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Preliminary & Incomplete

Racial Disparities in the Allocation of Wiretap Applications across Federal Judges

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January 2009
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Abstract

The degree to which discrimination causes the large racial disparities in the criminal justice system is difficult to measure. This paper studies a process where the influence of race on prosecutors' decisions can be directly identified: the process of obtaining judicial approval to conduct wiretap surveillance. The Department of Justice's strict internal review of wiretap applications implies that federal judges approve all wiretap applications they receive. In this setting, a judge's race should not influence the number of wiretap application she receives, unless prosecutors are biased. The paper tests this prediction using all wiretaps in federal criminal investigations during the years 1997-2007. The results show that African-American judges receive substantially fewer wiretap applications than other judges, even after controlling for other judicial and district characteristics. Consistent with the absence of an incentive to "shop" for favorable judges, other judicial characteristics such as ideology and prior professional experience do not influence the number of wiretap applications received. Nor does statistical discrimination explain the racial gap in wiretap applications; the wiretaps approved by African-American and other judges do not differ in the rates at which they produce incriminating evidence, arrests, or motions to suppress evidence. The results suggest that racial attitudes may influence even sophisticated and closely monitored actors in the criminal justice system, such as federal prosecutors.

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* Comments welcome at tmiles@law.uchicago.edu. The author thanks David Abrams, Jens Ludwig, Richard McAdams, Max Schanzenbach, Justin Wolfers, and several current and former federal prosecutors (who wish to remain anonymous) for helpful conversations and participants at the 2008 Midwest Law & Economics Association meeting and the University of Pennsylvania Law & Economics Seminar for helpful comments. The author also thanks Arthur Baptist, Youn Jin (Ann) Choi and Bryan T. Hart for outstanding research assistance.

1. Introduction

The criminal justice system is marked by sharp racial disparities, and an important and highly contested question is the degree to which discriminatory enforcement causes these disparities. Measurement of discrimination is difficult for three reasons. First, many observed outcomes flow from the choices of multiple actors, such as arrest rates which result from the conduct of both police and offenders. Second, even when the econometrician can isolate the behavior of a single actor, racial disparities may arise from omitted variables rather than discrimination. Third, the motivation of the actor is difficult to determine. Economics defines two types of discrimination. A decision-maker engages in *statistical discrimination* when he uses race as a criterion because it predicts future outcomes even if he is not racially prejudiced (Arrow 1973). A decision-maker has a taste for discrimination or engages in *utility-based discrimination* when his utility varies with the race of the individuals with whom he interacts (Becker 1957). Both types of discriminators may treat minority group members less favorably.

Several researchers, most prominently Ayres and Waldfogel (1994) and Knowles et al. (2001), have proposed a test that attempts to overcome these difficulties and that distinguishes statistical discrimination and utility-based discrimination. They ask whether the decision-maker in equilibrium tolerates a lower rate of success in the minority group's outcomes. A lower rate of success is the decision-maker's cost of indulging his taste in discrimination and is the observable difference between statistical and utility-based discriminators. For example, in Knowles et al.'s (2001) study, a higher rate of police searching minority drivers but equal rates at which searches of other and minority drivers yield contraband is consistent with statistical discrimination. A higher rate of searching

minority drivers but a lower rate of finding contraband among minority drivers is consistent with utility-based discrimination.[1]

This paper uses an analogous prediction to test whether another set of actors in the criminal justice system, federal prosecutors, engage in racial discrimination. Oral or electronic surveillance, or "wiretapping," is a prominent investigative technique that is closely regulated due to its highly intrusive nature. The unique regulatory structure governing wiretapping implies that when prosecutors seek approval from federal judges for wiretaps, there is no meaningful chance that the judge will deny the application. This environment provides a test for whether race enters the preferences of prosecutors. The key prediction is that if prosecutors are not utility-based discriminators, the frequency with which they ask a judge to approve a wiretap and the rate at which wiretaps yields incriminating evidence and arrests should not correlate with a judge's race.

