.51]	[1.49]	[1.14]	[0.99]	[1.04]	[1.00]	[0.68]	[0.48]	[0.12]	[0.14]
White, control	1.25	0.22	0.18	0.18	0.76	0.11	0.18	0.19	0.49	0.11	0.00	-0.01
	[1.01]	[0.69]	[0.62]	[0.63]	[0.77]	[0.53]	[0.83]	[0.85]	[0.42]	[0.42]	[0.00]	[0.03]
Race-year FE's	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Race-MSA FE's		Y	Y	Y		Y	Y	Y		Y	Y	Y
Educ, age FE's			Y	Y			Y	Y			Y	Y
Race-educ, race-age FE's				Y				Y				Y

Notes: See notes to Table 3A. Dependent variable is equal to one if the person reports being employed and zero otherwise.
***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively.

Table 4: Changes in black-white self-employment and employment rate gaps after set-aside initiation,
relative to pre-program trends
[absolute value of t-ratios]

	Self-employment			Employment		
	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)
A. 17-MSA sample						
Post-program B-W diffs						
Year 0	2.62***	2.69***	2.58***	1.70	1.33	1.26
	[3.58]	[3.74]	[3.44]	[0.91]	[0.67]	[0.72]
Years 1-2	2.47**	2.72***	2.56**	3.83**	3.63**	3.45**
	[2.50]	[3.01]	[2.62]	[2.49]	[2.09]	[2.09]
Years 3-4	2.36**	2.76**	2.66**	6.62**	6.62**	6.33**
	[2.04]	[2.37]	[2.11]	[2.63]	[2.52]	[2.69]
Years 5-7	2.94**	3.55**	3.44**	8.37**	8.89**	8.40**
	[2.12]	[2.50]	[2.25]	[2.46]	[2.63]	[2.69]
Pre-program						
B-W trend diff	0.077	-0.002	0.022	-0.889***	-0.863***	-0.855***
	[0.57]	[0.01]	[0.15]	[4.13]	[3.42]	[3.74]
White trend	-0.165	-0.120	-0.122	0.113	0.131	0.125
	[1.53]	[0.97]	[0.97]	[0.82]	[0.97]	[0.92]
B-W difference	-6.94***	-5.54***		-17.17***	-13.94***	
	[7.63]	[6.60]		[10.48]	[8.59]	
B. 25-MSA sample						
Post-program B-W diffs						
Year 0	3.14***	3.22***	3.19***	0.68	0.29	0.19
	[4.59]	[4.68]	[4.55]	[0.44]	[0.18]	[0.13]
Years 1-2	2.26**	2.53***	2.48***	2.94**	2.81**	2.61**
	[2.39]	[2.90]	[2.69]	[2.38]	[2.04]	[2.00]
Years 3-4	2.93***	3.33***	3.29***	4.70**	4.57**	4.32**
	[2.65]	[2.95]	[2.76]	[2.26]	[2.07]	[2.19]
Years 5-7	2.85**	3.46***	3.41**	5.89**	6.32**	5.97**
	[2.34]	[2.68]	[2.47]	[2.00]	[2.18]	[2.23]
Pre-program						
B-W trend diff	0.081	0.006	0.018	-0.587**	-0.558**	-0.543**
	[0.64]	[0.04]	[0.14]	[2.32]	[2.08]	[2.17]
White trend	-0.174*	-0.125	-0.127	0.110	0.130	0.125
	[1.79]	[1.14]	[1.13]	[0.91]	[1.08]	[1.03]
B-W difference	-7.14***	-5.82***		-15.57***	-12.42***	
	[8.16]	[6.81]		[8.90]	[7.24]	
Race-year FE's	Y	Y	Y	Y	Y	Y
MSA FE's	Y	Y	Y	Y	Y	Y
Educ, age FE's		Y	Y		Y	Y
Race-educ, race-age FE's			Y			Y

Notes: Sample is limited to observations that are no more than eight years before and seven years after the initiation year of a city's set-aside program. Pre-program trend based on data one-to-eight years before program initiation. Estimated sampling errors are corrected for heteroskedasticity and over-time clustering at the MSA level. Regressions are weighted by the CPS sample weights. The 17-MSA and 25-MSA samples contain 175,660 and 223,765 observations, respectively.
***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively.

Table 5: Average change in self-employment and employment rates one-to-four years after program initiation, relative to pre-program trends
[absolute value of t-ratios]

	More affected industries					Less affected industries				
	(1a)	(1b)	(1c)	(1d)	(1e)	(2a)	(2b)	(2c)	(2d)	(2e)
A. Self-employment										
Post-program effect (yrs 1-4)										
Black-white	2.47***	2.70***	2.72***	2.70***	2.68***	0.14	0.17	-0.30	0.13	-0.33
	[3.06]	[3.62]	[2.78]	[3.50]	[2.69]	[0.28]	[0.34]	[0.55]	[0.26]	[0.61]
White	-0.51	-0.48	0.02	-0.49	0.02	-0.10	-0.09	-0.16	-0.09	-0.16
	[1.02]	[0.97]	[0.04]	[0.97]	[0.04]	[0.47]	[0.40]	[0.68]	[0.41]	[0.67]
Pre-program										
B-W trend diff	-0.048	-0.099		-0.093		0.113*	0.104		0.111*	
	[0.45]	[0.95]		[0.87]		[1.69]	[1.58]		[1.73]	
White trend	-0.129*	-0.100		-0.103		-0.101***	-0.090***		-0.091***	
	[1.74]	[1.28]		[1.30]		[4.75]	[4.19]		[4.14]	
B-W difference	-4.80***	-4.16***				-2.16***	-1.43***			
	[6.04]	[5.48]				[4.75]	[3.23]			
B. Employment										
Post-program effect (yrs 1-4)										
Black-white	4.15**	4.27**	3.96**	4.02**	3.87**	-0.63	-0.86	-3.22**	-0.86	-3.20**
	[2.27]	[2.13]	[2.19]	[2.12]	[2.15]	[0.41]	[0.57]	[2.21]	[0.57]	[2.24]
White	-1.31	-1.17	-1.06	-1.16	-1.08	0.46	0.71	0.98	0.72	0.99
	[1.42]	[1.28]	[1.43]	[1.28]	[1.48]	[0.58]	[0.88]	[1.42]	[0.89]	[1.44]
Pre-program										
B-W trend diff	-0.403*	-0.403		-0.390		-0.137	-0.110		-0.104	
	[1.76]	[1.65]		[1.54]		[0.57]	[0.49]		[0.45]	
White trend	-0.209	-0.173		-0.171		0.232	0.226		0.222	
	[1.45]	[1.15]		[1.17]		[1.67]	[1.60]		[1.55]	
B-W difference	-9.40***	-8.77***				-6.08***	-3.58**			
	[5.49]	[5.63]				[3.89]	[2.30]			
Race-year FE's	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
MSA FE's	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Educ, age FE's		Y	Y	Y	Y		Y	Y	Y	Y
Race-educ, race-age				Y	Y				Y	Y
Race-MSA FE's			Y		Y			Y		Y
Race-MSA trends			Y		Y			Y		Y

Notes: See notes to Table 4. Based on 25-MSA sample and contains 223,765 observations.
***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively.

