The Effect of Court-Ordered Hiring Quotas on the Composition and Quality of Police

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September 2003

Abstract: This paper examines the role of the federal courts in integrating police departments in the United States. Using a new data set on police force demographic composition, city demographics, and employment discrimination litigation in 314 large U.S. municipalities, I demonstrate that the filing of a class action lawsuit alleging hiring discrimination against African Americans is associated with a trend break in police department black employment share. I estimate that the 25-year gain in black employment share in litigated departments is in the range of 8 to 12 percentage points. Given the low attrition rate of police officers, this is consistent with a hiring fraction African American approximately 12 to 19 percentage points above the pre-litigation police department fraction black. I find little evidence that litigation led to increased crime, despite large and persistent differences by race in police department entrance examination scores.

*E-mail: jmccrary@umich.edu. I thank David Card, Ken Chay, Guido Imbens, David S. Lee, Jesse Rothstein, Heather Royer, Emmanuel Saez, and Till von Wachter for comments on the paper. Stephanie Watt, Jeff Butler, Andrea Cann, Amy Hawkins, Katie Ikard, Alma Vega, and Shayesteh Zarrabi all provided excellent research assistance. Any errors are my own.

Introduction and Overview

Beginning in 1969, dozens of class action employment discrimination lawsuits were filed against police departments in the federal district courts alleging hiring discrimination against African Americans.[1] The typical case resulted in a judgment against the police department, with a substantially invasive remedy awarded to plaintiffs. Departments were almost always required to change the entrance examination administered to applicants, and frequently they were instructed to adopt explicit hiring quotas. I estimate that among 314 large municipal police departments, one-third were subject to a class action suit, three-quarters were unsuccessful in their defense, and almost 90% of the unsuccessful were subject to hiring quotas. This paper is the first comprehensive examination of the impact of these suits on police department black employment share and city crime rates.

To learn about the impact of litigation[2] on black employment share is a difficult task. First, because of the low turnover rate in police jobs, even dramatic changes in the demographic composition of new hires will not substantially affect black employment share in the short run. Assessing the impact of litigation thus requires contrasts of long-run trends. Second, litigated departments are not representative of all departments. Both private litigants and the Department of Justice targeted big departments with large disparities between black employment and city population shares. Third, unlitigated departments may have responded to the threat of litigation by increasing black employment share, confounding the direct effect of litigation with the effect of the threat of litigation.

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[1] While this paper maintains a focus on the impact of hiring suits brought on behalf of African Americans, many suits were additionally brought on behalf of women and Latinos, and a smaller number of suits may be found challenging promotion practices on behalf of any of these three groups. Less than a handful of class action suits have encompassed other protected class members. Throughout this paper, I utilize the terms “African American” and “black” interchangeably.

[2] Throughout this paper, “litigation” will be understood to mean class action employment discrimination litigation against police departments in which discrimination in hiring against African Americans is alleged, and in which the class is comprised at least partly of African Americans.

My approach to estimating litigation's impact is thus indirect and focuses on obtaining a bound on the contribution of litigation to the integration of police departments. To estimate the impact of litigation, I utilize natural variation in litigation status and in the timing of litigation. Because it is difficult to know the extent to which these estimates are subject to bias, I focus my discussion around plausible sources of positive bias in the estimated effects. I estimate that the average litigated department gained some 8 to 12 percentage points in black employment share over 25 years, relative to what it would have gained, had no department been litigated.

This research complements a large and varied literature in economics on the impact of federal government labor market interventions on black economic progress.[3] Much of the research on this topic has focused on evaluating two legislative and executive interventions, the 1964 Civil Rights Act (CRA) and the federal contractor program. Relative to the large literatures on these two topics, little attention has been paid to judicial enforcement of these acts. The argument that an important component of black economic progress that occurred following the CRA would not have occurred, but for its passage, would seem to be predicated on the belief that judicial enforcement of the CRA was effective. Moreover, many historians and sociologists view the courts as a primary catalyst in the evolution of federal antidiscrimination policy over the post-World War II era.[4] This view seems quite plausible in the context of public sector litigation. The 1964 CRA did not apply to public employers until the 1972 amendments; undeterred, the federal judiciary routinely found police departments to be guilty of discrimination against African Americans in the years 1969 to 1971, relying on the Reconstruction Civil Rights Acts of 1866 and 1871.

That the Reconstruction Civil Rights Acts lay dormant for nearly a century before the courts began to enforce them raises a question: Is the counterfactual of economic interest the evolution of black economic progress in the absence of the 1964 CRA, as has been tacitly

[3] Donohue and Heckman (1991) synthesize a great deal of the literature on this topic.

assumed in most of the economics literature, or is it the evolution of black economic progress in the absence of a federal judiciary predisposed to aggressive enforcement of civil rights?

Focusing on police departments as a way of getting at this larger question is attractive for several reasons. First, police departments, at least relative to private employers, are ideally suited to longitudinal study. Issues of survivorship are ignorable, and police departments have very stable employment levels over time. Further, the technological and educational requirements for becoming a police officer have changed little over time. In contrast, private sector industries in which the courts aggressively pursued affirmative action (for example telecommunications and steel) have undergone substantial changes in industrial organization, in employment levels, and in the human capital endowments of their workforce.[5] All of these factors would substantially complicate the analysis and potentially cloud the impact of court intervention on employers' hiring decisions.

Second, city crime rates form a natural, if crude, measure of police department productivity. If litigation leads to integration of a police force, but to no impact on crime rates, one might view the judicial intervention as a successful affirmative action plan. On the other hand, it is possible that litigation increased crime, perhaps due to productivity differences between affirmative action beneficiaries and incumbent officers, and perhaps due to morale problems.

Third, the integration of police departments has been an important policy issue in the United States at least since the riots of the mid-1960s. Police departments then were charged with being an army of white occupation; police departments now are charged with racial profiling and police brutality. For many people, then, this topic may be of interest in its own right.

The remainder of the paper is organized as follows. Section I describes briefly the evolution of employment discrimination law regarding public employers and details the specific

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[4] See, for example, Skrentny (1996), pp. 136-140 and Moreno (1997), pp. 235-281, Graham (1990) generally, and Burstein (1985), pp. 15-18.

employment practices of police departments challenged in the litigation. Section II presents a case study of Chicago. Section III presents the basic evidence on the impact of litigation on black employment share and considers robustness checks. Section IV examines the evidence on crime rates, while Section V concludes. The data collected for this project are described in detail in the Data Appendix.

I. Public Sector Employment Discrimination Law

Overwhelmingly, the litigation studied here pertains to neutral selection mechanisms that adversely affected the hiring chances of African American applicants to police departments. Although there was only rarely evidence or allegations of discriminatory intent, the federal courts justified the interventions into police department hiring practices by relying on a legal theory—disparate impact theory—developed by the courts in the mid- and late-1960s. The central tenet of disparate impact theory is that a neutral employment practice that disproportionately excludes a class of individuals protected under civil rights law constitutes *prima facie* evidence of discrimination. If a protected class group member may demonstrate that an employment practice leads to a disparate impact for their group, then the employer is obliged to demonstrate that the selection mechanism is predictive of job performance. The Supreme Court sanctioned this theory in 1971 in *Griggs v. Duke Power Company*: "The touchstone is business necessity. If an employment practice which operates to exclude Negroes cannot be shown to be related to job performance, the practice is prohibited."[6]

Disparate impact theory was largely developed in private sector cases alleging violations of Title VII of the 1964 CRA. Although coverage of Title VII would not be extended to public employers until the 1972 amendments, disparate impact theory arguments were put to the courts

[5] For a description of the litigation in these sectors, see Wallace (1976) and Stein (1998).
[6] 401 U.S. 424, 431 (1971).

between 1969 and 1971 in cases involving police departments in such disparate places as Alabama, California, Florida, Illinois, Massachusetts, North Carolina, and Pennsylvania. These cases alleged that employment practices exhibiting a disparate impact upon protected class group members violated the Fourteenth Amendment and the Reconstruction Civil Rights Acts of 1866 and 1871. The courts were receptive to these arguments, and in many cases required police departments to hire according to quotas by race, foreshadowing the remedies that would result from Title VII litigation brought after 1972.

Cases alleging discrimination in police departments could be brought either by private litigants or, after 1972, by the Department of Justice. Department of Justice involvement was important not only because of the high caliber of the government attorneys, but also because it placed in jeopardy the revenue sharing funds distributed to police departments by the federal government.[7] Nonetheless, while the Department of Justice was undoubtedly an active player in the litigation, private litigants brought fully 80% of the class action suits studied here.

The specific employment practice challenged in virtually every case is the police department entrance examination. These examinations are usually required by state and local civil service ordinances, which specify that hiring is to occur in rank order by test score.[8] Thus, traditional police department selection methods are essentially the Bowen and Bok (1998) model of race-neutral college admissions.[9]

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[7] Crime Control Act of 1976, § 122(c)(2)(E), 90 Stat. 2418 (1976). It appears from the records I have examined that this lever was only employed against the Chicago Police Department, and threatened against Syracuse.

[8] Rank order hiring is mitigated to a certain extent by the so-called "Rule of Two" or the "Rule of Three," or by the requirement that those with sufficiently high scores pass additional hurdles, such as a background investigation or physical examination. Myrdal and Raper note the prevalence of civil service systems already in the 1940s, but do not mention entrance examinations. "Slightly over half the [112 Southern] police systems studied are now using some form of civil service, many of them for less than five years." Myrdal (1944), p. 539. In footnote 'b' of the same page, Myrdal notes: "In few cases do the Southern city police civil service systems approximate in rigor the systems employed by Northern cities." Greisinger, Slovak, and Molkup (1979) note that "[b]y the early 20th century civil service systems based on the merit principle were established in many state and local governments. Since then, they have grown in numbers at a very rapid pace, so that by the contemporary era they have become almost ubiquitous." p. 26.

[9] pp. 31-42.

Many police department entrance exams, especially in the pre-litigation years, were aptitude tests akin to the verbal SAT. Detroit administered a three-hour I.Q. test to applicants into the 1970s.[10] The District of Columbia used a civil service examination designed for selecting applicants into general positions in the federal bureaucracy known as Test 21. Test 21 was comprised of 80 questions on vocabulary, reading comprehension, and analogies. A representative question reads:

> 73. PROMONTORY means most nearly
> A) marsh
> B) monument
> C) headland
> D) boundary
> E) plateau.[11]

The use of aptitude tests in police hiring is ongoing. For example, the Memphis Police Department continues to use an I.Q. test to screen its applicants.[12]

African American applicants, both historically as well as today, do not fare as well on these exams as white applicants.[13] For example, in the Detroit exams of 1967-1971, the African American pass rate was 44.3%, and the white pass rate was 80.7%.[14] The Memphis tests administered 1981-1989 had a similar impact on black applicants. The pass rate for whites was a high 96.7%, but that for African Americans was 69.2%.[15]

Entrance examinations that were arguably more job-related than aptitude tests also served to disproportionately disqualify African Americans. For example, approximately 20-25% of the questions on the 1970 New York City Police Department (NYCPD) entrance examination asked applicants to supply the appropriate behavioral response to a hypothetical situation supposed to typify the day-to-day experiences of a police officer.[16] Yet this exam excluded African

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[10] 483 F. Supp. 930, 948-952 (1979).
[11] 512 F. 2d 956, 975 (1975). The entire test is reproduced in the Appendix to the majority decision.
[12] 49 F. Supp. 2d 1051, 1072 (1999).
[13] Compare with the general evidence on the black-white test score gap in Jencks and Phillips (1998).
[14] 483 F. Supp. 930, 948 (1979).
[15] 49 F. Supp. 2d 1051, 1072 (1999).
[16] 431 F. Supp. 526, 543 (S.D. New York 1977).

Americans at almost the same rate as the Memphis exam: 81.9% of whites passed the 1970 exam, while 54.5% of African Americans did.[17]

Following a judicial determination that a police department entrance examination disparately impacted African Americans, there was typically a one to three year lag before any quota would be imposed. During this period, the judge would order the city to devise a test that either did not exhibit disparate impact or was job-related. If hiring was to proceed in the interim, temporary hiring quotas were sometimes put in place until the new exam was administered. If the department proved unable to devise a test that satisfied the court, and especially if its inability seemed willful, the court was likely to impose an ongoing quota.[18]

Police departments seem to have been fated to test the patience of judges in this regard, and quotas were imposed with some frequency. I have collected detailed information on the universe of published decisions regarding cases brought against the 314 large police departments studied here.[19] I estimate that these departments lost over 80% of the cases brought against them in 1969 to 1980, and that either interim or ongoing quotas were imposed in over 90% of the cases they lost in these same years. As we will see below, cases brought in the 1980s and 1990s were less numerous and for the most part targeted small police departments. Notably, these cases were also less successful than cases brought before 1980. I estimate that police departments litigated after 1980 won 65% of the cases brought against them. Moreover, only 43% of the cases departments lost resulted in a hiring quota.

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[17] The Rand Corporation, in summarizing its study of the 1968 and 1970 NYCPD entrance examinations, concluded that it was "confident that the patterns observed for [the 1970 exam] are not unique to that exam but are most likely typical of the patterns for all recent civil service patrolmen's exams." Quoted in 431 F. Supp. 526, 540 (S.D. New York 1977). The distribution of test scores by race for the NYCPD is discussed further in Section IV.A.

[18] Ongoing quotas either expired by their terms (e.g. a judge might require that 50% of new hires be African American until 40% of the force were African Americans) or remained in operation until further order of the court.

[19] See Data Appendix for details on the data collection.

However, although litigation was concentrated in the 1970s, quotas remained part of police department hiring into the 1990s, and even today. For example, Boston, Cambridge, Springfield, and Worcester remain subject to a hiring quota first imposed in 1973.[20]

II. Litigation and the Integration of the Chicago Police Department

Before turning to the main empirical analysis, it is useful to consider the experience of Chicago, for several reasons. First, it is one of the larger cities in the sample of cities I analyze below and is an important city in the United States. Second, the hiring and promotion practices of the Chicago Police Department (CPD) have been subject to litigation continuously from 1970 to 2000, and the remedies from this litigation included aggressive hiring quotas and withholding of some $100 million in revenue sharing funds.[21] Third, the City of Chicago has graciously given me access to a unique administrative data set containing records on the race and hiring and termination dates of a large sample of police officers dating to the early 1960s. These data allow me to assess the impact of litigation on department hiring practices in an unusually direct manner because they allow calculation of the fraction African American among new hires for many years prior and subsequent to litigation. Typically, only data on the overall black employment share is available.

The CPD was first required to justify its employment practices to a federal district court judge in 1970, after the Afro-American Patrolmen's League initiated suit. At that time Chicago employed a substantial number of black officers, especially relative to other major cities.[22] Figure 1 gives the black employment and black hiring share in the CPD from 1960 to 1997.[23]

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[20] *Castro v. Beecher*, 365 F. Supp. 655, 660-662 (D. Mass. 1973), and personal communication with Toni Wolfman, the lead attorney on the case.
[21] See, for example, the discussion of the litigation at 411 F. Supp. 218, 225-245 (N.D. Ill. 1976).
[22] The CPD employed substantial numbers of black officers even as of 1940. Raper (1940).
[23] I thank Adam Kingsley of the City of Chicago Law Department for the use of these data. The data set contains, for every individual who has worked for the CPD since approximately the late 1970s, a hiring

The black employment share in the 1960s and early 1970s was roughly 18%. For comparison, 23% of Chicago residents in 1960 were African Americans, and 33% were African Americans in 1970.