As with searches for physical evidence, investigators must obtain a warrant from a judge before wiretapping conversations. Prosecutors in most jurisdictions have discretion to choose the judge within the district who will review the application for a wiretap warrant. When a prosecutor seeks a judge's approval for a wiretap, the prosecutor appears directly before the judge, typically in the judge's chambers. In addition to delivering the warrant application, a document which commonly exceeds forty or fifty pages, the prosecutor often gives an oral summary of it, highlighting the nature of the investigation

[1] In a similar vein, Tomic and Hakes (2008) find that for some offense categories, the rate of felony charge dismissal is higher for black arrestees. This pattern exists for violent, drug, and weapons offenses but not property crimes, and the authors interpret these results as indicating the presence of racial bias in offenses requiring snap judgments by police. Heaton and Loeffler (2008) examine differences in white and black arrest rates for crimes committed by racially-mixed groups of offenders. They find that blacks experience an arrest rate three percentage points higher than their white confederates. This pattern is consistent with bias, but it is too small to explain the large racial disparity in arrest rates in the general population.

and the supporting evidence. The prosecutor answers any questions the judge has on the application.

Unlike searches for physical evidence, federal investigators must also secure authorization from the Department of Justice (DOJ) before seeking judicial approval for a wiretap. The DOJ's scrutiny of wiretap applications is more rigorous than judicial review. A consequence of the DOJ's higher threshold is that a federal judge should approve any wiretap applications that reach her. In fact, federal judges have approved nearly all wiretap warrant applications presented to them in the past decade.

In this setting, the allocation of wiretap applications across judges within a district is a test for whether prosecutors engage in utility-based discrimination. If prosecutors are not utility-based discriminators, a judge's identity should not influence the number of applications she receives. In contrast, if prosecutors are utility-based discriminators, the cost of interacting with minority judges will prompt prosecutors to seek approval for wiretaps from minority judges less often. Minority judges' receipt of fewer wiretap applications is consistent with utility-based discrimination by prosecutors.

A competing hypothesis is that any racial gap in wiretap applications is due to statistical discrimination. In this context, prosecutors may engage in statistical discrimination if judges varied in their likelihood of approving a wiretap application. Prosecutors would bring more wiretap applications to judges favorably disposed toward law enforcement and fewer to judges less favorably inclined toward it. This hypothesis generates two predictions. The first is a falsification test of the judge-shopping hypothesis. A large literature establishes that race correlates with attitudes toward law enforcement. Other variables such as a judge's ideology scores, her past sentencing practices, and past

legal practice as a defense lawyer or prosecutor are also important correlates of these attitudes. If prosecutors shop for favorable judges, these variables – in addition to race – should vary with the receipt of wiretap applications. But if prosecutors respond to race alone and not to favorable predispositions, these other variables should not be significantly related to the number of wiretap applications a judge receives.

Second, prosecutors shopping for favorably disposed judges should bring stronger wiretap applications to disfavored judges and weaker ones to favored judges. As a result, wiretaps approved by favorably disposed judges should produce incriminating evidence, arrests, and convictions at lower rates than those approved by more discerning judges. Their wiretaps might also result in motions to suppress the evidence more often. If African-American judges are on average more critical reviewers of wiretap applications, the racial gap in wiretap applications should be matched with racial gaps in wiretap outcomes. But if prosecutors do not shop for favorably disposed judges, there should be no racial gap in wiretap outcomes.[2]

The paper tests these predictions using a data set of all wiretaps used in federal criminal investigations during the years 1997-2007. The results show that African-American judges receive substantially fewer wiretap applications than other judges, even after controlling for differences in a judge's ideology, past sentencing practices, professional experience, and other factors. The racial gap is robust to a variety of controls for district-level differences, including the interactions of district and year fixed effects.

[2] This prediction differs from the familiar account of statistical discrimination in contexts such as racial profiling. In that setting, statistical discrimination is consistent with disparities in inputs (the rates at which police search motorists of different racial groups) and equality in outcomes (such as the rates at which searches yield contraband). Equality in outcomes results from adjustments in the behavior of the groups subject to treatment. For example, the racial group subject to more intensive searches reduces the rate at which it carries contraband (Knowles et al. 2001).

The gap persists across the type of phone tapped, the offense investigated, and even the day of the week and season of the year in which surveillance occurs.

The estimates also suggest that the racial gap in wiretap applications does not result from statistical discrimination. Other judicial characteristics that correlate with attitudes toward the criminal justice system and law enforcement are not meaningfully related to the number of wiretap applications a judge receives. In addition, the outcomes of wiretaps do not vary meaningfully with the authorizing judge's race. The wiretaps approved by African-American and other judges do not differ in the duration of their surveillance or in the rates at which they produce incriminating evidence, arrests, or motions to suppress. Instead, the wiretaps approved by African-American judges lead to marginally fewer convictions than those of other judges. Overall, the results suggest that racial attitudes may influence the decisions of even sophisticated and closely monitored actors in the criminal justice system, such as federal prosecutors.