Table 6: Change in self-employment and employment rates before-and-after set-aside program initiation,
in more affected industries by skill group
[absolute value of t-ratios]

	Older, skilled Age 30+, Educ 12+		Young, skilled Age 20-29, Educ 12+		Young, unskilled Age 20-29, Educ<12		Older, unskilled Age 30+, Educ<12	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
A. Self-employment								
Post-program effects								
B-W, treat-control	1.93***	2.02***	0.62**	0.58**	-0.05	-0.05	0.31	0.32
	[4.33]	[4.76]	[2.60]	[2.56]	[0.56]	[0.58]	[1.43]	[1.62]
White, treat-control	-1.08***	-1.08***	-0.14	-0.13	0.03	0.03	-0.09	-0.11
	[3.70]	[3.76]	[0.80]	[0.79]	[0.31]	[0.29]	[1.01]	[1.27]
B-W, control	-0.09	-0.08	0.02	0.03	-0.01	-0.01	0.11	0.09
	[0.60]	[0.54]	[0.26]	[0.29]	[0.31]	[0.44]	[1.37]	[1.18]
White, control	0.04	0.04	0.04	0.04	0.02	0.02	-0.04	-0.03
	[0.28]	[0.25]	[0.81]	[0.82]	[1.00]	[1.01]	[0.99]	[0.91]
B. Employment								
Post-program effects								
B-W, treat-control	0.08	0.06	2.89***	2.59***	0.13	0.16	-0.13	-0.17
	[0.09]	[0.06]	[5.45]	[5.45]	[0.42]	[0.54]	[0.17]	[0.24]
White, treat-control	-0.42	-0.42	0.09	0.09	0.03	0.02	0.29	0.27
	[0.66]	[0.66]	[0.23]	[0.26]	[0.15]	[0.14]	[1.57]	[1.45]
B-W, control	-0.29	-0.28	-0.13	-0.08	0.21*	0.20*	-0.39	-0.39
	[0.85]	[0.85]	[0.74]	[0.47]	[1.90]	[1.87]	[1.10]	[1.09]
White, control	0.33	0.33	-0.09	-0.08	0.01	0.01	-0.08	-0.07
	[1.18]	[1.18]	[0.63]	[0.58]	[0.25]	[0.26]	[0.74]	[0.68]
Race-year FE's	Y	Y	Y	Y	Y	Y	Y	Y
Race-MSA FE's	Y	Y	Y	Y	Y	Y	Y	Y
Educ, age FE's	Y	Y	Y	Y	Y	Y	Y	Y
Race-educ, race-age		Y		Y		Y		Y

Notes: See notes to Tables 3A and B. Results based on 25-MSA sample.
***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively.

Table 7: Change in self-employment and employment rates before-and-after set-aside program initiation,
in more affected industries by central city residence
[absolute value of t-ratios]

	Self-employment in more affected industries				Employment in more affected industries	
	Older, skilled Age 30+, Educ 12+		Young, skilled Age 20-29, Educ 12+		Young, skilled Age 20-29, Educ 12+	
	Central city reside	Outside central city	Central city reside	Outside central city	Central city reside	Outside central city
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Post-program effects						
B-W, treat-control	0.65*	1.38***	0.42***	0.15	1.85***	0.73
	[1.71]	[2.70]	[3.80]	[1.13]	[4.05]	[1.03]
White, treat-control	-0.16	-0.92**	-0.05	-0.09	0.43	-0.34
	[0.65]	[2.59]	[0.51]	[0.87]	[1.10]	[0.99]
B-W, control	-0.05	-0.03	0.01	0.01	-0.28	0.21
	[0.41]	[0.25]	[0.24]	[0.16]	[1.14]	[0.97]
White, control	-0.04	0.08	-0.00	0.04	0.15	-0.23*
	[0.41]	[0.73]	[0.06]	[0.93]	[1.20]	[1.77]
Race-year FE's	Y	Y	Y	Y	Y	Y
Race-MSA FE's	Y	Y	Y	Y	Y	Y
Educ, age FE's	Y	Y	Y	Y	Y	Y
Race-educ, race-age	Y	Y	Y	Y	Y	Y

Notes: See notes to Tables 3A and 3B. Results based on 25-MSA sample.
***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively.

Table 8: Change in employment rates before-and-after set-aside program initiation, in less affected industries
[absolute value of t-ratios]

	Employment in less affected industries									
	By age and education				Central city vs. outside		By age and education for central city residents			
	Older, skilled	Young, skilled	Young, unskilled	Older, unskilled	Central city reside	Outside central city	Older, skilled	Young, skilled	Young, unskilled	Older, unskilled
	(1a)	(1b)	(1c)	(1d)	(2a)	(2b)	(3a)	(3b)	(3c)	(3d)
Post-program effects										
B-W, treat-control	-1.61	-1.07	-0.31	-0.60	-4.10**	0.51	-1.42*	-1.85***	-0.19	-0.63
	[1.50]	[1.28]	[0.85]	[1.20]	[2.29]	[0.44]	[1.72]	[3.07]	[0.64]	[1.09]
White, treat-control	0.75	-0.02	0.21	-0.21	0.58	0.15	0.24	0.10	0.10	0.13
	[1.30]	[0.05]	[1.30]	[0.76]	[0.71]	[0.13]	[0.38]	[0.36]	[1.52]	[0.47]
B-W, control	0.66	-0.61	-0.06	-0.05	0.36	-0.44	0.75	-0.29	-0.10	0.00
	[1.48]	[1.69]	[0.57]	[0.23]	[0.70]	[0.82]	[1.61]	[1.29]	[1.13]	[0.00]
White, control	-0.18	0.16	0.03	-0.02	0.14	-0.15	0.03	0.05	0.00	0.05
	[0.80]	[0.86]	[0.74]	[0.16]	[0.68]	[0.43]	[0.17]	[0.64]	[0.21]	[0.77]
Race-year FE's	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Race-MSA FE's	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Educ, age FE's	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Race-educ, race-age	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: See notes to Table 3B. Results based on 25-MSA sample.
***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively.