The Justice Department joined the suit against the CPD in 1973, and in 1974 an interim hiring quota on behalf of African Americans was imposed, followed by an ongoing hiring quota in 1976. Figure 1 demonstrates that there was substantial compliance with the court's orders. In the years 1971-1973, the hiring share was roughly 10%. By 1975 it had climbed to 40%, and throughout the period 1975-1990, the hiring share averaged 32%. For comparison, the city fraction African American had risen to 40% by 1980, where it roughly remains today.

Although the demographic composition of new hires to the CPD responded dramatically to the hiring quota, the demographic composition of the police force changed only slowly, because attrition for CPD officers is so low. Using these data, I estimate an annual quit rate of roughly 4%.[24]

Unfortunately, for cities other than Chicago, high quality data on black hiring share are not available. It is thus of interest to establish the mathematical relationship between the hiring fraction African American and the police force fraction African American. Given a one-time

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date, a termination date, race and other information not used here. Because of the long tenure of most police officers, it is possible to generate a large sample of the officers who were working for the CPD in all years from 1960-1997. Using the Uniform Crime Reports (UCR), I estimated the sampling rate for each of these extracts. From a low of 40% in 1960, the rate rises smoothly in time, arriving at a constant level 95%-105% beginning in 1977. The sampling rate for African American officers appears to be higher than that of other officers for the early years. For 1962, 1967, and 1969 auxiliary estimates of the number of African American officers in the CPD are available (see Data Appendix); I estimate the black sampling rate in these years to be 80%, 76%, and 70%. These rates are to be compared to the overall rates of 50%, 65%, and 71%, respectively. The downward trend in hiring fraction African American pre-1970 is likely an artifact of this phenomenon, but the 1974 spike may not be attributed to this because the sampling rates were similar already in 1969.

[24] This estimate appears to be in the range of estimates available from published sources. Butterfield (2001) notes that "[i]n New York City, more than 1,700 officers left the 41,000-member force last year through retirement or resignation, a third more than the year before." (17/410=0.041) More historically, the the District Court of the Eastern District of Pennsylvania reports that "[a]s of September 22, 1972… there were 8,082 members of the [Philadelphia] Police Department… defendants estimate that approximately 96 persons presently employed on the Police Force will, as a result of attrition, no longer be employed on the Police Force as of January 1, 1973." (96*(365/(8+31+30+31))/8082=0.043. 348 F. Supp. 1084, 1110 (E.D. Pa. September 29, 1972).

shift in regime regarding hiring fraction African American, the length of time needed for the overall department fraction to rise to the new hiring fraction is related to the quit rate arithmetically. Let y_t denote department fraction African American at period t, π_t the fraction African American among newly hired officers, h_t the overall hiring rate, q_t^B the quit rate among African American officers, and ε_t the year-over-year percent growth in the workforce. Then one may easily show that

$$y_t = \frac{1-q_{t-1}^B}{1+\varepsilon_t} y_{t-1} + h_t \pi_t \approx \beta y_{t-k} + (1-\beta)\pi \qquad (1.1)$$

where $\beta = (1-q)^k$ is the fraction of the workforce observed at date *t* that is still in the workforce at date *t+k*. The approximation is exact if $\pi_t = \pi$, $\varepsilon_t = 0$ and $h_t = q_{t-1}^B = q$. These assumptions may be summarized easily in words: that the new hires fraction African American and the number of employees is constant over the period in question, and that African American officers quit at the same constant rate q at which other officers quit.

We may rewrite (1.1) as $\pi \approx \frac{1}{1-\beta}\left(y_t - \beta y_{t-k}\right) = y_{t-k} + \frac{1}{1-\beta}\left(y_t - y_{t-k}\right) \equiv \pi_{t-k} + \frac{\Delta}{1-\beta}$. This allows us to approximate the black hiring share using an estimate of the quit rate in conjunction with observed changes in black employment share. For example, returning to Figure 1, we observe that over 1974 to 1997, the black employment share increased by 0.088. Taking a 4% quit rate as given, we estimate that $\beta = (1-0.04)^{23} = 0.39$ and conclude that the hiring rate must have been an improvement of +0.145 over the old black employment share. This hypothesized quantity is to be compared to the difference between the actual hiring rate and the initial black employment share in 1974, or $0.324 - 0.163 = 0.161$.

In the empirical work that is to follow, I seek to gauge the extent to which the Chicago experience is typical of other litigated cities. Because data on black hiring share are not available, my analysis focuses on assessing the impact of litigation on black employment share. Due to the

slow evolution of black employment share, we would expect that even aggressive court orders and substantial compliance would not lead to dramatic changes in black employment share. Rather, we would expect these to lead to a trend break in black employment share after the filing date of litigation.

III. The Impact of Litigation on the Representation Gap

In this section of the paper, I first consider comparisons between litigated and unlitigated departments over time. Second, I consider estimators that take advantage of the timing of litigation. Foremost among these are event study estimates in the spirit of Jacobson, Lalonde, and Sullivan (1993). Third, I discuss threats to the internal validity of the estimates presented in the second sub-section. Finally, I present a direct test of the litigation threat effect, or the hypothesized tendency of unlitigated departments to integrate in order to avoid litigation.

A. Contrasting the Litigated and the Unlitigated

Litigated cities are far from a representative sample of the 314 cities under study. Table 1 presents means and standard deviations of key variables by litigation status for two samples: the "full sample" and the "long sample." The full sample is comprised of 314 U.S. cities with the largest police and fire departments, as measured by the 1972-1997 Census of Governments.[25] Data on police department black employment share for cities in the full sample extend only back to 1969, the first year of litigation. The long sample is a subset of the full sample, and is comprised of 120 cities for which it was possible to obtain historical estimates of black employment share dating to 1960. Because the historical estimates on black employment share for the long sample are drawn primarily from commission reports on the one hand, and microfilm

records of the Southern Regional Council on the other, cities in the long sample are either large and well-known, or in the South. A more detailed discussion of the data and sample selection is provided in the Data Appendix.

Turning to the statistics for the full sample, we see that although only 30% of all cities are ever litigated, residents of litigated cities comprise nearly 58% of the 60 million residents in 1960, and 50% of the 75 million residents in 1999. Among litigated cities, 23% of the population lives in the South, and 11% in the Deep South, whereas among unlitigated cities, 33% of the population lives in the South, and 13% lives in the Deep South. Figure 2 is a map of the contiguous United States, with unlitigated cities represented as circles in panel A and litigated cities represented as stars in panel B. It is clear from the map that litigation was not confined to any particular region. However, there does appear to have been a focus on the areas of the country in which African Americans reside disproportionately: the South, the Northeast, the Great Lakes area, and the coastal cities of the West.

In substantive terms, the most important difference between litigated and unlitigated cities regards the city fraction African American. Throughout the sample period, 1960 to 1999, litigated cities had larger African American populations than did unlitigated cities. In 1960, 12% of the residents of unlitigated cities were African Americans, but 18% of the residents of litigated cities were. This six point difference grew to a ten point difference in 1970 and an eleven point difference in 1980 before stabilizing. By 1999, 22% of the residents of unlitigated cities were African Americans, and fully 32% of the residents of litigated cities were.

Police department black employment share did not keep up with these sharp gains. In 1970, litigated departments employed only 7% black officers. Between 1970 and 1980, black employment and population shares increased by equal amounts, and between 1980 and 1990, and again between 1990 and 1999, black employment share grew by more than the city fraction African American. Consequently, from 1970 to 1999, the difference between black employment

[25] See Data Appendix for details.

and population shares, or the representation gap, in litigated departments converged from –0.17 to –0.09. In contrast, the representation gap among unlitigated departments remained roughly constant at –0.10 in 1970 and 1980, and –0.09 in 1990 and 1999. Taking these numbers at face value, a simple difference-in-differences estimate of the long-run impact of litigation on the representation gap is about +0.07.

To economize on space, the estimates in Table 1 are given only at ten-year intervals. For all years for which estimates are available, Figure 3 gives the time series graph of the representation gap separately for litigated and unlitigated departments. The top panel gives estimates for the full sample, which date to 1969. Several aspects of the graph are of note. First, unlitigated departments experience almost no convergence until around 1985. From 1985 to 1993 there is convergence of about +0.015, followed by a slight widening in the mid- to late-1990s. Second, starting sometime near 1977, litigated departments' representation gap converges to that of unlitigated departments. The convergence is very nearly linear in time starting around 1980.

Because the primary empirical goal of this paper is to establish a bound on the contribution of litigation to integration, the most important consideration in evaluating the figure is whether the gains accruing to the litigated are positively biased. The two leading candidate hypotheses to consider are, first, the existence of pre-existing trends, and, second, what might be termed a "social change" hypothesis. I consider each in turn.

Direct evidence regarding the pre-existing trends hypothesis may be obtained from the long sample of cities. As noted above, for these cities I was able to collect information on black employment share dating to the early 1960s.[26] Information on these departments is included in the final three columns of Table 1. While these departments comprise only about a third of those in the overall sample, they have jurisdiction over roughly half of the population of the 314 cities in the full sample. Litigated departments are particularly well-represented: the long sample accounts for about half of all litigated departments, and about 60% of the litigated population.

Among unlitigated departments, roughly one-third are in the long sample, with a similar fraction of the overall population. The South and the Deep South are overrepresented in the long sample, especially among unlitigated cities because so many of the historical estimates are taken from Southern Regional Council surveys.

The estimates from the long sample suggest that black employment share in 1960 was neither zero, nor yet as high as it would be in 1970. Interestingly, the 1960 to 1970 increase in black employment share is larger for unlitigated cities than for litigated cities, despite the fact that the black population share in unlitigated cities rose only marginally from 0.17 to 0.18, while that in litigated cities rose from 0.19 to 0.26. The bottom panel of Figure 3 presents the time series plot of the representation gap for litigated and unlitigated departments in the long sample. The representation gap worsened substantially in litigated departments between 1960 and 1970. During the decade of litigation, 1970 to 1980, the representation gap first slowed its negative slide, and by the end of the decade was on the road to convergence. Unlitigated departments, in contrast, appear to have exhibited a slowly improving representation gap from 1960 to 1985, followed by a more dramatic version of the post-1985 improvements noted in the top panel of Figure 3. The figure does not support the view that post-litigation trends continue what was started in the 1960s.

The second major factor that could lead a difference-in-differences estimator to be positively biased is social change. If coincident with the decade of litigation was the beginning of a new, national hiring regime in which the fraction African American among new hires was only a little below the city fraction African American, then mechanically it would be true that cities with low representation gaps would experience the greatest gains. This concern is a serious one. Litigated cities' 1970 representation gap is close to the 22nd quantile of the distribution for the unlitigated cities. There are 34 unlitigated cities with 1970 representation gap between the 15th and 30th quantiles. In 1970, 1980, 1990, and 1999, the representation gap in these cities was –

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[26] These data are from a variety of sources. See Section III for details.

0.17, –0.18, –0.13, and –0.15, respectively. These numbers are only slightly different from those of litigated cities; only in the 1990s do any differences in representation gap arise. A similar pattern emerges in contrasting with litigated cities alternative groupings of unlitigated cities with similar 1970 representation gap to the litigated. Although heuristic, this suggests that selection on low initial period representation gap is an important consideration. I will return to this consideration in Table 3, below.

Before moving on, however, it is worth noting that a social change hypothesis is in many ways observationally equivalent to a threat effects hypothesis. For example, unlitigated cities that integrate could be doing so because the era of discrimination is over, or because they were hoping to avoid costly litigation. This is an important difficulty. Whereas a threat effects hypothesis is consistent with a major role for the federal judiciary in prompting integration of police departments, a social change hypothesis is not.

The pre-litigation estimates in Table 1 and in Figure 3 suggest that litigation was targeted at big cities in which the police department black employment share was not rising nearly as quickly as the city fraction African American. Aside from the difficulties this poses regarding assessing the effect of litigation, a pattern of targeted enforcement is notable for two reasons. First, it suggests that it may have been possible for a department to forestall a class action employment discrimination suit by hiring more black officers. This does not establish that threat effects contributed to the observed gains in black employment share in unlitigated cities. However, it does mean that such a story is not entirely implausible.

Second, targeted enforcement is not the norm in the federal contractor program; Leonard (1985) characterizes the compliance review program as a "random enforcement process."[27] In comparing the enforcement process of the CRA and the federal contractor program, it may be of interest to economists that judicial enforcement of the CRA is highly decentralized, with the decision to litigate largely in the hands of individuals who feel they have been discriminated

against. It is interesting to note that when the CRA was initially passed, civil rights advocates took a dim view of decentralized enforcement, preferring an Equal Employment Opportunity Commission with cease-and-desist authority.[28]

B. The Timing of Litigation

Having considered comparisons between litigated and unlitigated departments over time, I now turn to an examination of the timing of litigation. Figure 4 gives the histogram of litigation filing dates of the cases under study, both unweighted and weighted by 1970 city population. The cases are clearly concentrated in the 1970s. Moreover, the size of the cities litigated in the 1970s far outstrips those litigated in the 1980s and 1990s.

While the differences between litigated and unlitigated cities in important confounders are quite substantial, the differences among litigated cities litigated at different times are more minor. Table 2 gives weighted means and standard deviations of key variables for five different ranges of filing dates. The regional composition of the cities varies widely by the timing of litigation. However, key confounders such as city fraction African American are quite comparable across at least the first three filing date ranges. The fourth filing date range is also somewhat close to the others in terms of many characteristics, but the fifth filing date range is more similar to the unlitigated cities than to any of the litigated cities. However, in a weighted regression, the contrasts will be focused between the first four filing date ranges because of their much greater size.

With this background, we are now ready to consider estimates of the impact of litigation the representation gap based on the event study, or the evolution of the representation gap before and following the date of litigation, separately by litigation status. The event study is important

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[27] Leonard (1985), p. 372. See also Heckman and Wolpin (1976), reaching a similar conclusion.
[28] Rose (1989), pp. 1133-1134.

for three reasons. First, most parametric models of the impact of litigation suggest that in the presence of variation in timing, a difference-in-differences estimator is negatively biased: some of those departments that will eventually be litigated have not yet been litigated. This is a potential explanation for the fact that the gains in representation gap among the litigated are greater in the 1980s and 1990s than in the 1970s: by the 1980s, most cities that would be litigated had already been litigated. Second, the event study allows us to more carefully assess the contribution of social change to the observed gains among litigated cities—a pure social change hypothesis predicts that litigated cities' gains will not vary by the timing of litigation. Third, the only aspect of litigation that is free from measurement error is the filing date.[29]

The event study estimates I present next correspond to least squares estimates of θ_j in the regression equation

$$y_{it} = \mu_i + \lambda_{rt} + \sum_{j=a}^{b} \theta_j D_{it}^j + \varepsilon_{it} \tag{1.2}$$

where y_{it} denotes the representation gap in department i at time t, $D_{it}^j = D_i 1(t = \tau_i^D + j)$ for $a < j < b$, $D_{it}^a = D_i 1(t \leq \tau_i^D + a)$, $D_{it}^b = D_i 1(t \geq \tau_i^D + b)$, D_i is an indicator for whether the department will ever be the defendant to a class action employment discrimination suit alleging discrimination against African Americans in hiring, and τ_i^D gives the filing date in departments sued.[30] Because not all parameters in (1.2) are identified as written, normalize $\theta_0 = 0$; the sequence θ_j then admits the interpretation of the deviation from the filing date representation gap.[31] I refer to the parameter sequence μ_i as "city effects" and λ_{rt} as "region-by-year effects."