The plan of the paper follows. Section 2 describes the procedures federal prosecutors must follow to obtain permission to conduct wiretap surveillance, and it predicts how these procedures influence a prosecutor's choice of the reviewing judge. Section 3 describes the data and its sources. Section 4 presents the empirical evidence, and Section 5 offers further interpretations of the estimates and policy implications.

2. Legal and Institutional Background

A. The Wiretap Application Process

Title III of the Omnibus Crime Control and Safe Streets Act of 1968 (Title III) codified at 18 U.S.C. §§ 2510-2522, governs law enforcement's use of wire, oral, and

electronic surveillance. The statute requires law enforcement officials to apply for a court order authorizing the interception of these communications, absent an express exemption such as consent of a party to the communication. Prosecutors wishing to conduct wire and electronic surveillance must therefore obtain a warrant from a judge of "competent jurisdiction," meaning a federal district or appellate judge or a state judge.[3] As a practical matter, federal prosecutors always seek approval for wiretap warrants from federal district judges rather than state judges.[4] A warrant to conduct wire, oral, or electronic surveillance is analogous to a warrant to search a physical place in that both types of warrant applications must be supported by probable cause. For a wiretap warrant, the government must establish probable cause to believe (1) that persons have committed, are committing, or are about to commit one of the crimes enumerated in Title III (§ 2516); (2) that particular communications concerning these offenses will be obtained through the interceptions; and (3) that the facility from which the communications are to be intercepted has been, is being, or is about to be used in connection with one of the offenses (18 U.S.C. § 2518(3)). In addition, the application must include a statement of whether other investigative techniques have been tried and failed, or why they appear unlikely to succeed or are too dangerous. To satisfy this latter requirement, the so-called "necessity requirement," prosecutors need not show that every conceivable investigative method has been tried and failed. Rather, they typically produce an expert opinion that gives specific

[3] Circuit courts have interpreted the phrase "competent jurisdiction" to mean that federal district courts cannot delegate the authority to review wiretap applications to magistrate judges. *See, e.g.*, *In re U.S.*, 10 F.3d 931, 935-96 (2d Cir. 1993).

[4] The divisions between federal and state investigations is a long-standing practice. See Kaplan (1965, p. 192) ("In the absence of other considerations, the assistant [U.S. Attorney] prosecuted where most of the investigatory work was done by federal agents, and decided in favor of state prosecution whenever state officials had 'built' the case").

reasons why alternative techniques are likely to fail or be excessively dangerous (O'Meara 1993).

Title III further provides that before a federal prosecutor applies for judicial order authorizing wire or oral surveillance, the Attorney General or one of a limited number of specified, high-ranking deputies must authorize the application (18 U.S.C. § 2516(1)). The DOJ responded to this requirement by creating a complex bureaucratic process to review wiretap warrants before high-ranking DOJ officials evaluate them. The purpose of this review and the goal of the attorneys who conduct it are to achieve meticulous compliance with the statute in order to prevent any future restrictions on the use of wiretaps.[5]

This process consists of four main steps (Staff of ESU 1997). First, the line prosecutor seeking to conduct wiretap surveillance must convince the chief prosecutor within a judicial district, the United States Attorney (USA), to submit a warrant application to the Electronic Surveillance Unit (ESU) within the DOJ's Office of Enforcement Operations (DOJ 2008). The USA and the supervising prosecutors within her office review the line prosecutor's request, and if they conclude that the application is not sufficiently strong or the investigation not sufficiently important, they may decline it.

Second, when applications reach the ESU, its attorneys "painstakingly and thoroughly" review the application (Bachner 1997). They review the form and substance of the application to ensure compliance with Title III, and "a good percentage of the time" they find problems with the application. The ESU attorney contacts the line prosecutor to discuss whether and how the problem may be fixed in order that the application can

[5] Frederick D. Hess, long-time Director of the Office of Enforcement Operations, summarized this perspective: "The wiretap is a great investigative tool and it can make your case for you. Cherish it, preserve it, and protect it. Don't ask us to push it beyond where it is supposed to go . . . This investigative tool is too important to play games with" (Hess 1997).

proceed (Hess 1997). The standard the ESU applies in reviewing whether probable cause is met exceeds constitutional and statutory requirements. For example, to ensure a high probability that criminal activity is still ongoing, the ESU applies a "21-day rule" under which the prosecutor must show that the targeted phone was used in connection with a crime within 21 days of the DOJ's authorization (Staff of ESU 1997). In addition, the OEO has a reputation of regularly denying requests at this stage.[6]

In addition, the ESU contacts the relevant office within the DOJ's Criminal Division, such as the Narcotics and Dangerous Drugs Section or the Organized Crime and Drug Enforcement Task Force in the case of a narcotics investigation, for its judgment that the investigation is sufficiently important to merit use of a wiretap. The ESU also refers the application to the investigating agency, which most often is the Federal Bureau of Investigation or the Drug Enforcement Agency, to determine that the wiretap will not exceed the agency's budget and responsibilities.