Table A1: Dates of Set-Aside programs in United States cities from four different sources

Name of MSA	CPS Rank of MSA	MBELDEF (1988)	JCPES (1993) Ordinance	JCPES (1993) Enacted	City record	Court case	Assigned year of Set-aside program
Akron, OH	49	1984	---	---	---	---	1984[b]
Albany, NY	46	1984	---	---	1984	---	1984[a]
Anaheim, CA	19	1985	---	---	No program	---	---
Atlanta, GA	21	1982	1970s	1991	1975	1982	---
Baltimore, MD	12	1982	1987	1988	---	1986	---
Birmingham, AL	45	1980	---	---	---	1977	1977[a]
Boston, MA	8	1987	1987	1987	1987	---	1987[a]
Buffalo, NY	25	---	---	---	---	---	---
Chicago, IL	3	---	1985	1985	1985	1985	1985[a]
Cincinnati, OH	22	1983	1978	1978	---	---	---
Cleveland, OH	13	1984	1982	1982	1994	---	---
Columbus, OH	36	1983	1980	1981	---	1981	---
Dallas, TX	17	---	1984	1984	---	---	1984[b]
Denver, CO	28	---	1983	1983	1983	1983	1983[a]
Detroit, MI	5	---	1983	1983	---	1983	1983[a]
Fort Worth, TX	44	---	1986	1986	1988	---	---
Gary, IN	53	---	---	---	---	---	---
Greensboro, NC	57	---	---	---	1985	---	1985[a]
Houston, TX	14	---	1981	1981	1984	---	---
Indianapolis, IN	30	---	1984	1984	---	1987	---
Kansas City, MO	27	---	1981	1981	---	---	1981[b]
Los Angeles, CA	2	1983	1983	1983	1987	1983	---
Miami, FL	26	1985	---	---	1985	---	1985[a]
Milwaukee, WI	20	1987	1989	1989	---	---	---
Minneapolis, MN	16	---	1980	1980	---	---	1980[b]
Nassau, NY	9	---	---	---	No program	---	No program
New Orleans, LA	32	---	1984	1984	---	---	1984[b]
New York, NY	1	---	1991	1992	1992	1992	1992[a]
Newark, NJ	15	1984	---	---	---	---	1984[b]
Norfolk, VA	48	---	---	---	No program	---	No program
Passaic, NJ	23	---	---	---	No program	---	No program
Philadelphia, PA	4	1984	1982	1983	1982	1982	1982[a]
Pittsburgh, PA	10	---	---	---	1980	---	1980[a]
Portland, OR	34	---	---	---	1977	---	1977[a]
Rochester, NY	38	---	---	---	1980	---	1980[a]
Sacramento, CA	42	1985	---	---	---	---	1985[b]
Saint Louis, MO	11	---	---	---	---	---	---
San Bernardino, CA	29	---	---	---	---	---	---
San Diego, CA	24	---	1986	1986	1985	---	1985[a]
San Francisco, CA	6	1984	1984	1984	1984	1984	1984[a]
San Jose, CA	31	1983	1983	1983	1984	1983	1983[a]
Seattle, WA	18	1986	1980	1980	Before 1984	---	---
Tampa, FL	33	1985	---	---	---	---	1985[b]
Washington DC	7	1980	1975	1975	---	1977	1977[a]

Notes: See text for details on the data sources.
[a] Included in the "17-MSA" and "25-MSA" samples.
[b] Included in only the "25-MSA" sample.

Table A2: Changes in black-white gaps after set-aside initiation, relative to pre-program trend
[absolute value of t-ratios]

	Effects for different samples of MSAs				
	(1)	(2)	(3)	(4)	(5)
Number of MSAs	25	28	32	38	21
A. Self-employment					
Post-program B-W diffs					
Year 0	3.31***	3.54***	3.22***	2.15***	3.01***
	[4.68]	[5.31]	[4.78]	[2.99]	[4.84]
Year 1	2.97***	3.35***	2.93***	2.20**	2.53***
	[3.02]	[3.75]	[3.38]	[2.54]	[2.67]
Pre-program					
B-W trend diff	0.003	-0.099	-0.126	-0.131	0.741**
	[0.02]	[0.69]	[1.10]	[1.07]	[2.48]
B. Employment					
Post-program B-W diffs					
Year 0	0.76	0.20	0.68	0.22	0.92
	[0.47]	[0.13]	[0.49]	[0.18]	[0.64]
Year 1	2.04	1.22	1.53	0.96	1.46
	[1.17]	[0.73]	[0.88]	[0.77]	[1.13]
Year 2	4.68**	3.88**	3.92***	2.85**	3.64**
	[2.29]	[2.11]	[2.67]	[2.17]	[2.52]
Year 3	5.55**	4.81**	4.83***	3.19*	4.61**
	[2.10]	[2.09]	[2.86]	[1.94]	[2.34]
Year 4	5.42*	4.68*	4.45**	2.80	5.14**
	[1.92]	[1.91]	[2.49]	[1.51]	[2.08]
Year 5	7.55**	6.79**	6.46***	6.40***	6.76**
	[2.42]	[2.37]	[3.11]	[3.75]	[2.06]
Year 6	4.72	4.08	3.64	3.80*	5.30
	[1.36]	[1.26]	[1.42]	[1.77]	[1.21]
Year 7	11.16**	10.70**	10.04***	8.28**	11.87**
	[2.21]	[2.38]	[2.85]	[2.56]	[2.55]
Pre-program					
B-W trend diff	-0.703**	-0.780**	-0.588**	-0.485*	-1.304
	[2.08]	[2.24]	[2.18]	[1.94]	[1.41]
Race-year FE's	Y	Y	Y	Y	Y
MSA FE's	Y	Y	Y	Y	Y
Educ, age FE's	Y	Y	Y	Y	Y
Race-educ, race-age FE's	Y	Y	Y	Y	Y
Number of observations	223,765	239,242	253,783	300,233	191,286

Notes: See notes to Table 4. Column (1) is based on the 25-MSA sample; column (2) adds MSAs with no programs (Nassau, Norfolk, Passaic); column (3) also adds MSAs where no program was found (Buffalo, Gary, Saint Louis, San Bernardino); column (4) further adds MSAs with conflicting program dates and uses court case or city records for dates (Los Angeles, Houston, Indianapolis, Columbus, Fort Worth). Column (5) excludes MSAs with program dates either before or after the 1979 to 1989 period (Washington, D.C., Portland, Birmingham, New York City). Estimated sampling errors are corrected for heteroskedasticity and over-time clustering at the MSA level, and regressions are weighted by the CPS sample weights. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively.