[29] See Data Appendix for a more detailed discussion.
[30] This specification is similar in spirit to that used by Jacobson, LaLonde, and Sullivan (1993). For departments not having judicial interventions, the dates are irrelevant so define them to be zero. It is understood that a<0.
[31] This is a different strategy than that employed by Jacobson, LaLonde, and Sullivan (1993). Because their outcome variable (earnings) was available far before the first treatment date, the omitted category was earnings from long before the treatment. In this application, this strategy is not feasible; the first treatment

Both sequences are nuisance parameters and are to be estimated by fixed effects. The stochastic error term in (1.2) is assumed to satisfy conditions guaranteeing consistency and asymptotic normality of least squares estimates of the μ_i, λ_{rt}, and θ_j.

An important point for estimation is that irregular observation of the outcome data coupled with heterogeneous treatment effects will introduce a composition bias into the graph of the θ_j. An example will serve to illustrate. Oakland, California, litigated in 1969, is observed at litigation lags 0, 4, 5, 6, 8, 12, 13, 15, 16, 18, 20, 21, 22, 24, 26, 28, and 30.[32] In contrast, Chicago, Illinois, litigated in 1970, is observed at litigation lags -1, 3, 4, 5, 7, 10, 11, 12, 14, 15, 17, 19, 20, 21, 23, 25, 27, and 29, which coincide with Oakland's only at 4, 5, 12, 15, 20, and 21. If Chicago and Oakland exhibit different gains in representation gap following litigation, then the event study graph will oscillate about the average effect of the two. Given the slow evolution of police department black employment share, a natural method to correct for the composition bias is to linearly interpolate between observation years, taking care to correct the standard errors for both the induced serial correlation and degrees of freedom.

Another source of composition bias in the graph of the sequence θ_j is the lack of historical data on early-litigated cities, and, to a lesser extent, the lack of future data on late-litigated cities. For this reason, in what follows I assign more weight to pre-litigation estimates obtained from the long sample.

The top panel of Figure 5 gives estimates of four specifications of equation (1.2) based on the full sample. The thick solid line gives estimates from a year effects specification, the thin dashed line gives estimates from a region-by-year effects specification, the thick line with circles gives estimates from a division-by-year specification, and the dashed line with triangles gives

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date (1969) is also the first year for which data on African American employment share in police departments is available for the full sample.

[32] The litigation lags listed assume use only of the full sample data set. Estimates for the 1960s are also available for both Oakland and Chicago, as both are in the long sample.

estimates from a state-by-year specification. Although the four specifications give estimates that are rather different in their magnitude, the direction of the estimated effects is quite similar. In the first decade after litigation, black employment share begins to converge towards the city fraction African American. In the second and third decades the improvement becomes marked. Although the full sample is representative of all departments, the pre-litigation estimates are subject to substantial composition bias. For example, Boston estimates the first litigation lag, but no further; Miami the second; New York the third; Houston the fourth; Birmingham the fifth; Pittsburgh the sixth, etc.

For this reason the pre-litigation estimates for the long sample, given in the bottom panel of Figure 5, are to be preferred to those in the full sample. The availability of data on the outcome dating to 1960 means that only the tenth and eleventh leads of litigation are affected by composition bias. The estimates strongly suggest that litigation was not preceded by representation gap convergence. In the decade prior to litigation, the representation gap appears to have been flat or perhaps even widening. The post litigation estimates are not identical to those of the top panel, but are similar in terms of magnitude and direction. An important difference between the estimates in the top and bottom panels is that the four specifications in the long sample give estimates that are more widely varying than do those in the full sample. For example, while the region-by-year effects models are similar in the long and full samples, the state-by-year effects models are much larger in the long sample. Conversely, the year effects models are much smaller in the long sample than in the full.

Because of the near linearity of the estimate sequences graphed in Figure 5, it is natural to pool the individual estimates to estimate the post-litigation and pre-litigation slopes. Panel A of Table 3 presents a variety of estimates of the post-litigation slope for the full sample. Following the pattern of the specifications in Figure 5, Model 1 includes year effects only, Model 2 region-by-year effects, Model 3 division-by-year effects, and Model 4 state-by-year effects. The first four columns are the estimates for all 314 cities in the full sample. The estimated slope

is in the range 0.07 to 0.12, with a standard error of about 0.02. The reported standard errors are based on the variance estimate of $\left(\sum_i w_i X_i' X_i\right)^{-1}\left(\sum_i w_i X_i' A_i X_i\right)\left(\sum_i w_i X_i' X_i\right)^{-1}$ where A_i is the outer product of the residuals for the *i*th city, w_i is a weight that sums to unity, and X_i is the matrix of time series observations for the *i*th city. Because there are a moderate to large number of cities in all specifications in Table 3, conventional t-ratio tests using these standard errors will exhibit approximately correct size regardless of the form of the heteroskedasticity and serial correlation within each city, provided that the residuals across cities are sufficiently uncorrelated.[33]

Columns (5) to (8) exclude all unlitigated cities from the estimation. The litigation effects for these models, then, are based purely off of the timing of the litigation among the litigated. This has some appeal: we view with some caution estimates that rely on using the unlitigated as a model of the counterfactual evolution of the representation gap among the litigated, had they in fact never been litigated. Nevertheless, caution is warranted in interpreting these estimates as well. Identification leans on the definition of the endpoints of the event study graph, which are usually thought of as irrelevant nuisance parameters. To see this, suppose that we estimate the pre- and post-litigation slopes in one step, by regressing y_{it} on city effects, year effects, and on the pre-and post-litigation trends, $W_{it}^1 = D_i(t-\tau_i^D)1(a < t \le \tau_i^D) + D_i a 1(t \le a)$ and $W_{it}^2 = D_i(t-\tau_i^D)1(\tau_i^D < t \le b) + D_i b 1(t \ge b)$, where $a < 0$ and $b > 0$ give the endpoints of the event study graph. As $a \to -\infty$ and $b \to \infty$, $W_{it}^1 + W_{it}^2 \to D_i(t-\tau_i^D)$, which, in a sample with $D_i = 1$ for each city, is linearly dependent on city and year effects. Alternatively, a researcher who did not begin with the event study graph might have defined the regressors more

[33] The only potential exception to this is the estimates in columns (21) to (24), which are based on only the 43 litigated cities in the long sample. For these estimates, the so-called clustered standard errors reported may be slightly too small.

naturally as $\tilde{W}_{it}^{1} = D_i(t-\tau_i^D)1(t<\tau_i^D)$ and $\tilde{W}_{it}^{2} = D_i(t-\tau_i^D)1(t>\tau_i^D)$. This model is similarly unidentified: $\tilde{W}_{it}^{1} + \tilde{W}_{it}^{2} = D_i(t-\tau_i^D)$.

With these caveats in mind, it is interesting to note that the estimates for the 92 litigated cities are substantially larger than the estimates that include all 314 cities. Estimates using all 314 cities can only differ from those using the 92 litigated cities due to different estimated year effects (or region-by-year effects, etc.). It is apparent that the year effects implicit in the estimates for the 92 litigated cities exhibit somewhat less of an upward trend than do the year effects from estimates for all 314 cities.

A common pattern to the estimates from both the full and the long samples is that the richer the set of geographical controls, the larger the estimated effects. For example, the state-by-year effects specification (Model 4) suggests that the impact of litigation is at least twice that suggested by the year effects specification (Model 1). This pattern is particularly pronounced for the long sample estimates in the bottom panel. This speaks against the existence of a litigation threat effect. One would generally presume that Mobile would be more likely to integrate following the litigation of Birmingham than would Denver. We will return to this consideration below, in sub-section III.D.

Panel B of Table 3 reports post-litigation trend estimates for subsets of cities in the long sample. These are generally similar to those in Panel A. However, as could be guessed by inspection of Figure 5, Model 4 estimates for the long sample are substantially larger than those for the full sample, and Model 1 estimates are substantially smaller. As for the full sample, the estimated impact of litigation is altered only slightly when we restrict our attention to litigated cities (columns (21) to (24)) and when we restrict our attention to cities contributing to identification of Model 4 (columns (29) to (32)).

Because of the greater confidence in the pre-litigation estimates for the long sample, panel B also presents estimates of the post-litigation trend minus the pre-litigation trend. Because

of the negative estimated pre-litigation trends, in all but one case (column (21)) these are larger than the post-litigation trend. There may be some debate regarding which of the reported coefficients is more related to the difference between the evolution of the representation gap in litigated cities given litigation and the evolution of the representation gap in litigated cities given no litigation. However, because of the existence of alternatives to litigation, I believe that the post-litigation trends are a more reasonable estimate of this quantity than the difference between the post- and pre-litigation trends. For example, using the difference as a policy impact measure assumes that in the absence of litigation the representation gap would have continued its negative slide unabated. This seems implausible. First, on a technical point, the representation gap is bounded from below by –1. Second, and more substantively, during much of the post-litigation era, a very large gap between the police department black employment share and the city fraction African American would have presented a political problem for elected officials. It is hard to imagine that nothing would have been done to reduce a gross representation gap, unless the counterfactual world in which the federal courts did not aggressively enforce antidiscrimination law was also one in which, for example, politicians were not responsive to black voters. In this paper, I do not consider such a sweeping counterfactual model.

C. Specification Checks

Because the dependent variable in the above estimation is the simple difference between black employment share and city fraction African American, the estimates presented are potentially driven by changes in city demographics, which presumably do not respond to litigation. The most direct way to address this concern is to re-estimate equation (1.2) with city fraction African American replacing the representation gap. In results not reported, such estimates are quite small in magnitude, and generally positive in the post-litigation period, which would lead the estimates presented in Figure 5 and in Table 3 to be negatively biased, not biased.

For the full sample, the phantom effect of litigation on city fraction African American generally does not exceed 2% in magnitude, and for all but the specification of Model 1, are closer to 1% of 0.5%. For the long sample, the phantom effect is slightly larger in magnitude, closer to 1% or 1.5%, and estimates corresponding to Models 2 and 3 have negative estimates after the 14^{th} lag. Estimates corresponding to Models 1 and 4 have uniformly positive estimates in the post-litigation period. The pre-litigation estimates for both the full and the long sample are slightly negative and generally about –1% to –1.5%. It appears that some portion of the negative pre-litigation trend may be explained by this phenomenon. Re-estimating equation (1.2) with police department black employment share as the dependent variable and including city fraction African American as a control results in a coefficient quite close to unity for city fraction African American. As surmised, the pre-litigation event study estimates from this specification are slightly less positive than those graphed in Figure 5. The robustness of the results to the manner in which we control for city fraction African American means that the estimates of the impact of litigation reported in this paper apply equally to the representation gap and to police department black employment share.

In the discussion of the estimates in Section III.A, I specifically contrasted estimates from Models 1 and 4 and concluded that it was surprising that estimates from Model 4 were larger. Technically speaking, such contrasts are not entirely appropriate, because an implicit sample selection rule underlies Model 4: some states have only one city, some states have multiple cities of which none are litigated, and some states have multiple cities of which all are litigated at the same date. Model 4 is implicitly estimated only over the subset of cities not in such states. In order to compare Models 1 and 4, it is thus appropriate to isolate those cities actually contributing to the identification of Model 4, and estimate Models 1 through 4 on that subsample. This is done in columns (9) to (12) of Table 3 (full sample) and again in columns (25) to (28) (long sample). These estimates are largely similar to those in columns (1) to (4) and (17) to (20), respectively.

Thus, the conclusion that the estimates for Model 4 are larger than those for Model 1 is not a statistical artifact of sample selection.[34]

As emphasized previously, the most important consideration in evaluating the evidence in Figure 5 and Table 3 is the extent to which the estimates might be positively biased. For, if we conclude that they are negatively biased, then we have attained our goal: to bound the contribution of litigation to the integration of police departments in the United States. The most likely cause of positive bias in the estimates presented so far pertains to the fact that litigated departments are drawn from the bottom half of the 1969 representation gap distribution. I turn next to an evaluation of the extent to which this fact can account for the magnitude of the estimates presented.

In Section III.A, above, I noted that a subset of unlitigated cities drawn from the 15th to 30th quantiles of the 1969 representation gap distribution exhibit similar gains to the litigated until the 1990s. One interpretation of the improvements in representation gap for these unlitigated cities is litigation threat effects: because these cities' representation gaps were close to those of the litigated, they deemed litigation likely and sought to integrate in order to avoid the costs of litigation. According to this view, the relative erosion of the representation gap in the 1990s would reflect the erosion of the likelihood of litigation.

A strong argument against interpreting these facts as due to litigation threat effects could be made on philosophic-scientific grounds: a threat effects hypothesis is a classic example of inductive reasoning. For example, an equally plausible interpretation of the gains accruing to the unlitigated with low initial representation gaps is social change. Yet a social change hypothesis is not consistent with a major role for litigation in integrating police departments, while a litigation threat effects hypothesis is. We should be wary of the threat effects hypothesis for this reason. Moreover, because threat effects and social change will usually be observationally equivalent,

[34] Panel A also presents estimates in columns (13) to (16). These estimates will be addressed below.

and because I argue in this paper that the courts played an important role in integrating police departments, I interpret the gains accruing to the unlitigated as being due to social change.

Given this stance, it is important to attempt to gauge how much of the litigation's estimated may be attributed to the fact that the litigated are drawn from the bottom half of the 1969 representation gap distribution. A very natural way to do this is to assign weights to the unlitigated, where the weights are chosen so that the reweighted distribution of 1969 representation gap for the unlitigated is similar to the distribution for the litigated.[35]

Columns (13) to (16) of Table 3 give reweighted estimates of the post-litigation slope for the usual four specifications. Consistent with the ideas discussed above, the reweighting was done to equalize the 1969 representation gap distribution for the litigated and unlitigated departments.[36] The weights in the proof above involve the conditional and unconditional probability of litigation. To implement the idea of the proof, I estimated the conditional probability of litigation using a logit model and a third-degree polynomial in 1969 representation gap. For the unconditional probability, I used the sample average. The estimates from this reweighting scheme suggest that the 25-year gain in black employment share is somewhat smaller than previously estimated. For example, the difference between Model 2 in column (2) (usual weights) and Model 2 in column (14) (reweighting the unlitigated to achieve balance) is about +0.026; that between the estimates for Model 4 is about +0.02. A similar reweighting exercise is

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[35] While this idea has great currency among readers of the program evaluation literature, I do not know of a reference for it. A direct proof builds on the ideas in Theorem 2 of Rosenbaum and Rubin (1983). Let X denote covariates that are to be balanced, D a binary variable, and $g(\cdot)$ a function. By iterated expectations, $E[g(X)(1-D)\frac{p}{1-p}] = E[g(X)p]$, where $p \equiv E[D \mid X]$. Similarly, $E[g(X)D] = E[g(X)p]$. But then $E[g(X) \mid D=1] = E[g(X)w \mid D=0]$, where $w \equiv \frac{p}{1-p}\frac{1-\pi}{\pi}$, $\pi \equiv P(D=1)$. Since this line of argument works for any function $g(\cdot)$, this establishes the equality of the reweighted distributions. Like Theorem 2 of Rosenbaum and Rubin, this proof follows directly from the fact that D is binary and that X and D possess a joint distribution; I would like to stress that this line of reasoning is entirely mute regarding the appropriate interpretation of reweighted contrasts.

[36] Because all estimates in the paper are weighted by 1970 city population, the weights assigned to the unlitigated are the product of population and the weighting factor given in the proof above.

performed in columns (29) to (32) for the long sample.[37] These estimates, too, suggest that the degree of overstatement in the original estimates is about +0.02. Unreported event study estimates of the impact of litigation on representation gap, weighting by the same factor, are visually quite similar to those presented in Figure 5.