Third, after the ESU has completed its review, its Director drafts a memorandum to one of the Attorney General's designated deputies with a recommendation to approve or reject the application (DOJ 2008). These officials "almost invariably" approve the applications at this stage (Hess 1997).[7] Finally, if the deputy approves the application, the Director of the ESU sends a letter together with the Attorney General's authorization to the USA. Only after this process is completed may the line prosecutor seek judicial approval for the wiretap.

[6] For example, "the people in OEO do a fantastic job. They are the people who review, among other things, applications for wiretaps and bugs, and they scrub them. And they're very smart. And what we like about OEO is they know how to say yes and how to say no. And if they don't think [a wiretap application] meets the standards, they'll tell you that and you won't get your wiretap or bug" (Fitzgerald 2008).

[7] According to Hess, these officials "have problems with the requests once in a while, but major problems are rare after the extensive review process in OEO" (Hess 1997).

In most judicial districts, the line prosecutor has discretion to choose the judge within the district who will review the application. Title III, the statute governing the use of wiretaps in criminal investigations, does not specify how the reviewing judge should be chosen. Some district courts have adopted as part of their local rules the requirement that the chief judge in the district review all wiretap applications. Twenty-five of the 90 districts have such a "chief judge rule."[8] But these districts tend to be smaller, and judges sitting in such districts account for 19.5% of the observations in the judge-by-year balanced panel studied here.

Even after the wiretap has been authorized by the DOJ and approved by a judge, the process of conducting the electronic or oral surveillance presents substantial challenges. Prosecutors and investigating agents cannot indiscriminately record the conversations of targets and later sift through them to locate any conversations involving criminal activity. Rather, Title III requires that investigators "minimize" the intrusion into privacy by listening to or recording only those conversations relating to the targeted criminal activity. The minimization requirement means that when listening to targets' conversations, investigators must immediately assess whether the conversation pertains to criminal activity in order to decide whether monitoring should continue. To assist in making these determinations, the prosecutor leading the surveillance must be available on a 24-hour basis for the duration of the wiretap (Bachner 1997). Surveillance itself often involves a team of technicians, monitoring agents, sometimes translators, and often agents in the field conducting simultaneous visual surveillance. The line prosecutor's task in

[8] These districts are Arkansas-Eastern, Arkansas-Western, California-Southern, Georgia-Southern, Idaho, Illinois-Northern, Iowa-Northern, Iowa-Southern, Kentucky-Eastern, Kentucky-Western, Louisiana-Middle, Michigan-Western, Minnesota, Mississippi-Northern, Mississippi-Southern, Montana, Nebraska, North Carolina-Western, Oklahoma-Northern, Pennsylvania-Middle, Pennsylvania-Western, Rhode Island, South Dakota, West Virginia-Southern, and Wyoming.

coordinating these teams can be a "logistical nightmare" (Styron 1997). The prosecutor must file periodic reports with the court, typically every 10 days, describing the continuing need for surveillance and any progress toward the investigation's goals (18 U.S.C. § 2518(6)). If the investigation will extend beyond the thirty days that Title III permits a court order to last, the prosecutor must apply for an extension of the wiretap warrant. To obtain it, the prosecutor must again proceed through the authorization process at the DOJ and receive approval from the court. When an extension is likely needed, the line prosecutor should begin this process "almost immediately" to avoid interruptions in the surveillance (Bachner 1997), and thus, the prosecutor often pursues the extension process while also managing the surveillance.