Figure 1: Black and white self-employment and employment rates from 1979 to 1989

A. Self-employment rates (in percent) in entire United States and in U.S. cities
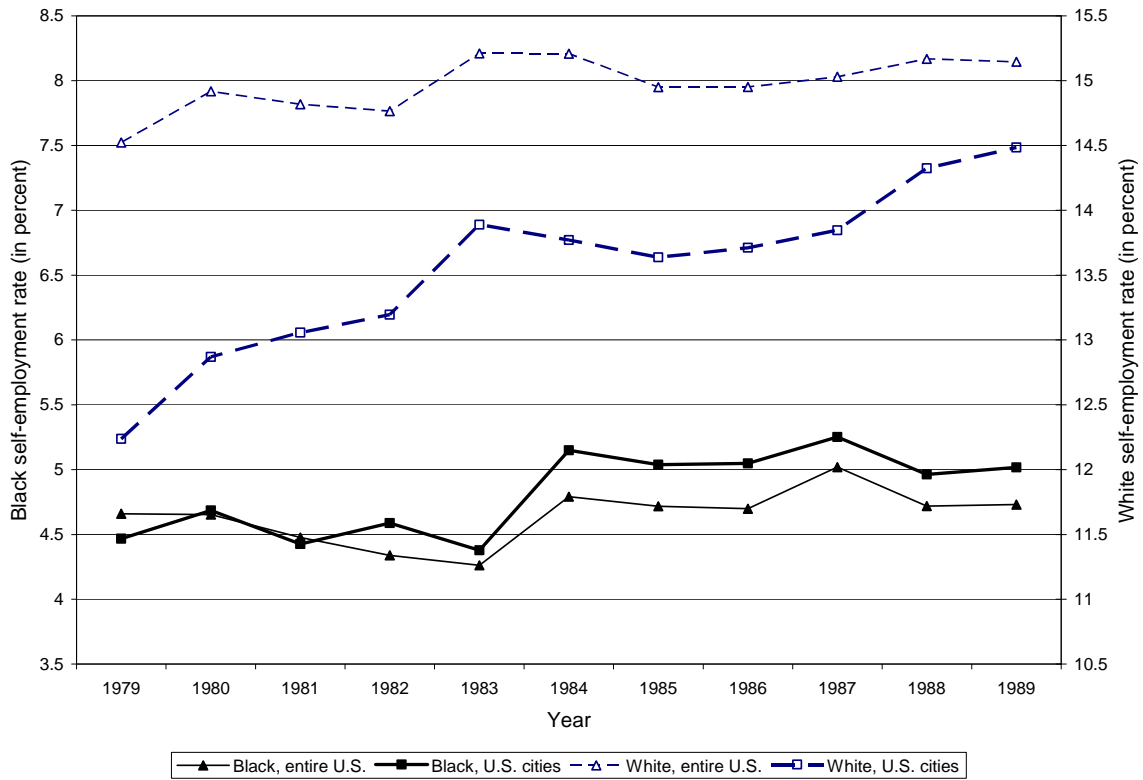
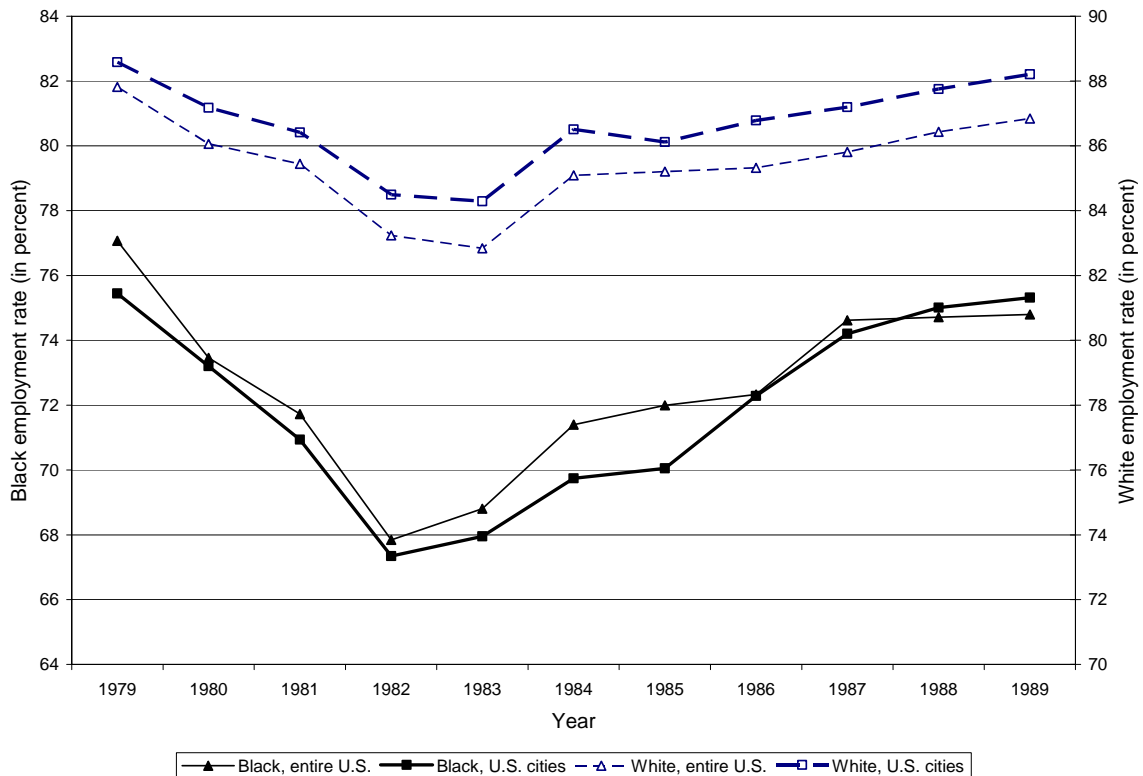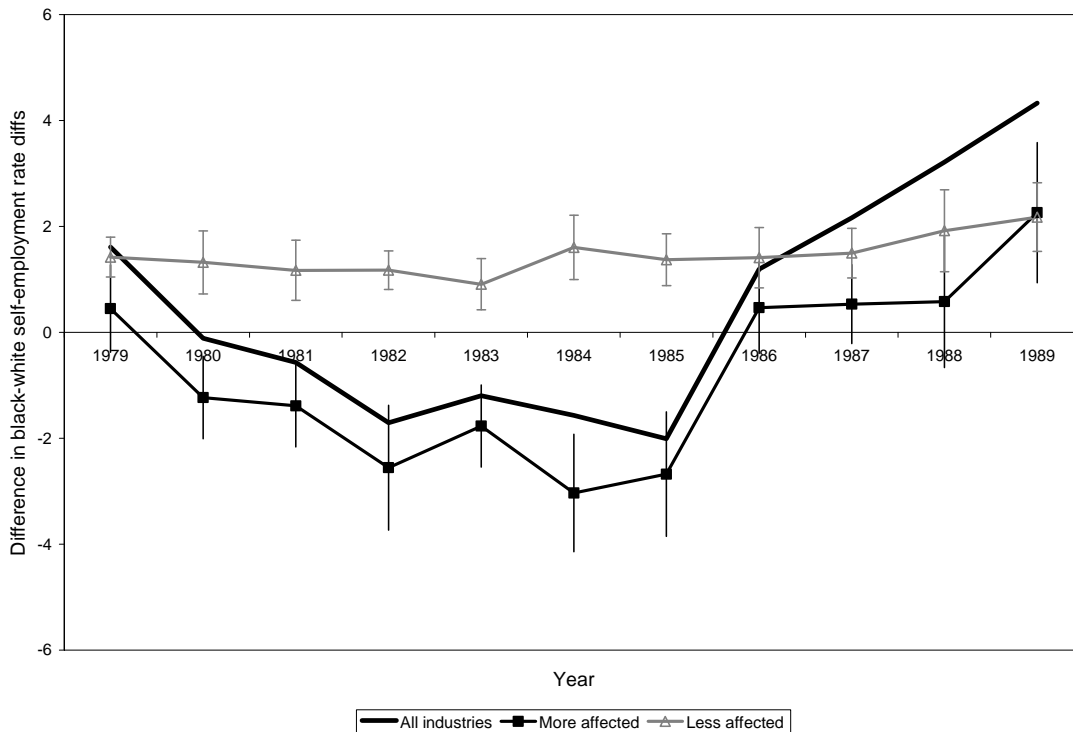B. Employment rates (in percent) in entire United States and in U.S. cities