D. A Direct Test of the Litigation Threat Effect

Although a litigation threat effects hypothesis is difficult to test, it is beyond question that they are of great substantive interest. The Employment Section of the Civil Rights Division of the Department of Justice employed perhaps twenty-five attorneys in the 1970s.[38] Given these resource constraints on the federal government, and the organizational and procedural difficulties involved with the bringing of a class action lawsuit by private litigants, there was little chance of litigating all police departments engaged in employment practices with disparate impacts.

Testing a litigation threat effects hypothesis requires that we specify how beliefs regarding threats are formed. While we have a generally poor understanding of beliefs formation, a natural starting point in this application is to consider whether there is any evidence that unlitigated cities appear to have responded to the litigation of cities in the same federal district.[39]

To test this idea, I considered the subsets of unlitigated cities in the full and long samples. To each unlitigated city, I sought a match of a litigated city in the same state or sub-state area. If a match was found, I assigned to the unlitigated city the litigation date of the litigated city. If multiple matches were found, I assigned to the unlitigated city the minimum of the dates.

This generated a sample of unlitigated cities in the full and long samples with neighbor-litigation dates. Not all cities were located in the same state or sub-state area as litigated cities;

[37] The logit was of course estimated separately for the long sample and new reweighting weights generated.
[38] Rose (1985).
[39] The federal district courts are organized along geographical lines. Each state has at least one district. For example, the 314 cities under study here are located in 44 states and in 75 federal districts.

for example, no city in Rhode Island was litigated. These cities play the role of the unthreatened in this analysis.

Figure 6 presents the event study of the impact of neighbor-litigation on the representation gap, for both full and long sample cities. These estimates are obtained by estimating equation (1.2) over the sample of unlitigated cities. Cities that were unable to be matched to litigated cities in the same state or sub-state area were assigned $D_i = 0$; cities able to be matched were assigned $D_i = 1$ and a litigation date that corresponding to the minimum litigation date of their matches. The results in Figure 6 are surprisingly inconsistent with a threat effects hypothesis. The post-litigation estimates for the full sample unlitigated cities, given in the top panel, never exceed 0.01. The pre-litigation estimates for the full sample are occasionally large, but these appear to be related to composition bias. Among unlitigated cities in the long sample, given in the bottom panel of Figure 6, there appears to be very little pre-litigation trend. Interestingly, the estimated post-litigation effects are negative, not positive, for both Model 3 (division-by-year effects) and for Model 4 (state-by-year effects).

E. Summary of Results from this Section

I draw five main empirical conclusions from the estimates presented in this section of the paper. First, the litigation of the 1970s was targeted at big cities with large and growing African American populations and low black employment share. This suggests that one may think of the judicial enforcement as having targeted those employers with the largest number of potential black jobs at stake. Second, despite the substantial initial differences between litigated and unlitigated cities, by 1999 litigated cities' representation gap was largely equal to that of unlitigated cities. Third, this convergence appears to have occurred almost entirely in the years following litigation. Fourth, although unlitigated cities with large initial representation gaps

exhibit more convergence than cities with small gaps, this pattern is not strong enough to account for the gains accruing to litigated cities. Fifth, there is little evidence that unlitigated cities exhibited a behavioral response to the litigation of cities located near them.

In sum, the evidence supports the view that litigation played a major role in integrating police departments. Were there unintended consequences to the judiciary's aggressive interventions?

IV. The Impact of Litigation on Crime Rates

The primary goal of the judicial interventions under study has been to induce police departments to hire more black officers. However, it is not inconceivable that the effort inadvertently led to more crime.[40] For example, written police department entrance examinations, required by state and local civil service ordinances in virtually all jurisdictions, appear to exhibit sizeable black-white test score gaps.[41] If the entrance examinations are predictors of job performance, hiring quotas with bite would inevitably lead to a downgrading in the quality of a police force, by requiring departments to hire individuals whose low test scores would otherwise have prevented their selection.

On the other hand, it has been argued that because of the high arrest rate of African American men and the high crime rate in African American neighborhoods, black officers are an essential element in a police department's ability to fight crime. Such a view was espoused by Gunnar Myrdal in 1944, by the pioneering Southern police chiefs who hired African American officers in the 1950s, by the President's Commission on Law Enforcement and Administration of

[40] Lott (2000) argues explicitly that crime increased as a result of judicial interventions in which the Justice Department was involved.

[41] To my knowledge, Kephart (1957) is the first to relate black under-representation on police forces to low average test scores. Speaking of Philadelphia, he notes "under-representation seems to stem not from discriminatory personnel practices, but from the failure of large numbers of Negroes to qualify on the civil service examinations." (p. 35). For a

Justice in 1967, by the National Advisory Commission on Civil Disorders (better known as the Kerner Commission) in 1968, by the National Advisory Commission on Criminal Justice Standards and Goals in 1973, and by many of the judges deciding these cases.[42]

Yet even if the crime-fighting ability of African Americans is better than that of whites, litigation could lead to more crime through reduced morale. Samuel Williams, long-time president of the Los Angeles Board of Commissioners and one of the founders of the National Organization of Black Law Enforcement Executives, argued forcefully in 1975 that

> [t]he entrance of minorities into a department under a judge-fashioned statistical umbrella can only lead to an organization that will be divided for the foreseeable future. I am talking here about one and two generations at the least, twenty to fifty years. I am talking of departments torn by factions and laced with angry mutterings. I am talking about police departments which are deprived of that crucial cooperation among brother officers so critically essential to effective service.[43]

In an observational study such as this, it will be extremely difficult to plausibly disentangle these various factors. However, in terms of a program evaluation of the judicial intervention, *which* of these factors affects crime is less relevant than *whether* they affect crime, taken together. It is this question that appears answerable.

In what follows, I first present new historical evidence on the distribution of test scores by race for the New York City Police Department entrance examination. These data allow direct calculation of the difference in the distribution of test scores of newly hired officers under various hiring rules. Of particular interest is the distribution of test scores under the rank-order hiring rule traditionally used by police departments; and the distribution of test scores under an

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[42] Myrdal (1944), p. 543. Note that Myrdal's conclusions are based in large part upon Raper (1940). Comments of several police chiefs in 1954 may be found in Southern Regional Council (no date), Reel 22. The most literate comment on point is that of the chief of the Miami Police Department, at that time the leading Southern employer of African American police officers. "Our negro police unit has performed a commendable service for the past nine years. The crime rate has decreased in the negro sections." In 1963, the chief of police in Greenville, Mississippi, argued that "[o]ne of the things that police all over the Nation know is that Negro policemen can spot trouble in the Negro district faster and do what is needed [better] than whites." President's Commission on Law Enforcement and Administration of Justice (1967), p. 167, quoting from the lost 1963 U.S. Civil Rights Commission publication, "Administration of Justice Staff Report." Also see National Advisory Commission on Civil Disorders (1968), pp. 165-167, and National Advisory Commission on Criminal Justice Standards and Goals (1973), pp. 329-333.

aggressive court-ordered hiring quota. Because these two distributions turn out to be rather similar, I conclude that it is unlikely that litigation would have exerted a detectable effect on crime rates, even if the entrance examinations circumvented by court orders were substantially predictive of ability as a crime-fighter. Second, I consider direct estimation of the impact of litigation on crime rates. As suggested by the analysis of the distribution of test scores, however, there appears to be no discernible effect.

A. Test Scores by Race on the NYCPD Entrance Examination: 1968-1999

That police departments hire in rank order by entrance examination score may establish a general presumption that, in the absence of litigation, police departments would have hired the most qualified applicants. If true, then court-ordered hiring quotas likely reduced the quality of entering officers. Depending on how responsive crime rates are to the quality of a city's police force, one might then expect that quotas with bite to lead directly to increased crime. But how much bite would an average quota have had?

The answer depends on the extent to which the African American test score distribution is different from the white distribution. If, for example, the distributions are not too different, then the average ability of those hired under the quota and those not hired under the quota would be similar. If, however, the distributions are very different, then a believer in the job-relatedness of the entrance examination would be led to question the competence of the average quota beneficiary.

Is it possible to learn about the distribution of test scores by race on police department entrance examinations? Unfortunately, the answer is in general no. However, for the New York City Police Department (NYCPD), I have been able to obtain information on the distribution of

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[43] Samuel L. Williams, "Law Enforcement and Affirmative Action," *The Police Chief*, Vol. 42, No. 2, February 1975, pp. 72-73, quoted in Vila and Morris (1999).

test scores by race for the 1968, 1970, 1979, and 1998-99 examinations.[44] While the distribution of test scores in New York are likely not representative of all departments, some 8% of the nation's police officers work for the NYCPD, and in 1999, the NYCPD accounted for fully 20% of the officers in the 314 cities studied here.

For all examinations for which I have data, Table 4 gives the fraction of white and black applicants with scores in each decile of the overall test score distribution. The numbers in the Table suggest that in both the 1968 and 1970 examinations, African Americans constituted roughly 5% of those in the upper three deciles and 20% of all applicants.

In 1972, the Guardians Association and the Hispanic Society of the NYCPD mounted a legal challenge to these examinations. The court found that the city was in violation of Title VII because the exams were insufficiently job-related; however, the case was declared moot because of the mid-1970s fiscal crisis, which was to preclude hiring in the NYCPD for a period of several years. When the City was again in a financial position to contemplate hiring, in 1979, the Personnel Department went to great lengths to design an exam that was job related:

> Exam No. 8155… is a "second generation" selection procedure. Despite the various flaws in construction of the test, it is clear that some attempt was made to develop the test with recognition of at least some of the standards that courts had established in the first wave of Title VII cases. Aware that the validity of the test would likely have to be demonstrated, the City performed an extensive job analysis, consciously used… concepts [from the EEOC Uniform Guidelines on Employee Selection Procedures] in determining the qualities that were being tested for, and attempted to eliminate extraneous variables, such as the applicant's prior knowledge, his reading level, and his ability to complete the test in a relatively short amount of time.[45]

However, after the test was administered, the Guardians again challenged the test under Title VII, because this test—although it exhibited less disparate impact than the 1968 and 1970 exams—continued to pass whites at a much higher rate than African Americans. For example, non-whites constituted only 18% of those among the top three deciles, despite being 34% of applicants.

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[44] The 1968 and 1970 data are from Chaiken and Cohen (1973). The 1979 data are from *Guardians Association of the New York City Police Department v. the New York City Police Department*, 630 F. 2d 79, 103 (1980). The 1998-99 data were obtained through Freedom of Information Act Request.

[45] 630 F.2d 79, 89 (2nd Cir. 1980).

Interestingly, the black-white test score gap on the NYCPD entrance exam does not appear to have changed much since 1979. Figure 7 plots kernel density estimates of test score distributions for whites, and for African Americans and Latinos combined, for the 1979 and 1998-99 examinations. The distributions are virtually identical.

Using the data underlying Figure 7, it is a simple matter to simulate the impact of quota hiring on the distribution of test scores among the newly hired. For example, from the 1979 exam, the NYCPD anticipated that all those with scores of at least 94 would move on to the next phase in the selection process. Thus, all new hires were, in the absence of court intervention, to be drawn from the extreme upper tail of the test score distribution. The range of scores would have naturally been from a low of 94 to a high of 110, with an average score of 96.8. In 1980, in response to the evidence regarding the difference in the distribution of test scores by race, the district court judge ordered that 50% of new hires be African American or Latino.[46] I estimate that a cohort selected under this aggressive quota would have had scores ranging from 87 to 110 and an average of 95.3.

Thus, it appears that even with an aggressive hiring quota and a visually discernible difference in test score distributions, the average qualifications of an entering cohort would only be affected in relatively minor ways. Given these minor discrepancies in the test score distributions of the hypothetically newly hired, it would perhaps be surprising if litigation exerted much of an impact on crime rates.

B. Descriptive Statistics and Estimation Results

I turn now to the descriptive statistics on crime rates by litigation status and litigation timing noted in Tables 1 and 2. For both the full and long samples, Panel C of Table 1 gives the

log crime rate for litigated and unlitigated cities at ten-year intervals beginning in 1960. For the full sample, it appears that crime rates in litigated and unlitigated cities differed by less than ten percent until the 1990s, when crime rates fell dramatically in the litigated cities. Among cities in the long sample, the unlitigated had higher crime than the litigated in 1960. However, beginning in 1970 the crime rates in litigated and unlitigated cities mirror each other.

In contrast to the close correspondence between crime rates in litigated and unlitigated cities, crime rates do appear to differ by the timing of litigation. Panel C of Table 2 gives the log crime rate for five categories of litigation date ranges at ten-year intervals beginning in 1960. Cities litigated in 1977-1980 have a 1960 crime rate that is 30% higher than the estimates for the other four litigation date ranges. Between 1960 and 1970, however, the crime rate in cities that were litigated 1969-1974 and in the 1980s soared relative to that in cities litigated 1977-1980. Between 1970 and 1999, the crime rates in the five city groups appear to move roughly in parallel.

The top panel of Figure 8 gives the log crime rate by litigation status for 1960 to 1999. As suggested by the ten-year interval data in Table 1, there are no discernible differences between litigated and unlitigated cities on average following the onset of litigation. What differences do exist are concentrated in the years before litigation: during the 1960s, the crime rate in litigated cites grew rapidly relative to unlitigated cities. However, already by 1970 the crime rates had equalized. From 1970 to 1994, crime rates in the two sets of cities are virtually identical. Only with the late 1990s drop in crime do any differences in crime rates emerge.

The bottom panel of Figure 8 presents event study estimates of the impact of litigation on crime in cities in the full sample. An important point to note is that there are no issues with composition bias in the pre-litigation estimates: the data on crimes date to 1960 for all 314 cities in the full sample. There appears to have been something of a crime wave in the litigated cities

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[46] *Guardians Association of the New York City Police Department v. New York City Police Department*, 484 F. Supp. 785, 799 (S.D. New York 1980). This example is for illustrative purposes only; the 50%

some four to six years prior to litigation. Following the spike six years prior to litigation, crime rates declined back to zero. Following litigation, the estimates range to either side of zero with little apparent pattern. In sum, there seems to be little evidence that litigation is related to crime rates, either from the simple time series evidence in the top panel, or from the event study estimates.

V. Conclusion and Discussion

This paper has examined the impact of class action employment discrimination litigation on black employment share in 314 large municipal police departments. The evidence points to several key conclusions. First, the litigation of the 1970s was targeted at big cities with large and growing African American populations and low black employment share. Second, despite substantial initial differences between litigated and unlitigated cities, by 1999 litigated cities' representation gap was largely equal to that of unlitigated cities. Third, this convergence appears to have occurred almost entirely in the years following litigation. Fourth, although unlitigated cities with large initial representation gaps exhibit more convergence than cities with small gaps, this pattern is not strong enough to account for the gains accruing to litigated cities. Fifth, there is little evidence supporting a threat effect hypothesis. In particular, litigated cities do not appear to exhibit any gains in black employment share prior to litigation, and unlitigated cities do not appear to integrate following the filing of the first class action employment discrimination suit in their state.

On the whole, I interpret the evidence summarized in this paper as consistent with a major role for the federal courts in integrating police departments in the United States. My preferred estimate of the 25-year gain in police department black employment share is 10 percentage points, and the effect is estimated with some precision. Because of the low attrition

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hiring quota was subsequently softened by the appeals court. 630 F. 2d 79 (2nd Cir. 1980).

rate of police officers, this is consistent with a hiring fraction African American roughly 16 percentage points above the pre-litigation police department fraction black.