Title III provides that surveillance should terminate when law enforcers obtain the investigative objective – evidence of the targeted criminal offense – or if the surveillance does not meet this objective, it must terminate upon expiration of the court order (18 U.S.C. § 1518(5)). When surveillance ends, the product of the intercept must immediately be "sealed" (18 U.S.C. § 2518(8)(a)). Sealing refers to the court's taking physical custody of the tapes of the intercepted conversations, which assures the authenticity of the tapes from the time that the court takes custody. Violation of the requirements of Title III results in suppression or exclusion of the evidence from the criminal prosecution.[9]

B. The Absence of an Incentive to "Judge Shop"

The stringency of the DOJ's authorization process implies that judges are certain to approve wiretap applications that reach them. During the eleven years studied in this paper, judges rejected only 4 of 6,684 wiretap applications, a rejection rate of less than one

[9] 18 U.S.C. §§ 1515, 2518(10)(a); *Katz v. United States*, 389 U.S. 347 (1967).

tenth of one percentage point (U.S. Administrative Office various years).[10] The low rate of successful challenges is likely due to several factors related to the determination of probable cause, which is the central issue in obtaining judicial approval for a wiretap. First is the impact of the DOJ's authorization process on which wiretap applications reach judges. It may screen out applications in which probable cause is marginal. Or, the DOJ may withhold approval of marginal applications until line prosecutors and enforcement agents bolster them with additional evidence.

The USAs and their line prosecutors may forgo marginal cases. Federal prosecutors typically have more cases than their resources permit them to pursue. The extensive reach of the federal criminal law (Beale 2005) and the severe penalties of the federal system, particularly for drug offenses, imply that the opportunity costs for cases with limited chances of success are large. The labor-intensity of electronic surveillance implies that the direct costs of a wiretap investigation are substantial. The cost of the average wiretap surveillance exceeded $67,000 in 2006 (Administrative Office 2006).

In addition, the pursuit of marginal cases does not offer career advantages for prosecutors. In Landes' (1971) now-classic model of prosecutorial behavior, prosecutors maximize the expected sentences net of the prosecutor's cost of obtaining convictions. Glaeser et al. (2000) specified a model in which prosecutors' objective functions included both crime control and career-enhancing human capital. A line prosecutor's career prospects are not heightened by acquiring a reputation within the DOJ for pursuing dubious cases. In describing the high standards U.S. Attorneys' Offices apply to wiretap applications, Richman (2003) states that "prosecutors have an interest in building a

[10] The actual rejection rate for federal wiretap applications may be lower. The Administrative Office does not reveal whether these four rejections occurred in wiretap applications before state or federal courts.

professional reputation for legal acuity that will often be best served by blocking an action." Boylan (2002) found that the career outcome most favored by USAs was a federal judgeship, and that USAs who later joined the federal bench had led U.S. Attorneys' Offices that secured the longer average sentences than did USAs who later joined law firms. Conviction rates varied little across USAs' subsequent career outcomes. This evidence suggests that the portfolio cases available to federal prosecutors may not have a sharp risk-reward tradeoff, and may make them reluctant to pursue wiretaps with low probabilities of success.

Another factor influencing the incentive to judge-shop is the nature of the judicial task. A growing body of research shows that a judge's personal characteristics correlate closely with the legal conclusions she reaches (Sunstein et al. 2006; Hettinger et al. 2006; Rowland and Carp 1996). But this tendency may not exist in determining probable cause. In experiments with state court judges, Wistrich et al. (2005) found substantial uniformity in judges' evaluations of probable cause. Their conclusions about the existence of probable cause in the experiment were not influenced by whether the search yielded incriminating evidence or by the judge's gender and prior professional experience. Wistrich et al. attribute this uniformity to the large body of case law that courts have developed clarifying the meaning of probable cause. In cases involving wiretap evidence, defense counsel rarely challenge the judge's probable cause determination. Instead, defense counsel most commonly challenge the duration of the wiretap and the satisfaction of the necessity, minimization, and sealing requirements (O'Meara 1993 (noting all four), Annis 1997 (noting minimization and necessity)). Interestingly, three of these four issues pertain to the operation of the wiretap rather than matters that judges evaluate ex ante.

3. Data

The source of the data on wiretaps is the Administrative Office (AO) of the United States Courts (various years). The federal wiretap statute, Title III, requires the AO report annually to Congress the number and nature of wiretap warrant applications filed. It further requires judges and prosecutors to submit information to the AO on wiretap warrant applications (18 U.S.C. § 2519). Judges must report the date of the application, name of the investigating official, the offense under investigation, the type of intercept, the location of the device tapped, and the length of time for which the wiretap was authorized. Prosecutors must submit the cost of the wiretap, the length of time it was in operation, the total number of intercepts, the number of incriminating intercepts, and the number of any resulting arrests and convictions. To satisfy its Congressional reporting requirement, the AO publishes an annual *Wiretap Report*. The *Report* identifies the authorizing judge by her last name. The *Report*'s Appendix Table 1 summaries judges' and prosecutors' submissions on wiretaps authorized for criminal investigations that have concluded operation by December 31 of the year. Information on wiretaps whose operation continued into the next calendar year is included in the following year's report. Its Appendix Table 2 contains updates on subsequent developments regarding the wiretaps, such as later arrests and convictions. Title III does not authorize the AO to gather information on the identities of the targets of the wiretaps or on the use of wiretaps when a party of the communication consents to the tapping.