Figure 2: Difference in black-white differences in self-employment rates between cities initiating set-asides program in 1985 and those not

A. Self-employment difference-in-differences

— 1985 vs. 1980 or before   — ■ —1985 vs. No program   — ■ — 1980 or before vs. No program

B. Self-employment difference-in-differences, by industry group

—— All industries   — ■ — More affected   —△— Less affected

Notes: Panel A plots differences between MSAs that: i) do not have a set-aside program during the period (New York, Nassau, Passaic, Norfolk – 48,946 observations); ii) started a program in 1980 or before (Washington DC, Pittsburgh, Minneapolis, Portland, Rochester, Birmingham – 52,988 obs); iii) began one in 1985 (Chicago, San Diego, Miami, Tampa, Sacramento, Greensboro – 53,210 obs). In Panel B, contrasts are between MSAs that began a program in 1985 and the other two groups of MSAs. "More affected" and "less affected" industries are defined in Table 2. Panel B results are from regressions that include education and age dummies, race-specific MSA effects, race-specific year effects, and a 1985 program dummy interacted with year effects. The ± 1 standard error bands shown are corrected for heteroskedasticity and over-time, MSA-level clustering.

Figure 3: Event study results for black-white differences in self-employment and employment rates
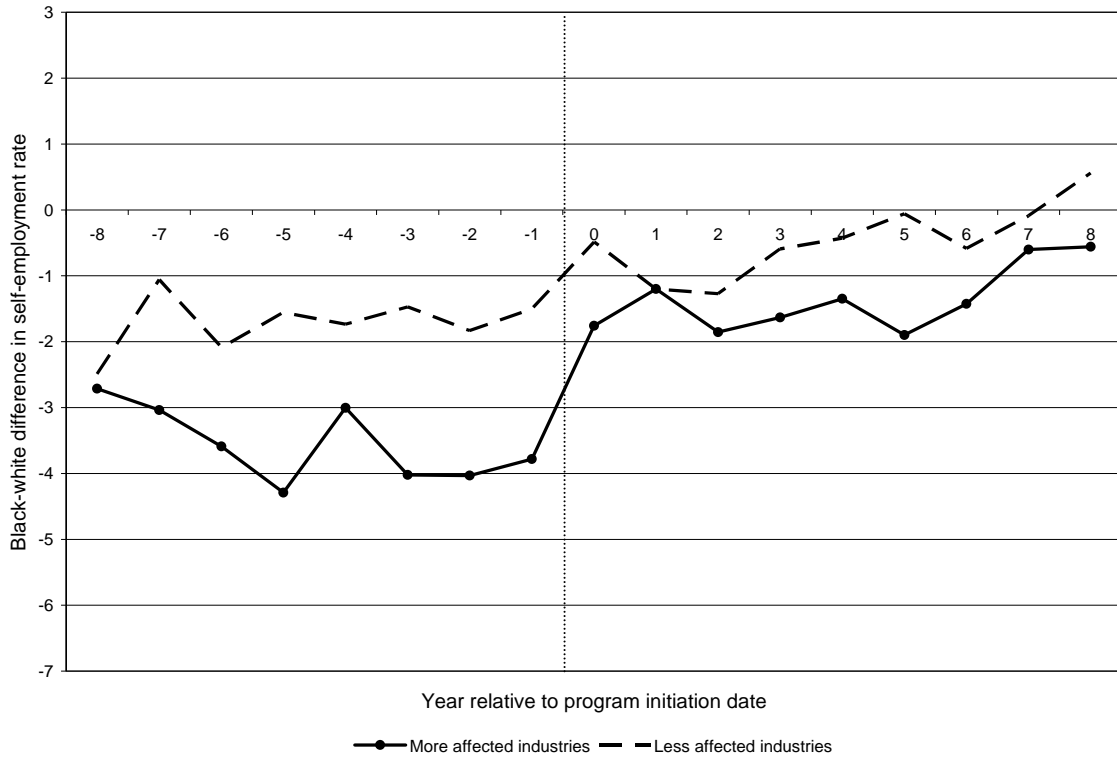
A. Event study for 17-MSA sample
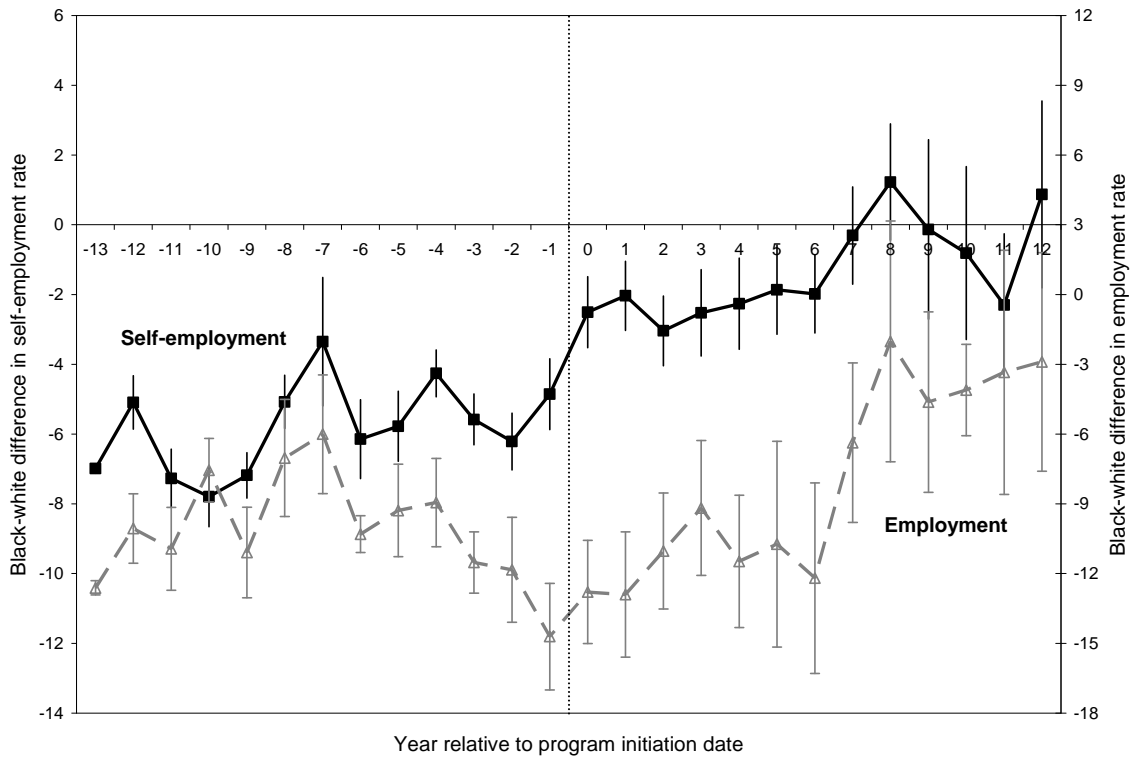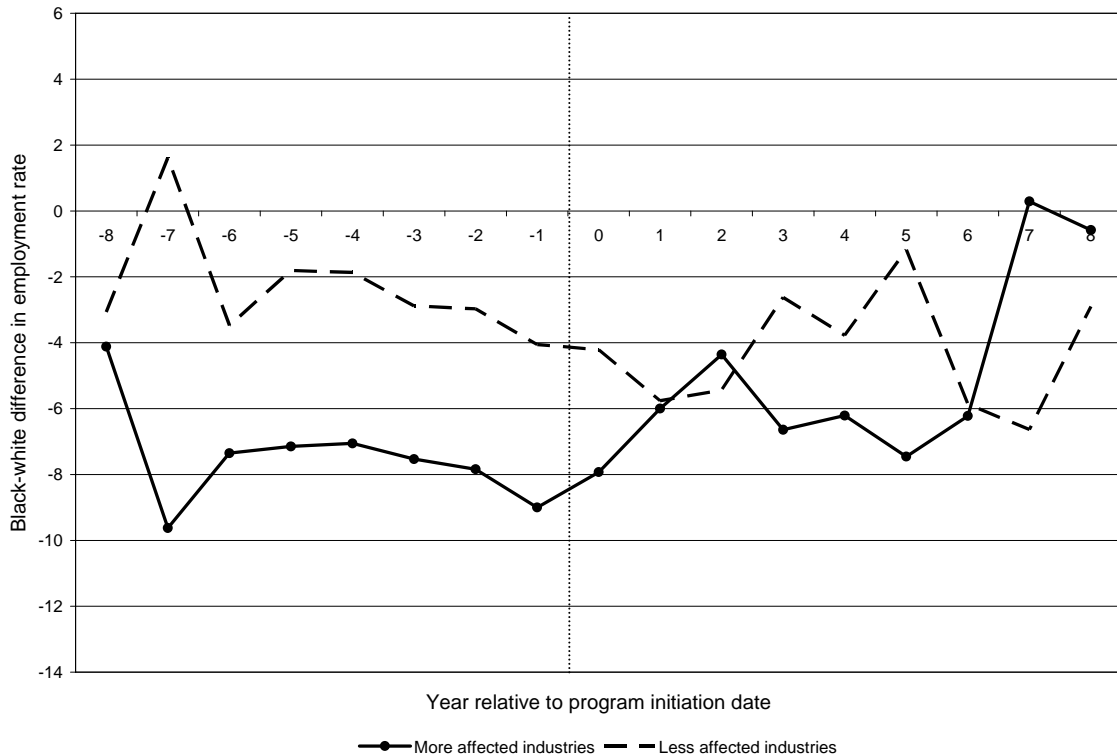