In contrast to the estimated impact of litigation on police department black employment share, I find little evidence of an impact of litigation on crime. This may be because of a complex series of effects that offset one another. For example, litigation seems likely to have lowered the morale of incumbent officers, and it seems plausible that—at least conditional on entrance examination score—African American officers were more productive crime fighters than other Americans. However, the analysis in this paper rules out, I believe, one mechanism by which litigation might have been expected to affect crime: as demonstrated in Section IV.A, even aggressive hiring quotas change the test score distribution of new hires only minimally. Unless African Americans fare much better on the New York City Police Department entrance examinations than on the entrance examinations of other cities, this conclusion would seem to hold for police departments generally.

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Data Appendix

The data used in this study come from a variety of sources. Data on police department demographics, the cornerstone of this paper, are obtained primarily from the EEO-4 survey of the Equal Employment Opportunity Commission (EEOC), conducted annually by the EEOC since 1973.[47] These data are confidential, but may be made available to researchers.[48] Prior to this research, electronic files of the EEO-4 survey were available for 1980, 1984, 1985, 1987, 1989, 1990, 1991, 1993, 1995, 1997, and 1999. As part of this research, I traveled to the EEOC and recovered the historic 1973 and 1974 surveys. Remarkably, electronic files of these surveys existed. However, the EEOC had lost the ability to interpret the raw file. I inferred a codebook for these data and was able to construct a full electronic file for both years.[49] In addition to the 1973 and 1974 files, I was able to locate several 9 track reels containing the only known copies of the 1982 EEO-4 survey. Unfortunately, these reels, twenty years old when accessed, had experienced a high degree of physical degradation. Consequently, only half of the records from the 1982 survey are available. I inferred a codebook for these data as well, and a partial electronic file was constructed.

While electronic copies of the EEO-4 survey do not exist for all years, for many of the missing years, microfilm records are contained in the Alexandria warehouse of the EEOC. For the 314 cities under study here, I was able to photocopy the 1975 and 1977 survey forms at the EEOC, and the data from the survey forms was hand-entered into electronic files by a team of research assistants.[50]

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[47]Beginning in 1991, the EEOC has conducted the EEO-4 survey only once every two years. The 1972 EEOA granted authority to the EEOC to collect data from public employers. The 1964 CRA granted the EEOC authority to collect analogous data from private employers, and the EEO-1 survey of private employers has been conducted annually since 1966.

[48] Contact the author for details regarding the requirement for using EEOC confidential data.

[49] These data were stored in an electronic medium that had experienced no physical degradation.

[50] David Card generously paid for my plane fare and for the wages of the research assistants.

The 1980 electronic file contains a number of missing records, especially for large cities.[51] It appears that electronically submitted returns were not included in the electronic file compiled by the contractor for the 1980 survey. Because of these problems, I additionally inspected the 1981 microfilm records for the subset of cities among the 314 under study that were missing from the 1980 survey. Survey forms for these cities were photocopied and hand-entered along with the 1975 and 1977 surveys.[52]

Because the first EEO-4 survey was not conducted until 1973, further data on the number of African American officers in police departments were collected from several historical sources. The most nationally comprehensive of these is a listing in the 1970 *Municipal Yearbook* that details the number of "minority group employees—uniformed" for a large number of police departments. This is unfortunately the only year for which the *Municipal Yearbook* publishes these numbers. I interpret these numbers as the number of African American and Latino officers and have discounted them by the estimated 1973 fraction of African American and Latino officers that were African American. The other nationally comprehensive survey comes from Arthur Raper's unpublished 1940 research on "Negro policemen" upon which Gunnar Myrdal relied in Chapter 25, "The Police and Other Public Contracts," of *An American Dilemma*.[53]

Raper's pioneering research led to a series of surveys of Southern police departments spanning 1947 to 1969. Raper was a longtime writer for the Commission on Interracial Cooperation, which was succeeded in 1944 by the Southern Regional Council (SRC). The SRC continued Raper's surveys of the number of "Negro uniformed policemen, detectives (plainclothes), and policewomen" for Southern municipalities and published the results

[51] Notably, New York and Los Angeles are not included in the 1980 file.
[52] Data for cities located on the same microfilm reel as a city with missing data were also included.
[53] Raper (1940), Myrdal (1944). Comparing 1930 and 1940 Census totals of African American police officers with Raper's estimates suggests that Raper may have identified the employers of some 90% of the black police officers in the country.

approximately annually from 1947 to 1954 in *New South*.[54] Publication of these numbers ceased after 1954, as the SRC focused its efforts on school desegregation and voter registration. However, the sociologist Elliott Rudwick conducted surveys of Southern police departments in 1954, 1959, and 1961 and the SRC published the results in 1962 in *The Unequal Badge*.[55] The SRC then in 1969 attempted what was to be its final survey of Southern police departments. The results of the survey were not published, but these data are available in the recently published microfilm records of the SRC, *Southern Regional Council Papers, 1944-1968*.[56]

For non-Southern cities, data on the number of "Negro police" in 1954 for a small number of cities are published in Kephart (1954) and Kephart (1957). Data on larger cities' black employment share have also been published in the reports of several presidential and gubernatorial commissions: the President's Commission (1967), the Kerner Commission (1968), and the National Advisory Commission (1973). Finally, it appears that the U.S. Commission on Civil Rights conducted a survey of employment of African American police officers in 230 departments in 1962.[57] However, the Commission did not publish this work, the Commission Library does not possess a copy, and the survey results are not part of the holdings of the National Archives' holdings of the Commission's papers.

These historical sources allowed estimation of 1969 black employment share for all 314 cities under study. Because litigation began in 1969, this is an important aspect of the data collection undertaken here. For a subset of 120 cities, referred to in the body of the text as the "long sample," data on black employment share for 1960 to 1969 are available. Data on black employment share were combined with data on city fraction African American (linearly interpolated between Census years).

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[54] Data for 1947 are published in *New South*, October 1947, Vol. 2, No. 10, p. 9; 1948 in September 1948, Vol. 3, No. 9, p. 7; 1949 in September 1949, Vol. 4, No. 9, p. 9; 1950 in September-October 1950, Vol. 5, Nos. 9 and 10, p. 6; 1952 in September 1952, Vol. 7, No. 9, p. 6; 1953 in October-November 1953, Vol. 8, Nos. 10 and 11, pp. 6-7.
[55] Rudwick (1962).
[56] Southern Regional Council (2000), Reel 111(434), pp. 289-298.

The 314 cities for study were selected from among those U.S. municipalities with the largest police and fire departments, on the basis of the Census of Governments from 1972, 1977, 1982, 1987, 1992, and 1997. The specific mechanism for selection was as follows. First, I merged the employment counts by function of government for all municipal respondents in the above years. Average employment levels over the period were computed. There were 328 cities whose average police and fire employment levels placed them in the top 400 nationally of both categories. These cities were then sought in the electronic files of the EEO-4 survey. Satisfactory data for 314 cities was available. Most of the cities unable to be matched are quite small. However, Washington, D.C. and San Francisco were excluded from the analysis because survey responses from these cities were not available until the 1980s.[58]

The resulting sample is comprised of 314 cities in all nine Census regions, and in all but the least populated of the 48 contiguous states.[59] Almost all of the major cities in the United States are included.

To learn which of these cities were litigated I read all published decisions pertaining to employment discrimination brought against the police and fire departments of each city under study. All published decisions are available electronically through commercial providers.[60] Debby Kearney of Boalt Law School provided me with access to the so-called lawschool version of Lexis-Nexis, which allows sophisticated searches by keyword and the ability to Shepardize any decision located. Over 1,300 published decisions were read. All cases cited by these cases as pertaining to protective service employment discrimination are included in the 1,300 decisions consulted. I take this as confirmatory that my survey of published decisions incorporates a large

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[57] President's Commission (1967), pp. 167-168.
[58] The fire and police departments of San Francisco were litigated in 1970 and 1973, respectively. The police department of Washington, D.C. was litigated in 1970.
[59] Montana, North Dakota, Vermont, and Wyoming are all unrepresented. The 2000 population ranks of these states among the 48 contiguous states are 43, 46, 47, and 48, respectively.
[60] The leading providers are Lexis-Nexis and Westlaw.

majority of existing published decisions. Of course, unpublished decisions may nonetheless be an important omission in this data collection strategy.

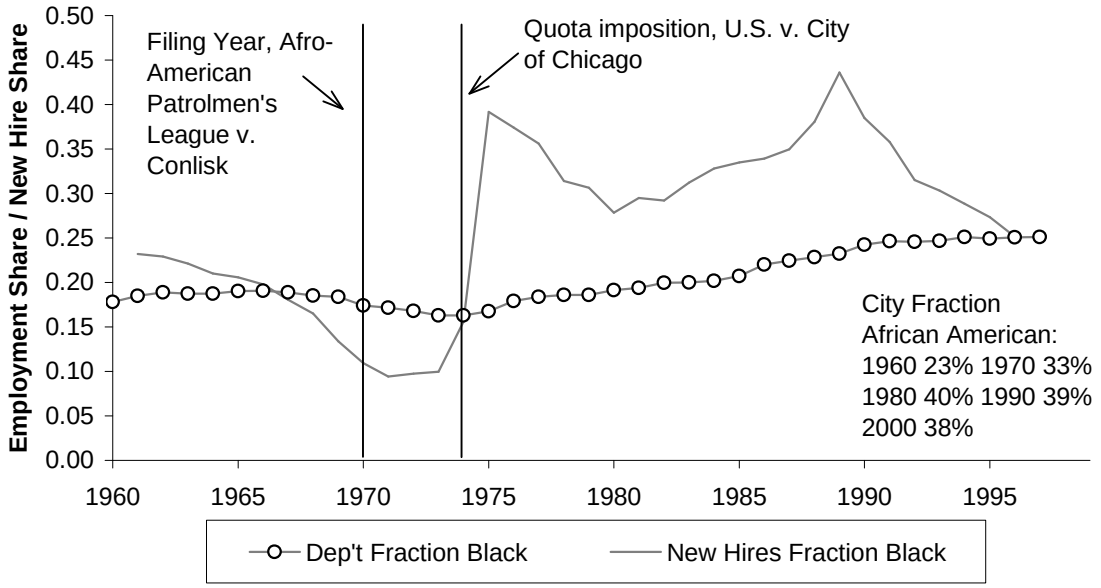
At the outset of this research, I hoped to ascertain the precise quotas required of departments and the precise years that they are in effect. Quotas were used frequently by judges in these cases as a remedy; I estimate that at least 80% of the litigated cities were subject to quota hiring for at least one year. However, because of the thorny problem of unpublished decisions, this aspect of the litigation is much more poorly measured than whether a city was litigated, and the date as of which the litigation began. For virtually every district court for virtually the entire period 1969 to the present, the first two numbers of the unique identifier for a case, the Civil Action Number, are the last two digits of the year. Thus, the location of even one published decision pertaining to a case results in knowledge of the filing year. However, the quotas imposed on departments are not fully disclosed in each decision. Only if a substantially complete published record is available is it possible to learn the precise contours of the requirements of the court. For these reasons, in the present paper, I focus on the two aspects of the litigation that are measured best: whether a department was sued, and the year in which the suit began.

Finally, data on crime and police staffing were obtained from the Uniform Crime Reports (UCR) available from the Inter-university Consortium for Political and Social Research (ICPSR) for 1975 to 1999. Data prior to 1975 are not available from ICPSR, but are available from the Federal Bureau of Investigation (FBI).[61]

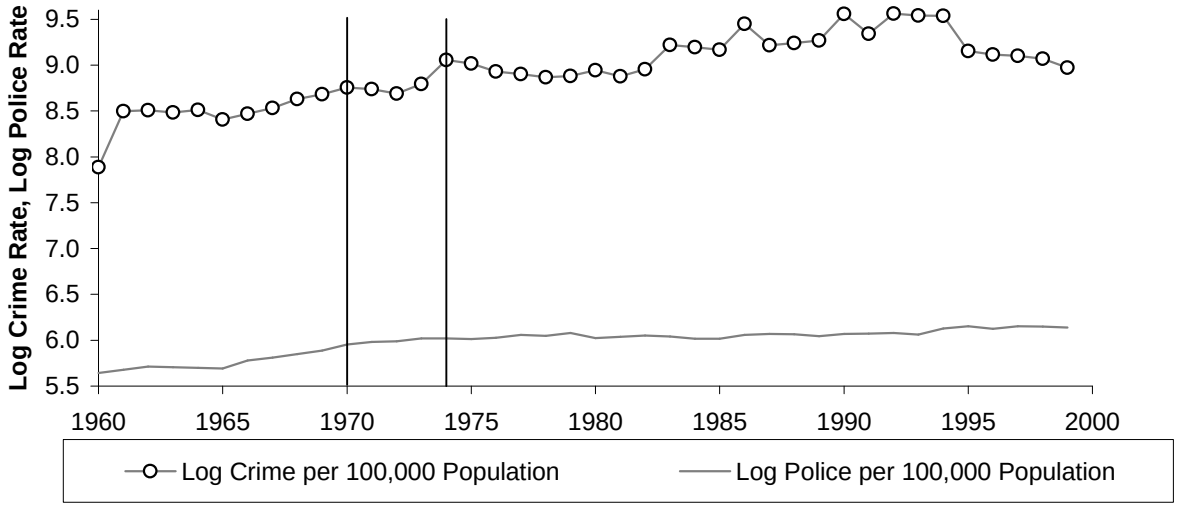
[61] Joanne Vanatta of the Programs Support Section of the FBI deserves special thanks for her help in transferring the files. Per year of data provided, the FBI must charge, per Department of Justice regulations, $140. The data may only be provided on 9 track tape. Special thanks to Rick Kawin and his staff for loading the 15 tapes.

Figure 1. The Chicago Experience

A. Black Employment Share and Black Hiring Share



B. Log Crime Rate and Log Police Rate



Notes: Panel A gives black employment share and black hiring share in Chicago Police Department, 1960 to 1997. Estimates are from an administrative data set obtained with permission of the City of Chicago. Panel B gives log crime per 100,000 city population (linearly interpolated between Census estimates) and log police per 100,000 city population. Estimates are from the Federal Bureau of Investigation Uniform Crime Reports.