The Federal Judicial Center's database of federal judges provides biographical and demographic information on all current and past Article III judges. From this source, the author identified all federal judges who have served in district court since 1997. Of these

1,280 judges, 562 (or 43.9%) had senior status at some point during the observation period. Another 586 (or 45.8%) did not serve in each of the eleven years of the observation period; 338 joined the bench after 1997; 248 left it (through either death or retirement) before 2007; and 6 both arrived after 1997 and left before 2007. With such turnover in the courts' make-up, concern is that the estimates, especially any observed impact of race, may be due to changes in the composition of judges in the data. For that reason, the main analysis of the paper relies on a balanced panel of judge-by-year observations that includes only judges who served on the bench in at least part of each of the eleven years of this time frame. It includes judges who were confirmed to the bench before or during 1997 and who either remained on the bench or left it in 2007. The main results were re-estimated with the data restricted to the set of 1,280 judges who served on the bench at any point during the eleven-year period, and as shown below, the estimates were similar.

Table 1 presents mean differences between African-American and other judges in the data. Of the 694 judges, 64 (or 9.3%) are African-American, and in the unbalanced panel of all judge-by-year observations, African-American judges account for 8.9% of the observations. Table 1 shows that African-American and other judges differ across several dimensions, and one of the most pronounced differences is in political ideology. Consistent with the well-established pattern that African-Americans disproportionately align themselves with the Democratic party (Dawson 1994, Tate 1993), African-American judges in the data were more likely than other judges to be Democratic appointees. Democratic presidents appointed over 75% of the African-American judges in the data and only 41% of other judges.

Political scientists continue to dispute which measure of judicial ideology is best (Pinello 1999; Epstein and King 2002; Sisk and Heise 2005), but the most frequently used measure of judicial ideology is the common space score of Poole and Rosenthal (1997). Giles et al. (2001) developed a method of adjusting these scores for the practice of senatorial courtesy, according to which a senator belonging to the same political party of the president has some influence on the selection of nominees to the district courts within her state.[11] By this measure, African-American judges are significantly more liberal on average than other judges. But the gap shrinks considerably when conditioning on the party of the appointing president; from a difference of .298 to about .045 points on this -1 to +1 scale. Most of the variation in the ideology scores comes from differences in the party of the appointing president rather than senatorial courtesy.

These comparisons reflect variation both across and within districts, and prosecutors in choosing which judge to review a wiretap application are constrained to choose a judge within their judicial districts. To get a sense of the ideological distribution within districts, rankings of judges by ideological scores within each district were calculated. Table 1 shows that ideological differences between the two racial groups persist within districts. African-American judges ranked more liberal on average within their districts than other judges, and they were more often the most liberal judge within their districts.

A limitation of the ideology scores and partisan affiliations is that they measure political attitudes generally. A judge's view of the criminal justice system may be more

[11] According to the Giles et al. (2001) procedure, when neither senator in the relevant state belonged to the president's party, the judge received the Poole-Rosenthal score of the appointing president. When at least one senator belonged to the president's party, the judge received that senator's score, and when both senators belonged to the president's party, the judge received the average of the two senators' scores. Judges in the District of Columbia received the appointing president's ideology score.

relevant to her evaluation of a wiretap application. To obtain more precise measures of judicial attitudes toward the criminal justice system, two measures of each judge's sentencing patterns were collected from the "Federal Judges" data base of Transactional Records Access Clearing House (TRACFed) at Syracuse University: the percentage of defendants that the judge has sentenced to incarceration, and the average length of these sentences.[12] The roughly equitable caseload between judges and the random assignment of cases to them implies that the average severity of criminal cases a judge hears over a long period of time, such as the eleven years studied here, should be roughly the same across judges. Any differences in these outcomes across judges should therefore reflect their attitudes toward the criminal justice system. Neither of these variables differs across the two racial groups. The average sentences of African-American judges are about two and a half months shorter than those of other judges. But this difference is not statistically significant, and it is small relative to the average sentence length of two and a half years. The incarceration rates differ by less than two percentage points. The absence of a large gap in these variables is surprising given the sizeable differences seen in ideology scores and in the party of the appointing president. The similarity in sentencing patterns may reflect the fact that the federal sentencing guidelines, which were mandatory during the study period, permitted judges little discretion in sentencing (Stith and Cabranes 1998).