B. Event study for 25-MSA sample
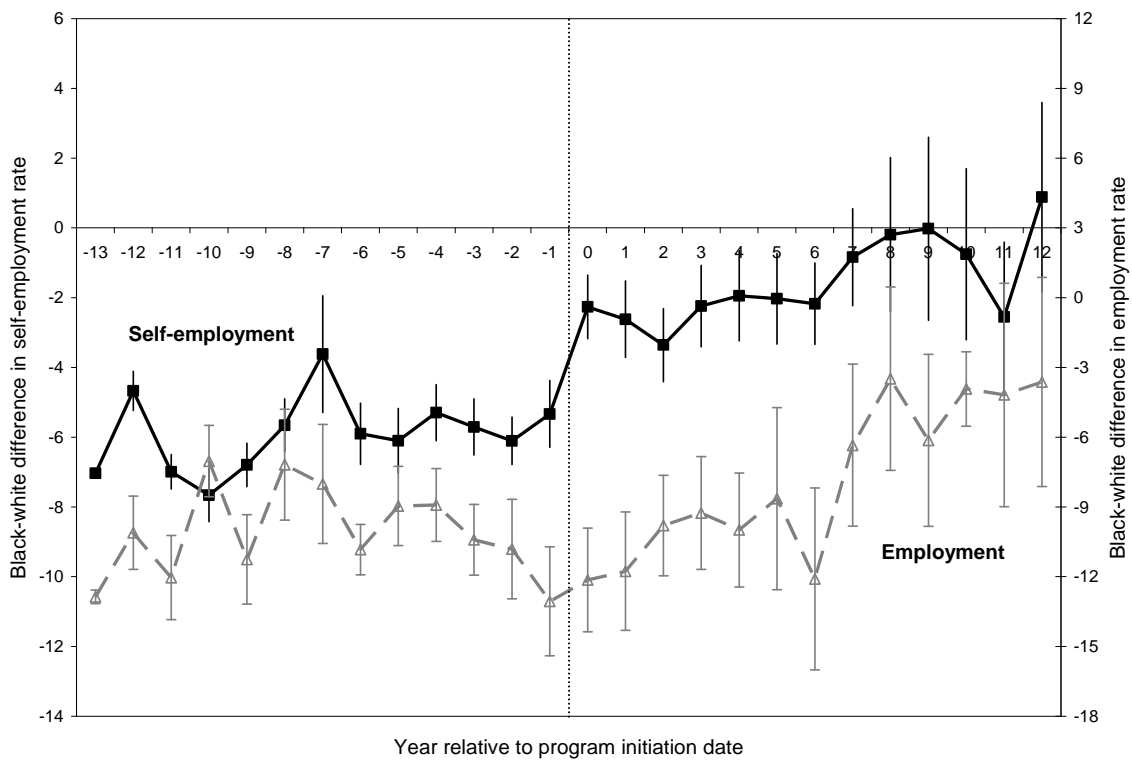
<u>Notes</u>: Series come from linear probability models that include MSA fixed-effects, race-specific year effects, unrestricted indicators for education and age, and program time fixed-effects. Regressions are weighted by CPS sampling weights. The ± 1 standard error bands are shown and are corrected for heteroskedasticity and over-time clustering at the MSA level.