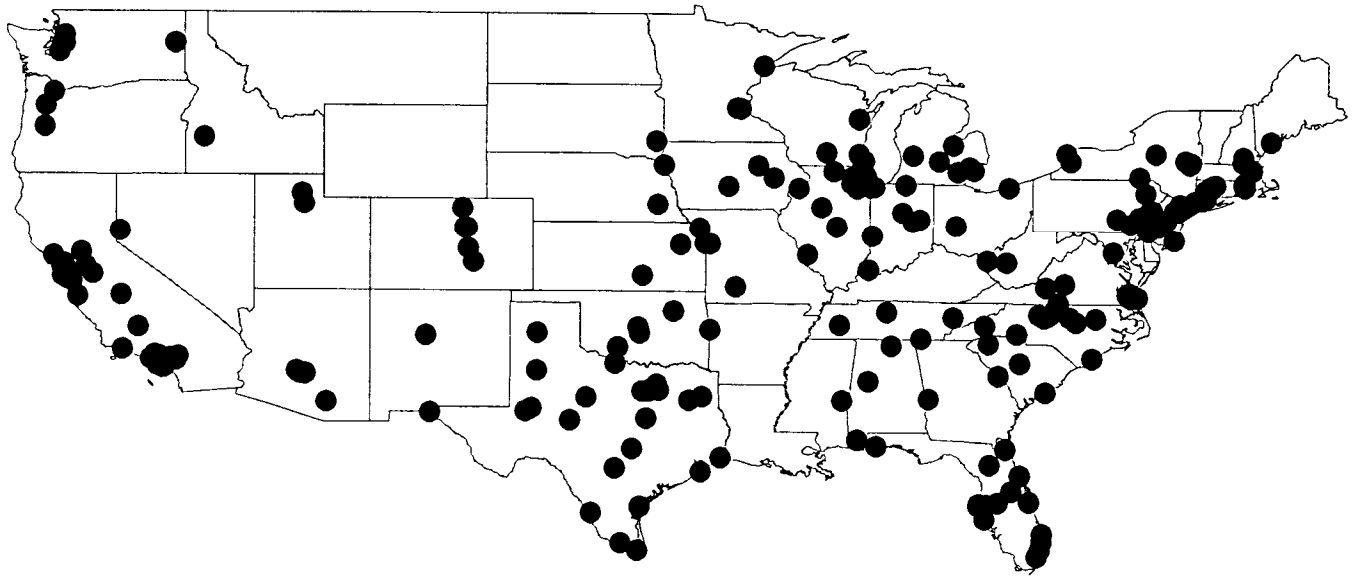
Table 1. Means and Standard Deviations of Key Variables by Litigation Status

		Full Sample			Long Sample		
	Year	All (n=314)	Litigated (n=92)	Unlitigated (n=222)	All (n=120)	Litigated (n=43)	Unlitigated (n=77)
A. City Characteristics							
Fraction South[1]	NA	0.28	0.23	0.33	0.36	0.28	0.52
Fraction Deep South[2]	NA	0.12	0.11	0.13	0.15	0.12	0.20
City Population[3]	1960	1.9	3.8	1.1	2.5	4.9	1.2
in hundred thousands		(5.3)	(9.3)	(1.5)	(7.9)	(12.7)	(2.2)
	1999	2.4	4.1	1.7	3.2	5.2	2.1
		(5.5)	(9.6)	(1.8)	(7.9)	(12.7)	(2.3)
Fraction African	1960	0.16	0.18	0.12	0.19	0.19	0.17
American		(0.11)	(0.10)	(0.12)	(0.11)	(0.09)	(0.14)
	1970	0.20	0.24	0.14	0.23	0.26	0.18
		(0.13)	(0.11)	(0.14)	(0.13)	(0.11)	(0.16)
	1980	0.24	0.29	0.18	0.28	0.31	0.23
		(0.17)	(0.14)	(0.18)	(0.17)	(0.13)	(0.21)
	1990	0.26	0.31	0.20	0.30	0.33	0.26
		(0.18)	(0.15)	(0.20)	(0.18)	(0.13)	(0.24)
	1999	0.28	0.32	0.22	0.31	0.33	0.28
		(0.19)	(0.16)	(0.21)	(0.19)	(0.14)	(0.26)
B. Police Department Characteristics							
Log Sworn Police	1960	5.2	5.3	5.0	5.3	5.4	5.0
per 100,000 Population		(0.4)	(0.4)	(0.3)	(0.4)	(0.4)	(0.4)
	1999	5.6	5.8	5.4	5.8	5.9	5.5
		(0.4)	(0.4)	(0.3)	(0.4)	(0.4)	(0.4)
African American	1960	NA	NA	NA	0.04	0.05	0.03
Employment Share					(0.04)	(0.04)	(0.03)
	1970	0.06	0.07	0.04	0.07	0.07	0.06
		(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
	1980	0.10	0.12	0.08	0.12	0.12	0.11
		(0.08)	(0.06)	(0.09)	(0.09)	(0.07)	(0.12)
	1990	0.15	0.18	0.11	0.17	0.19	0.15
		(0.12)	(0.09)	(0.13)	(0.12)	(0.10)	(0.16)
	1999	0.18	0.23	0.13	0.21	0.23	0.17
		(0.14)	(0.12)	(0.15)	(0.15)	(0.11)	(0.19)
C. City Crime Rate							
Log UCR Index Crimes	1960	7.9	7.8	7.9	7.9	7.8	8.0
per 100,000 Population		(0.4)	(0.4)	(0.5)	(0.4)	(0.3)	(0.5)
	1970	8.7	8.8	8.7	8.8	8.8	8.7
		(0.3)	(0.3)	(0.4)	(0.3)	(0.2)	(0.4)
	1980	9.1	9.1	9.1	9.2	9.2	9.2
		(0.3)	(0.3)	(0.3)	(0.3)	(0.2)	(0.3)
	1990	9.3	9.3	9.2	9.3	9.3	9.3
		(0.3)	(0.3)	(0.3)	(0.3)	(0.3)	(0.3)
	1999	8.9	8.8	9.0	8.8	8.7	9.0
		(0.5)	(0.5)	(0.5)	(0.5)	(0.5)	(0.6)

Notes: Table gives estimated means and standard deviations (parentheses) of variables listed by litigation status. Unless otherwise noted, means and standard deviations are weighted by 1970 city population. [1]South as defined by the Census Bureau. [2]Deep South is defined as Alabama, Mississippi, Louisiana, Arkansas, Tennessee, Georgia, South Carolina, North Carolina, and Virginia. This grouping excludes the District of Columbia and seven states included in the Census South: Delaware, Florida, Maryland, West Virginia, Kentucky, Oklahoma, and Texas. [3]Equally weighted.

Figure 2. Map of Cities by Litigation Status

A. Unlitigated Cities



B. Litigated Cities

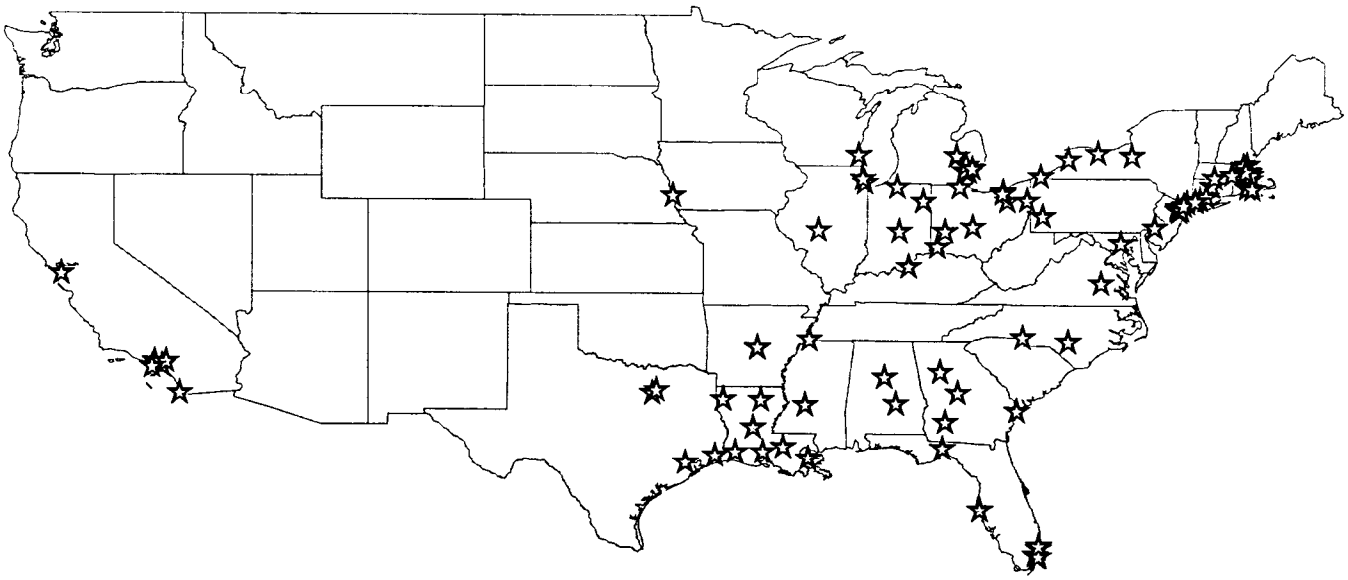
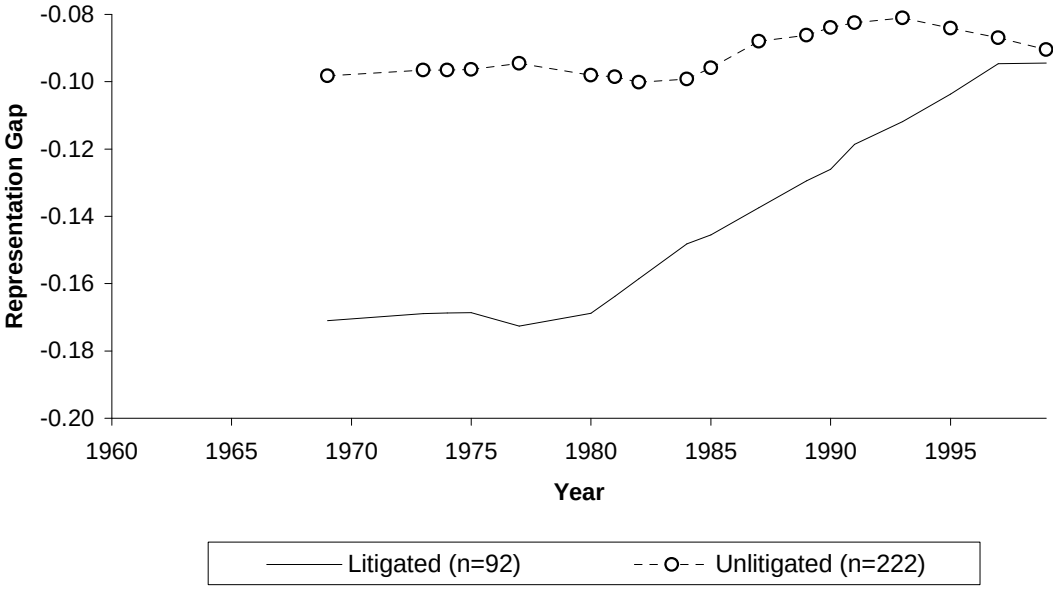
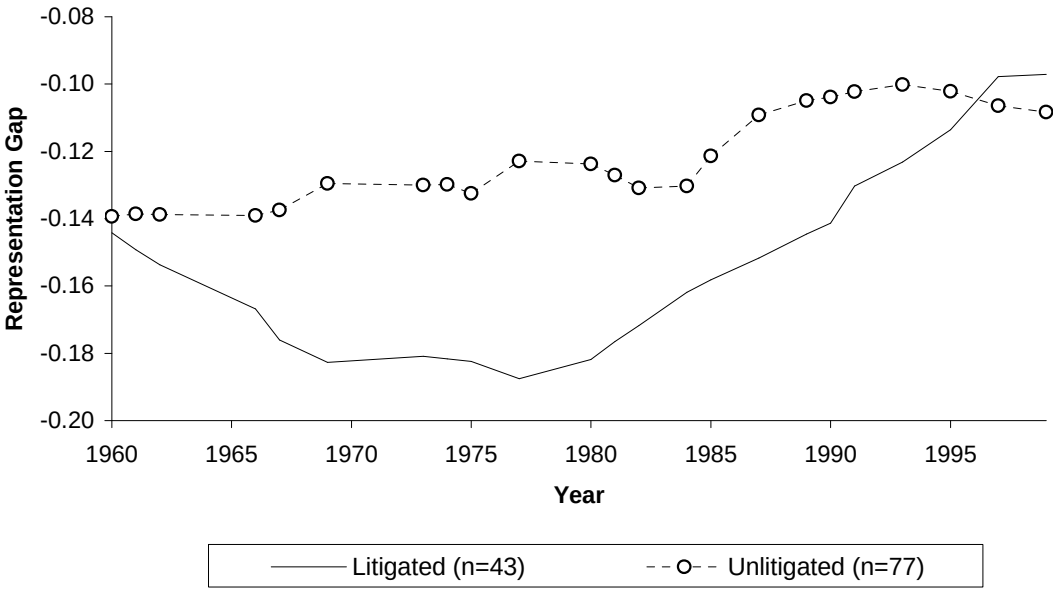


Figure 3. Representation Gap by Litigation Status

A. Full Sample



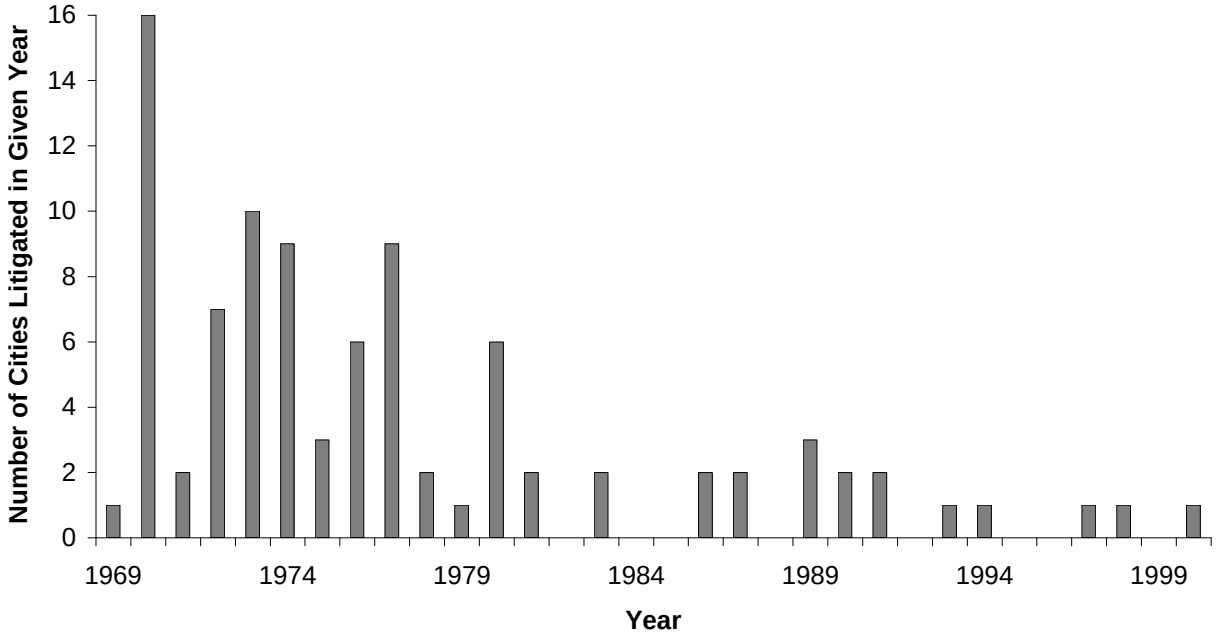
B. Long Sample



Notes: Figure gives time series of representation gap, or difference between police department black employment share and city fraction African American, separately by litigation status, for two samples. The full sample in panel A is comprised of 314 cities with information on black employment share dating to 1969. The long sample in panel B is comprised of 120 cities with information on black employment share dating to 1960.