Judges differed in other demographic and experiential characteristics. African-American judges were on average six years younger, and their average service on the federal bench was 3.5 years shorter than their colleagues. Consistent with their shorter tenure on the federal bench, African-American judges spent less of their time during the

[12] Incarceration rather than conviction rates were used because conviction rates in federal courts exhibit little variation. For felony defendants, they typically exceed 90% (U.S. Department of Justice, 2005, Table A.9).

observation period on senior status, a form of semi-retirement in which federal judges may reduce their caseloads.[13] Their shorter average tenure is also reflected as the lower incidence of their service as chief judges. The chief judge handles administrative tasks within the court, and the chief is determined by seniority.[14]

Except for the default category of private practice, work as a prosecutor was the most common prior professional experience of the judges. Almost half the judges previously served as either state or federal prosecutors. This experience was even more common among African-American judges as nearly 60% of them had served as prosecutors. In contrast, experience as either a public or federal criminal defender was rare. Only 4% of judges overall had performed this type of work. But the incidence of it was higher among African-American judges; nearly 11% of them had worked as these types of criminal defense attorneys.

Experience as a judge on state court before nomination to federal court was also common. Slightly less than 40% of all judges in the data had served on a state bench, and the rate was again higher among African-American judges. Over 50% of them had been state court judges. Other types of governmental experience were less common and had more modest differences between the racial groups. About 19% of judges previously worked in the legislative or executive branches of state government, and with respect to the federal government, about 16% previously worked in the legislative branch or in non-prosecutorial positions with the executive branches.

[13] Judges are eligible for senior status according to the so-called "rule of 80" as set by statute (18 U.S.C. § 371(c)). A judge may retire at her current salary at age 65 after performing 15 years of service as an active judge. The statute defines a sliding scale of eligibility for older judges with fewer years of service.
[14] The chief judge is the judge in active service who is senior to the judges who are (1) 64 years of age or under; (2) have served one or more years as a judge; and (3) have not previously served as chief judge.

The overwhelming majority of federal district judges were not educated at elite schools. Fewer than 20% of the judges attended an elite law school,[15] and slightly more than 10% attended Ivy League colleges. There is no meaningful difference between the racial groups in the type of undergraduate institution attended. But African-American judges attended elite law schools at slightly higher rates than others. Service as a law clerk to a judge is commonly considered an important form of legal training, and the selection process for clerkships is highly competitive (Avery et al. 2001, 2007). About 21% of all the judges in the data previously served as law clerks, but this figure was more than 9 percentage points lower for African-American judges.

Finally, African-American and other judges differed in the types of districts in which they served. African-American judges served on average in larger judicial districts. Their courts had an average of 19 judges while the average court membership for other judges was nearly 16 judges. Although not reported in Table 1 in order to conserve space, the geographical distribution of judges varied by race. African-American judges were more likely than other judges to serve in judicial districts in the northeast (31.3% versus 26.2%) and less likely to serve in districts in the south (28.1% versus 34.3%). But African-American and white judges served at equal rates in districts in the midwest and west. African-American judges were slightly less likely to serve in a district with a local rule requiring the chief judge to review all wiretap applications than were other judges.

[15] The definition of elite is of course unavoidably arbitrary. Here, it is narrowly defined as Harvard, Yale, Stanford, Columbia, and Chicago.

4. Estimates

A. Baseline Estimates.

The frequency with which prosecutors ask a judge to approve a wiretap warrant is modeled as a function of the characteristics of the judge and the judicial district. The ordinary least squares regression takes the form

$$Y_{ijt} = Z_{ij}\delta + X_{ijt}\beta + \alpha_i + \alpha_t + \alpha_{it}t + \alpha_n + \epsilon_{ijt}, \quad (1)$$

where Y_{ijt} is the number of wiretap applications judge j in district i approves in year t as fraction of the number of days in the year in which the judge served on the bench (multiplied by 100). A judge's actual days of work are not observed. But, judges who were confirmed during 1997 or who left the court in 2007 have shorter "exposure times" or availability to receive wiretap applications during those years. The denominator corrects for these partial years of a judge's service; it takes a value equal to the number of days the judge was a member of the court during those years.