Figure 4: Black-white differences in self-employment and employment rates for more and less affected industry groups, 25-MSA sample

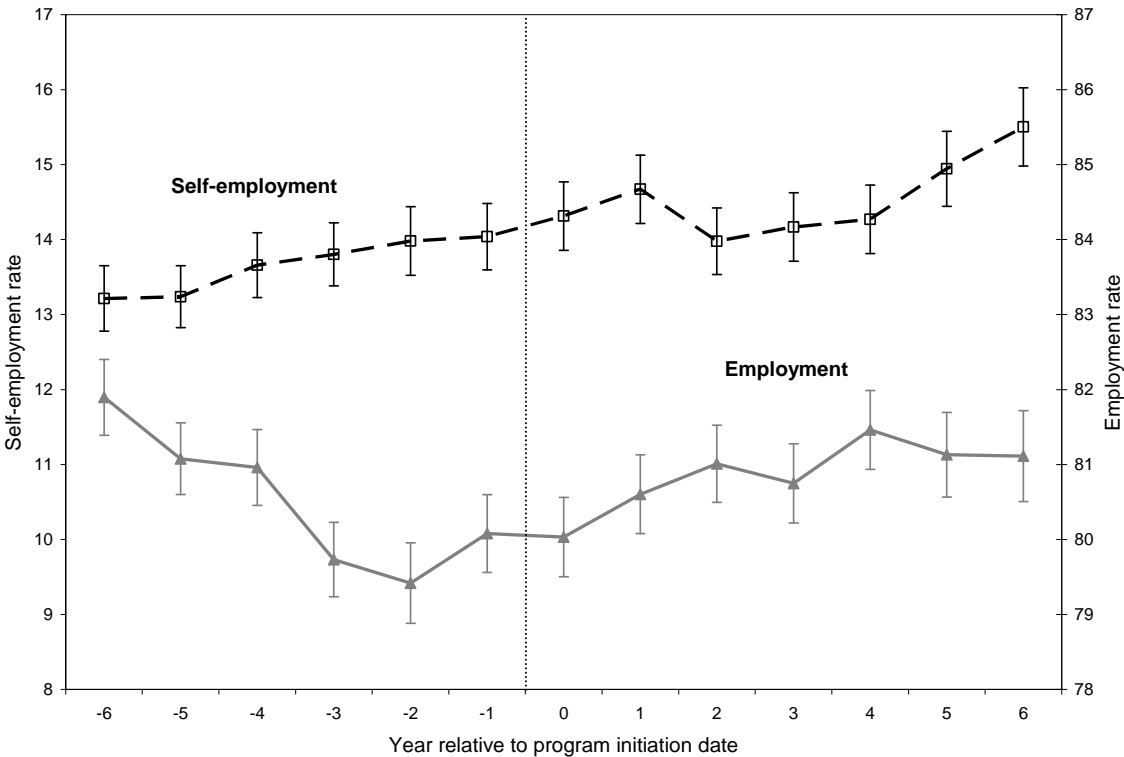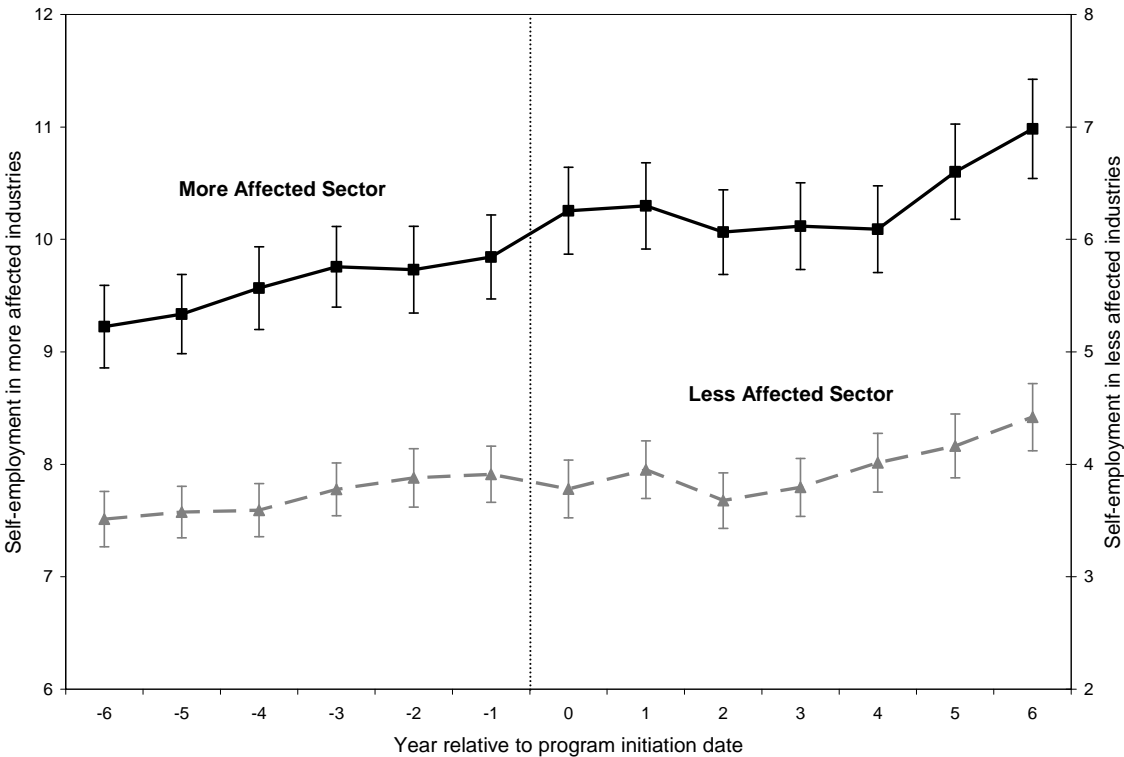## A. Self-employment, adjusted

B. Employment, adjusted

Notes: Series come from linear probability models that include MSA fixed-effects, race-specific year effects, unrestricted indicators for education and age, and program time fixed-effects. Dependent variables are indicators equal to one if person is self-employed (or employed) in more affected (or less affected) industries. Regressions are weighted by CPS sampling weights.