Figure 4. Histograms of Litigation Dates

A. Unweighted



B. Weighted by 1970 City Population

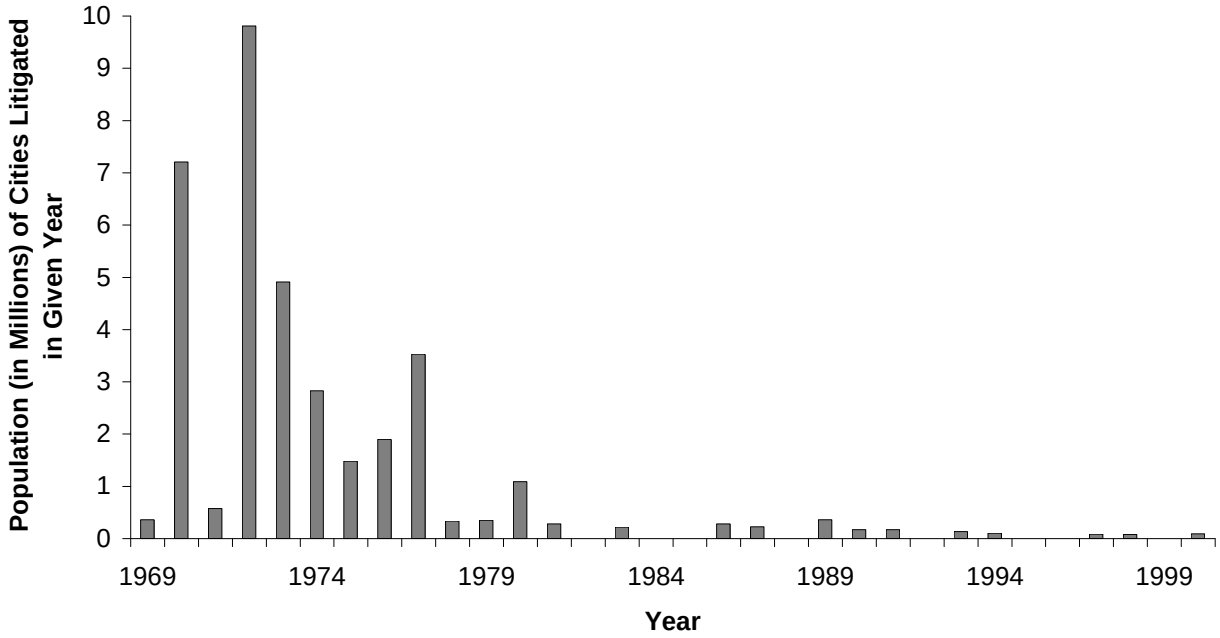


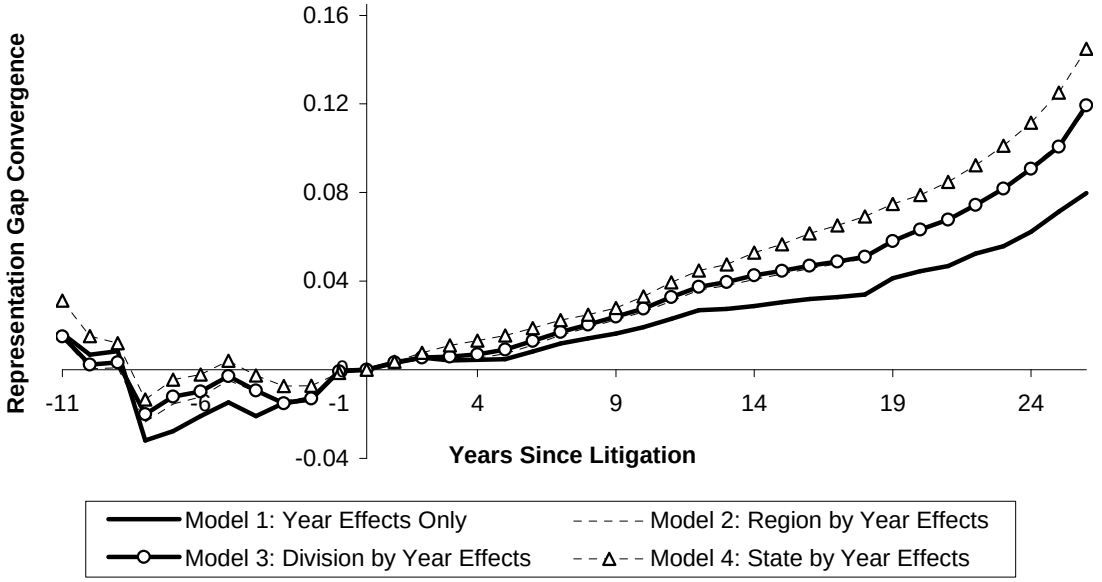
Table 2. Means and Standard Deviations of Key Variables by Litigation Timing

	Year	Litigated 1969-71 (n=19)	Litigated 1972-73 (n=17)	Litigated 1974-76 (n=18)	Litigated 1977-80 (n=18)	Litigated 1981-2000 (n=20)
A. City Characteristics						
Fraction South	NA	0.07	0.24	0.50	0.15	0.23
Fraction Deep South	NA	0.03	0.10	0.25	0.12	0.06
City Population	1960	4.4	8.5	3.1	2.7	0.9
in hundred thousands		(8.8)	(18.1)	(2.4)	(5.6)	(0.5)
	1999	4.0	8.5	3.7	3.4	1.2
		(7.0)	(18.8)	(3.6)	(8.4)	(0.5)
Fraction African	1960	0.20	0.19	0.21	0.15	0.08
American		(0.09)	(0.09)	(0.12)	(0.08)	(0.10)
	1970	0.27	0.27	0.23	0.19	0.12
		(0.11)	(0.11)	(0.11)	(0.07)	(0.13)
	1980	0.32	0.32	0.29	0.20	0.15
		(0.13)	(0.13)	(0.13)	(0.09)	(0.16)
	1990	0.33	0.35	0.32	0.20	0.16
		(0.12)	(0.13)	(0.15)	(0.11)	(0.17)
	1999	0.33	0.36	0.36	0.21	0.19
		(0.12)	(0.14)	(0.18)	(0.14)	(0.18)
B. Police Department Characteristics						
Log Sworn Police	1960	5.6	5.5	5.1	5.1	4.9
per 100,000 Population		(0.2)	(0.4)	(0.3)	(0.2)	(0.4)
	1999	6.0	6.0	5.5	5.6	5.4
		(0.3)	(0.3)	(0.2)	(0.2)	(0.4)
African American	1960	0.08[1]	0.04[1]	0.04[1]	NA	NA
Employment Share		(0.05)	(0.01)	(0.04)		
	1970	0.12	0.06	0.05	0.04	0.04
		(0.07)	(0.03)	(0.03)	(0.02)	(0.05)
	1980	0.16	0.12	0.10	0.09	0.05
		(0.06)	(0.06)	(0.05)	(0.03)	(0.05)
	1990	0.21	0.19	0.19	0.15	0.08
		(0.07)	(0.10)	(0.10)	(0.05)	(0.08)
	1999	0.26	0.25	0.24	0.16	0.09
		(0.09)	(0.11)	(0.15)	(0.06)	(0.08)
C. City Crime Rate						
Log UCR Index Crimes	1960	7.8	7.8	7.8	8.1	7.8
per 100,000 Population		(0.3)	(0.2)	(0.2)	(0.5)	(0.5)
	1970	8.6	8.9	8.7	8.8	8.6
		(0.4)	(0.2)	(0.2)	(0.2)	(0.5)
	1980	9.0	9.2	9.1	9.2	9.1
		(0.3)	(0.1)	(0.3)	(0.2)	(0.5)
	1990	9.3	9.3	9.3	9.2	9.2
		(0.4)	(0.2)	(0.4)	(0.2)	(0.5)
	1999	9.0	8.7	9.1	8.8	8.8
		(0.3)	(0.5)	(0.3)	(0.3)	(0.6)

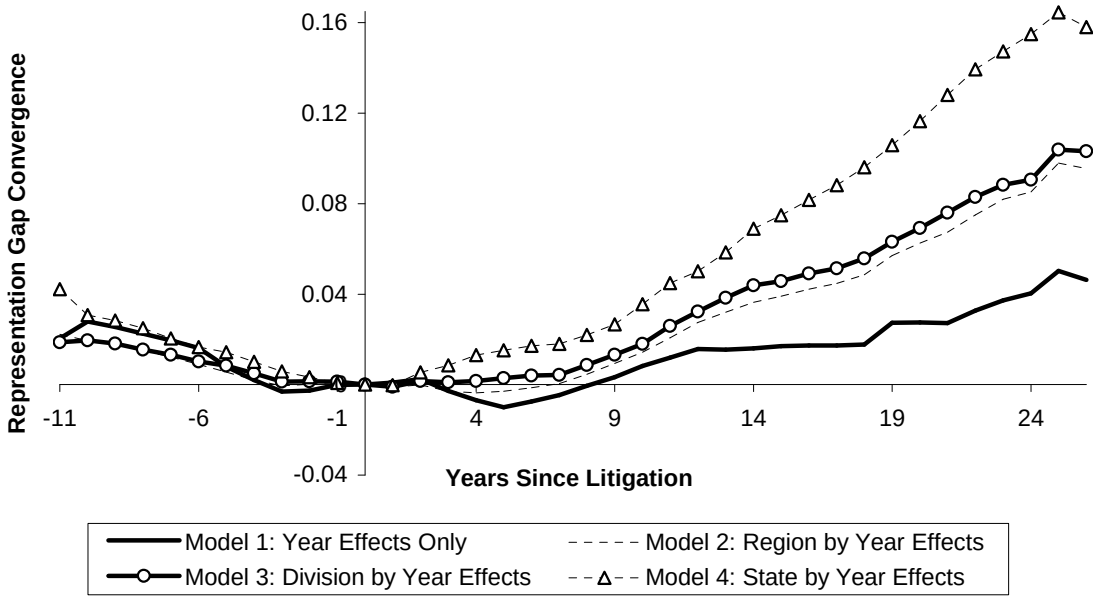
Notes: For 92 litigated cities, table gives estimated means and standard deviations (parentheses) of variables listed by categories of litigation filing date. As in Table 1, all estimates barring those for city population are weighted by 1970 city population. Unlike Table 1, estimates presented are based on the full sample only, unless otherwise noted. [1] Estimates for 1969-71, 1972-73, and 1974-76 taken from the 11, 9, and 8 respective cities in the long sample with litigation dates in the given categories. Estimates for other litigation date ranges omitted because long sample cities do not appear to be representative.

Figure 5. Event Study Estimates of the Impact of Litigation on the Representation Gap

A. Full Sample



B. Long Sample



Notes: Figures give event study estimates of the impact of litigation on representation gap. Model 1 estimates are least squares regression coefficients of the impact of the representation gap on year effects, department effects, and leads and lags of an indicator which is equal to one if the department is litigated in the given year. Models 2, 3, and 4 fully interact year effects with four indicators for the Census regions, nine for the Census divisions, and 50 for the states, respectively.

Table 3. Parametric Estimates of the Impact of Litigation on the Representation Gap

A. Full Sample Estimates

	All 314 Cities				92 Litigated Cities Only			
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 1**	**Model 2**	**Model 3**	**Model 4**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
25 * Post-Litigation Trend	0.071	0.099	0.099	0.120	0.139	0.131	0.120	0.176
	(0.028)	(0.019)	(0.022)	(0.016)	(0.080)	(0.066)	(0.070)	(0.044)

	255 Cities Identifying Model 4				Reweighting the Unlitigated			
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 1**	**Model 2**	**Model 3**	**Model 4**
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
25 * Post-Litigation Trend	0.067	0.103	0.103	0.120	0.042	0.073	0.071	0.100
	(0.030)	(0.024)	(0.025)	(0.016)	(0.030)	(0.024)	(0.027)	(0.020)

B. Long Sample Estimates

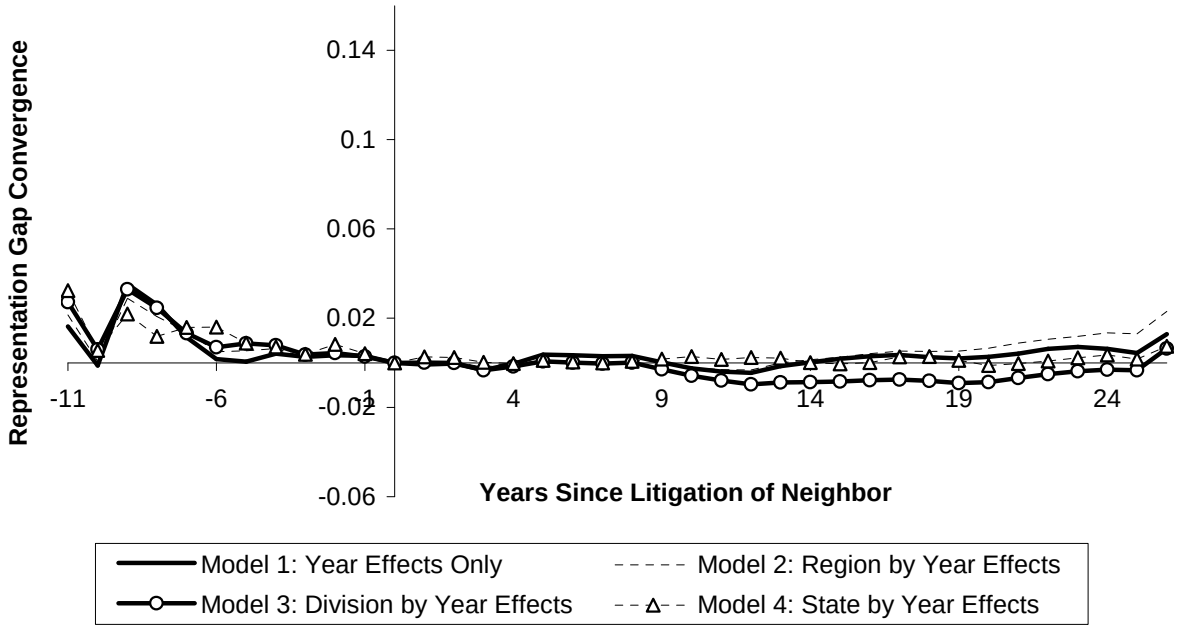
	All 120 Cities				43 Litigated Cities Only			
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 1**	**Model 2**	**Model 3**	**Model 4**
	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
25 * Post-Litigation Trend	0.047	0.096	0.101	0.161	0.042	0.101	0.102	0.151
	(0.045)	(0.042)	(0.047)	(0.025)	(0.061)	(0.053)	(0.055)	(0.034)
25 * Post-Pre Trend Difference	0.127	0.184	0.179	0.289	0.039	0.164	0.158	0.279
	(0.058)	(0.061)	(0.060)	(0.034)	(0.089)	(0.055)	(0.056)	(0.039)

	86 Cities Identifying Model 4				Reweighting the Unlitigated			
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 1**	**Model 2**	**Model 3**	**Model 4**
	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)
25 * Post-Litigation Trend	0.098	0.119	0.117	0.161	0.029	0.089	0.090	0.148
	(0.071)	(0.056)	(0.057)	(0.025)	(0.046)	(0.040)	(0.046)	(0.031)
25 * Post-Pre Trend Difference	0.176	0.203	0.192	0.289	0.138	0.190	0.177	0.297
	(0.107)	(0.071)	(0.069)	(0.034)	(0.068)	(0.066)	(0.063)	(0.041)

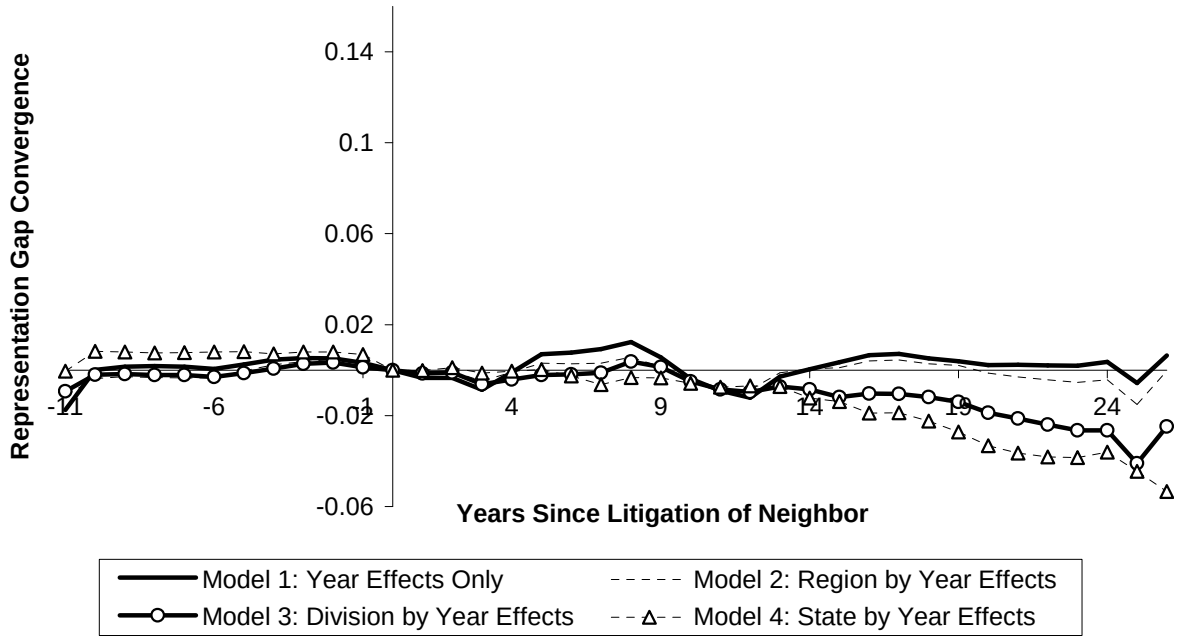
Notes: Panel A presents estimates of the annual gain (times 25) in representation gap in the years following litigation for four choices of specification, three subsets of the full sample, and two different weighting schemes. The four specifications correspond to those from Figure 5: year effects only, region-by-year effects, division-by-year effects, and state-by-year effects. Estimates in columns (1) to (4) are for all 314 cities in the full sample. Estimates in columns (5) to (8) are based on litigated cities only (see text for discussion). Estimates in columns (9) to (12) are based only on cities in states with multiple cities represented that are not all litigated at the same date--i.e. the set of cities corresponding to the implicit sample selection rule underlying Model 4. Estimates in columns (1) to (12) are weighted by 1970 city population. Estimates in columns (13) to (16) are weighted by a factor that approximately equalizes in distribution the 1969 representation gap for litigated and unlitigated cities (see text for details). In all columns, the estimated annual gain was obtained by imposing a linear restriction on the estimates of θ_j from equation (1.2) (see text). Panel B is organized identically, but is based on the 120 cities in the long sample. Panel B additionally reports the difference in post-litigation and pre-litigation annual gains in representation gap, because the long sample contains sufficient historical data to provide confidence in the pre-litigation estimates. Standard errors (parentheses) are clustered at the city level, and are asymptotically consistent as the number of cities approaches infinity (see text for formula). Standard errors are further inflated by a degrees of freedom correction due to imputation of the dependent variable.

Figure 6. A Direct Test of the Litigation Threat Effect Hypothesis

A. Unlitigated in Full Sample



B. Unlitigated in Long Sample



Notes: Figure presents event study estimates of the impact of litigation of a litigated city on the representation gap in unlitigated cities in the same state or sub-state area as the litigated city. To conduct the estimation, I assigned to each unlitigated city the first litigation date of a city in its state or sub-state area, following as nearly as possible the boundaries for the district courts.

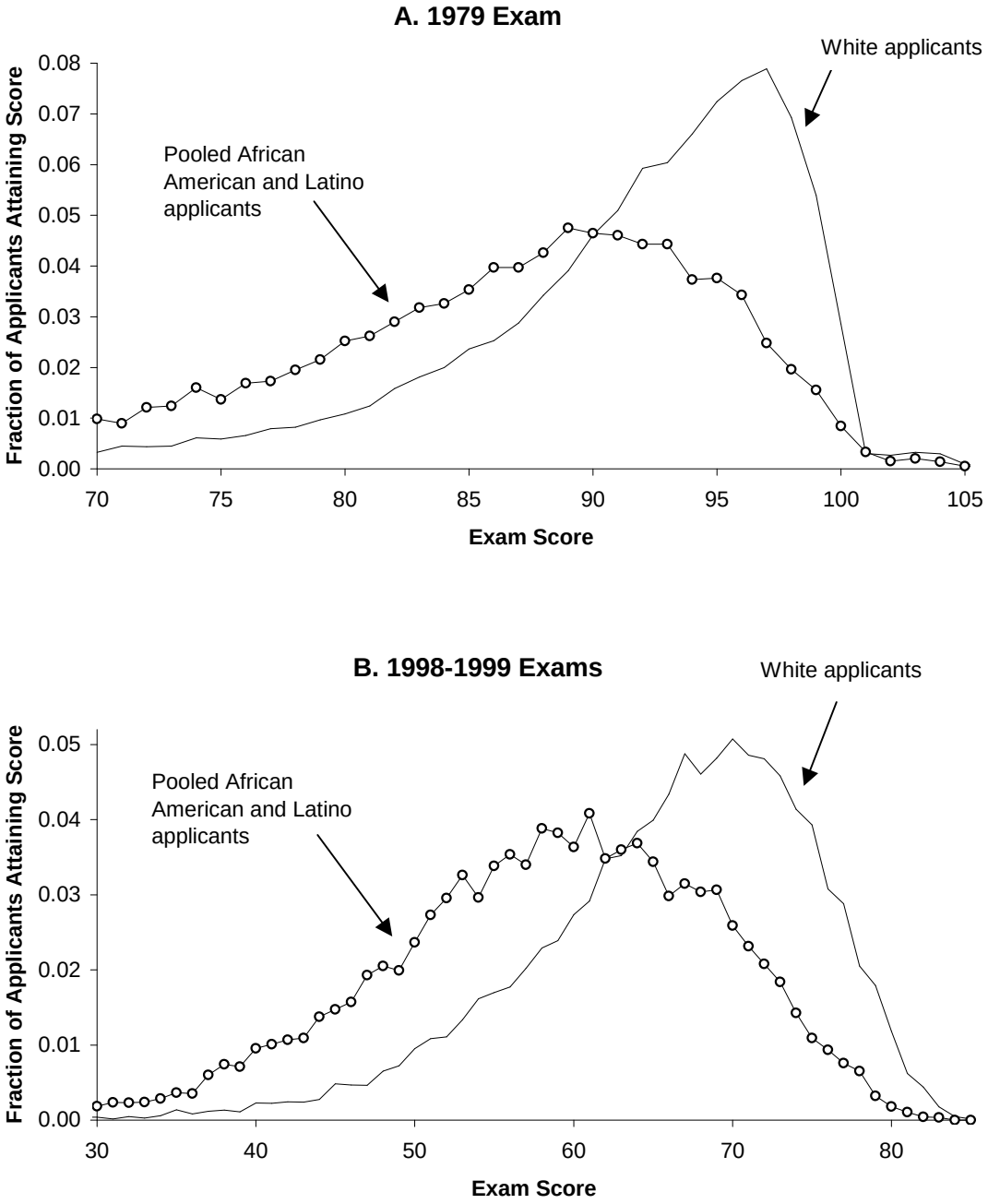
Table 4. New York City Police Department Entrance Examination Scores by Race, 1968-1999

	July 1968 Examination			May 1970 Examination		
Decile	Score Range	White Fraction	Black Fraction	Score Range	White Fraction	Black Fraction
1	96-100	12.9	2.3	94-100	12.7	2.9
2	90-96	12.9	2.6	92-94	12.1	4.0
3	86-90	12.8	3.5	89-92	11.8	5.5
4	82-86	12.0	6.8	87-89	12.2	4.2
5	78-82	10.2	13.3	84-87	10.8	8.0
6	73-78	10.6	9.1	80-84	10.5	9.3
7	69-73	7.1	14.8	76-80	9.8	10.4
8	63-69	7.1	15.9	70-76	8.6	13.8
9	56-63	8.2	11.2	61-70	7.1	17.9
10	29-56	6.2	20.6	0-61	4.5	24.2
N		2,774	725		4,511	1,002

	June 1979 Examination[1]			Pooled 1998-1999 Examinations[1]		
Decile	Score Range	White Fraction	Black Fraction[2]	Score Range	White Fraction	Black Fraction[2]
1	99-110	10.0	3.9	75-85	16.2	4.1
2	97-98	15.4	5.1	72-74	13.5	5.3
3	95-96	15.4	8.3	69-71	14.8	8.0
4	93-94	13.1	9.4	66-68	13.8	9.2
5	91-92	11.4	10.4	63-65	11.4	10.7
6	89-90	8.8	10.9	60-62	9.1	11.2
7	86-88	9.1	14.1	56-59	8.5	14.6
8	81-85	9.3	17.9	51-55	6.8	15.3
9	71-80	7.1	18.9	41-50	4.7	15.9
10	0-70	NA	NA	0-40	1.2	5.6
N		19,109	9,858		13,009	11,197

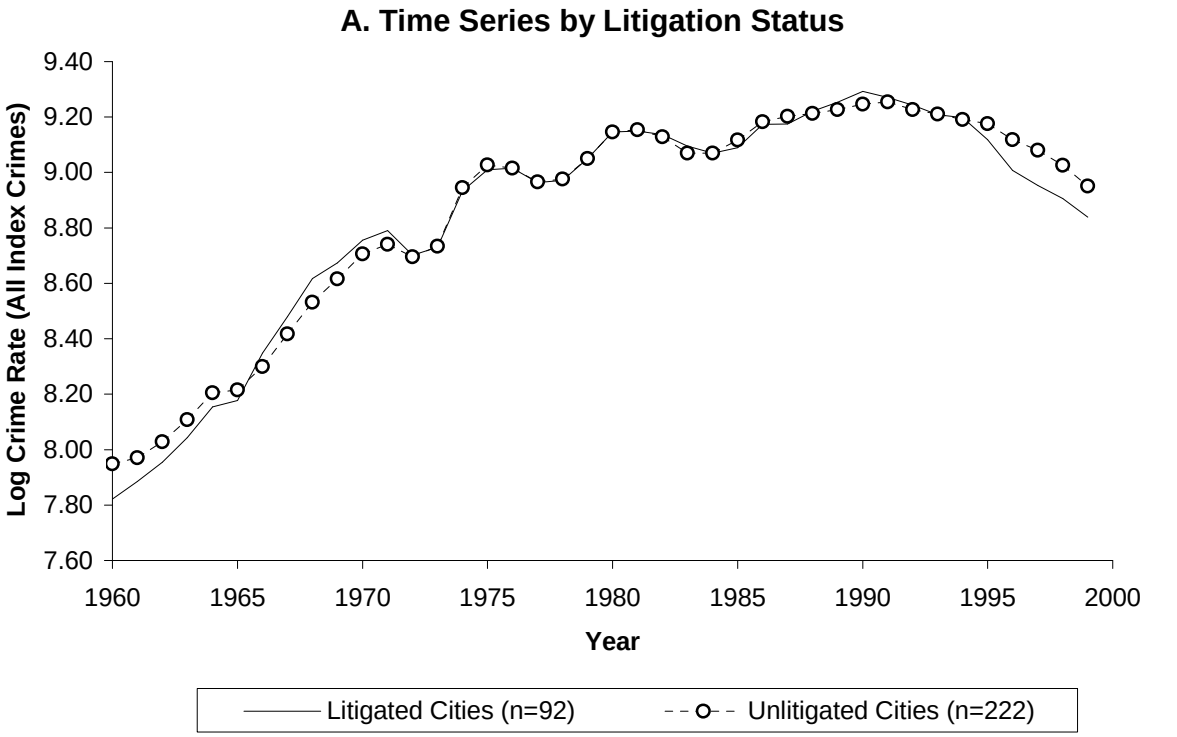
Notes: [1]Because of test score aggregation, score ranges are approximate deciles. [2]Black includes African Americans and Latinos. For the 1968 and 1970 examinations, the distribution of scores is available separately for African Americans and Latinos. The distributions for these two groups are quite similar.

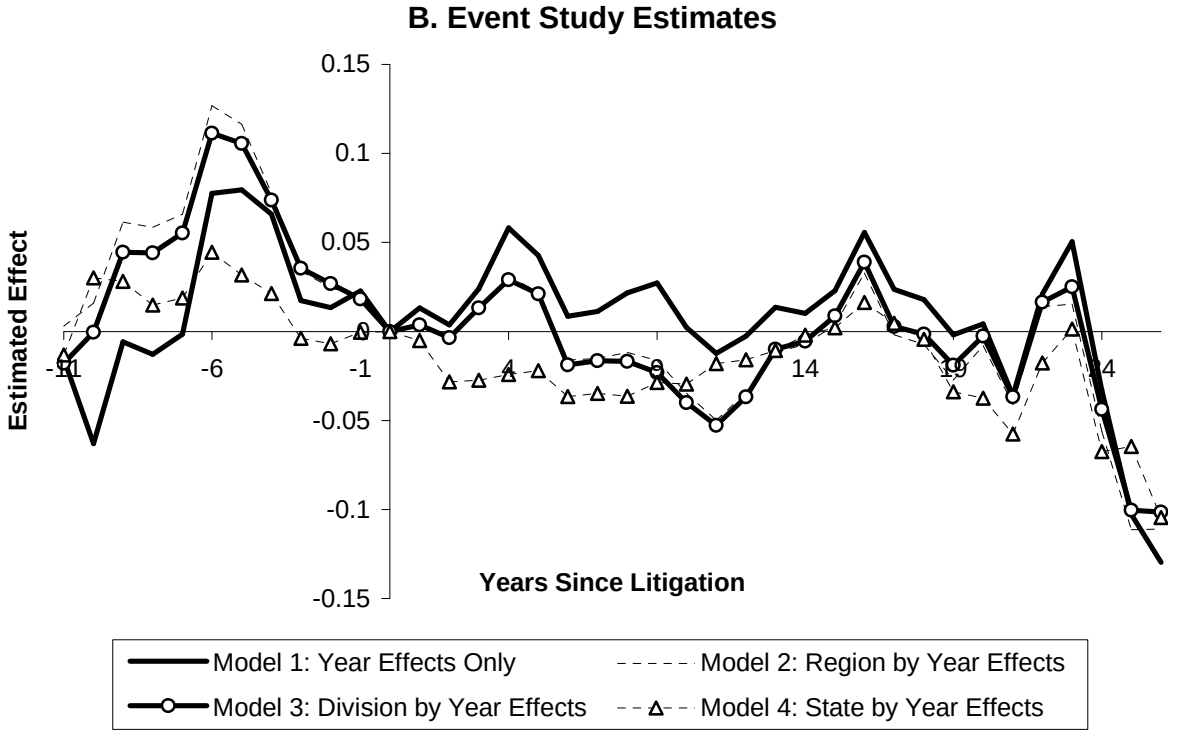
Figure 7. Density Estimates, NYCPD Entrance Examination Scores by Race



Notes: Figure gives kernel density estimates of entrance examination scores for applicants to the New York City Police Department in 1979 and in 1998-1999.

Figure 8. The Impact of Litigation on Log Crime Rates





Notes: For 314 cities in full sample, top panel of figure presents the time series of the log of all index crimes per 100,000 population by litigation status, 1960 to 1999. Also for the full sample, bottom panel of figure presents event study estimates of the impact of litigation on log crimes per 100,000 population.