Figure 5: Aggregate self-employment and employment rates for the 25-MSA sample
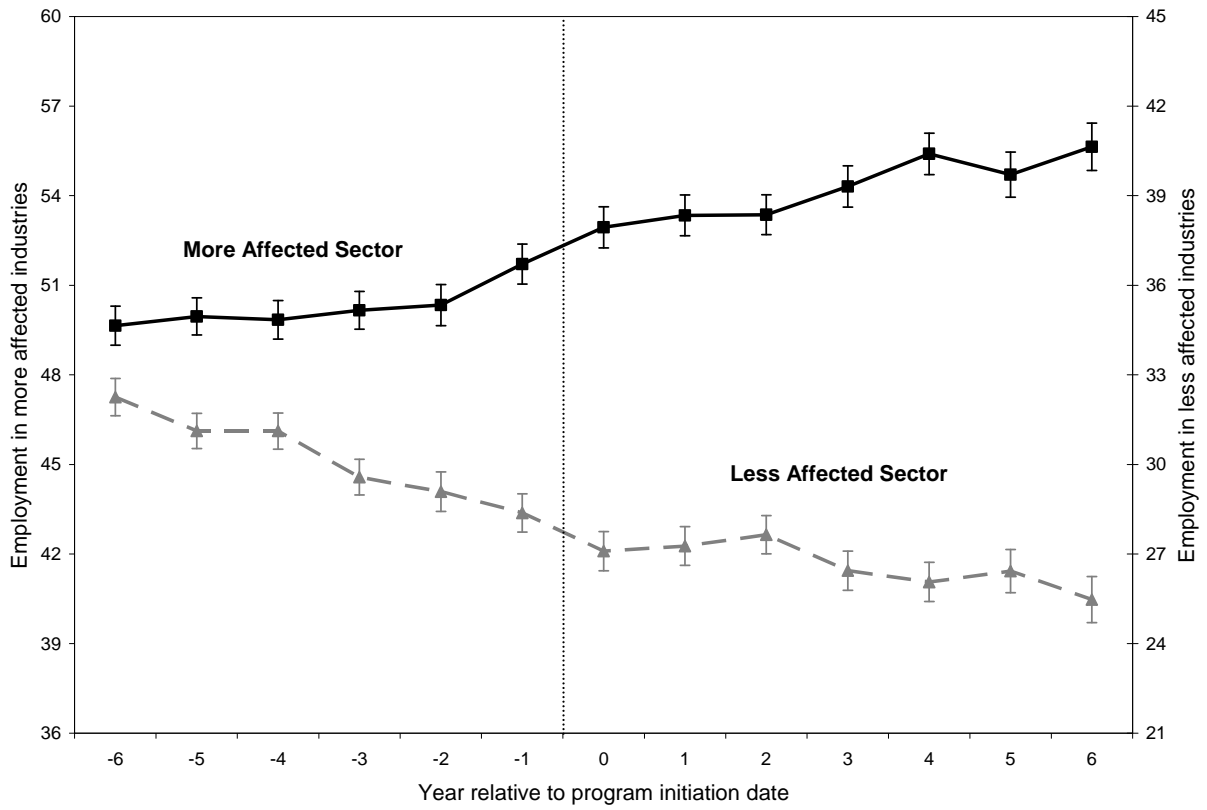
A. Overall self-employment and employment

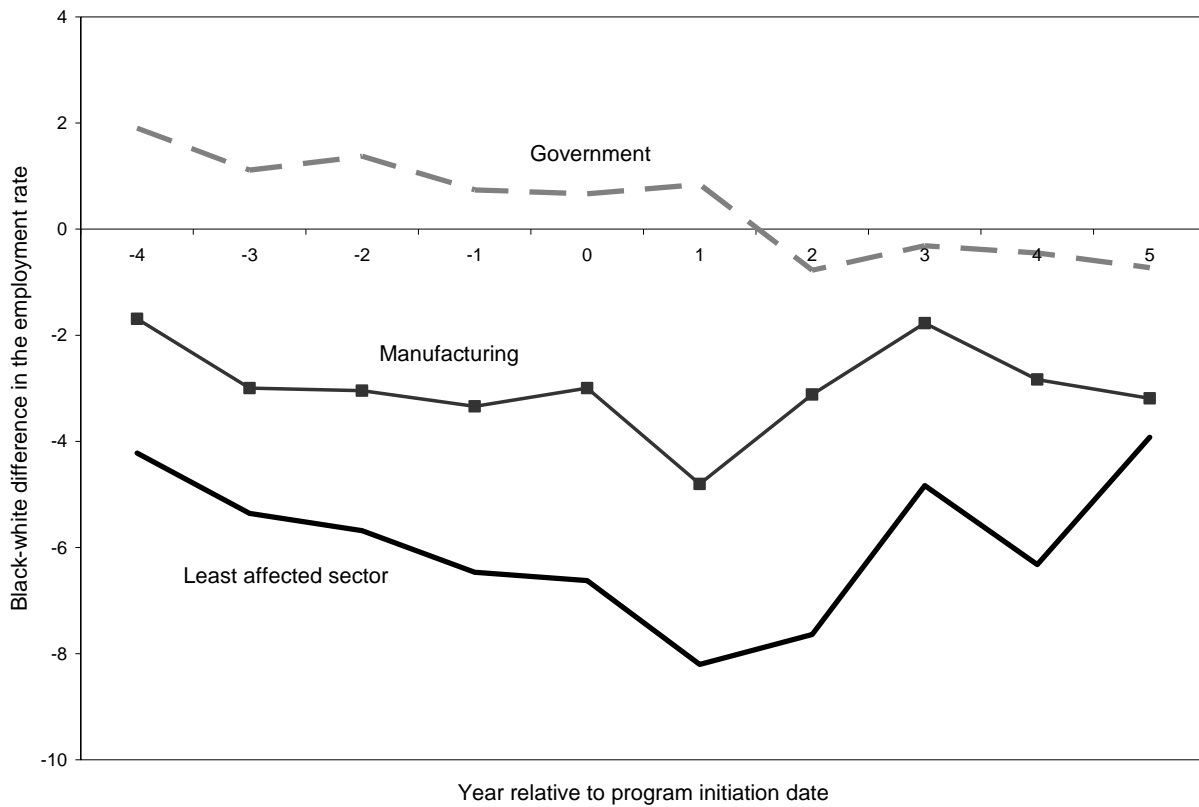B. Self-employment by industrial sector

C. Employment by industrial sector

Notes: Series come from linear probability models that include MSA fixed-effects and program time fixed-effects, which are plotted. Dependent variables are indicators equal to one if person is self-employed (or employed) in more affected (or less affected) industries. Regressions weighted by CPS sampling weights. The ± 1 standard error bands are shown and are corrected for heteroskedasticity.

Figure A1: Black-white employment differences in less affected industries,
25-MSA sample

Notes: Series come from linear probability models that include MSA fixed-effects, race-specific year effects and program time fixed-effects. Dependent variables are indicators equal to one if person is employed in the sector or industry. Regressions are weighted by CPS sampling weights.