The term Z_{ij} contains time-invariant judge and district characteristics, and X_{ijt} contains the time-varying judge and district characteristics. The variable α_i is a fixed effect for district court i, and the data include 90 district courts. A federal statute, 28 U.S.C. §§ 81-144, defines 89 district courts in the fifty states plus the District of Columbia.[16] The number of judges within each district court varies across districts and is set by statute (28 U.S.C. § 133). The term α_t is a fixed effect for year t, and α_{it} is an interaction of district and year. An extension of this specification interacts district fixed effects and year fixed effects. The opportunity to shop for favorable judges may rise with the number of judges on a court. Although district and year fixed effects absorb most of this variation, the

[16] The paper's analysis excludes the district courts in Puerto Rico, the Virgin Islands, Guam, and the Northern Mariana Islands.

baseline equation also includes a fixed effect for the number of judges sitting on the district court in a given year, α_n, rather than a continuous variable for the number of judges within a district. Standard errors are clustered at the judge level.

Before proceeding to the regression results, Table 2 presents mean differences in wiretaps by the race of the authorizing judge. The columns labeled (1) present the total number of wiretap applications received, and those labeled (2) present the applications normalized by the number of days the judge was a member of the court in each year. Both sets of columns show a similar pattern. Overall, the difference between African-American and other judges in the number of wiretap applications received is modest. But this is due to the fact that the typical senior status judge receives far fewer wiretap applications, and the average African-American judge, as a younger and more recent nominee to the bench, is less likely to be senior status. When the means are conditioned on whether a judge is active or senior status, larger and statistically significant differences emerge. The average active-service African-American judge receives .68 or about two-thirds of wiretap application per year. Other judges receive on average .91 of an application per year or nearly one application per year, a difference of 26%.

Table 3 presents the first set of regression results. The estimates in columns (1) and (2) show that the patterns seen in the summary statistics are robust to the inclusion of district and year fixed effects and district-year interactions. District fixed effects are potentially quite important given the wide variation in the frequency of wiretap applications across districts (Minzner and Anderson 2005). Even in the presence of the fixed effects, the same pattern appears: when the equation does not include the senior status variable, the racial gap in wiretap applications is modest and statistically

insignificant. But once the regression accounts for the senior status of judges, the absolute magnitude of the race effect is more than twice as large and statistically significant. The regressions in columns (3) through (8) include controls for other judicial characteristics and again add progressively more fixed effects. They show that the estimate on race is remarkably stable, and it is nearly identical to the simple difference seen in the summary statistics.

In all specifications, the coefficient on senior status itself is statistically significant and implies a substantial reduction in the number of wiretap applications a judge receives. The size of this effect likely reflects the judge's availability to receive wiretap applications. Senior status enables a judge to take a reduced workload, and a senior judge may simply spend fewer days in the office and not be present when wiretap applications need reviewing.

Other than race and senior status, coefficients on only one other variable implies a substantial impact on the number of wiretap applications received: whether the judge currently serves as chief judge in a district requiring the chief to review all wiretap applications. The estimated coefficients on the interaction of the fraction of a judge's year spent as chief judge and the presence of a local rule requiring the chief judge to review all wiretap applications hover around .5 and .6. These values imply that in years in which a judge serves as chief, she receives between 1.83 and 2.19 wiretap applications on average. This result is consistent with the pattern observed in summary statistics; in districts with this "chief judge rule," chiefs receive 1.94 applications on average while their colleagues receive only .15 on average. The data suggest that where it exists, the "chief judge rule" is

followed, and the small number of applications received by other judges in these districts are likely attributable to days on which the chief is on vacation or otherwise not available.

Other aspects of service as chief judge bear little relationship to the number of wiretap applications received. The regressions in columns (3) and (4) show that judges who eventually serve as the chief judges of their districts receive fewer applications during the years in which they are not the chief. But once the regressions include district fixed effects, as in columns (5) through (8), the coefficient on this variable flips sign and its absolute magnitude falls by half. These patterns suggest that in districts where a higher fraction of judges have at some point held the chief judgeship – which tend to be smaller, rural districts – prosecutors use wiretaps less often. But, it also implies that within these districts, a judge who at some point serves as chief judge receives in the years when she is not the chief about the same number of wiretap applications as district colleagues who never serve as chief.

Similarly, the coefficient on the fraction of the year spent as chief judge implies that a chief judge in a district that does not require the chief to review all wiretap applications receives about the same number of applications as other judges in the district. The coefficient on the fraction of the year spent as chief judge is about -.03. In none of the regressions does it approach standard levels of statistical significance.

The specifications in columns (3) and (4) include indicators for whether the district has a local rule requiring the chief judge to review all wiretap applications. The regressions in the subsequent columns exclude this indicator because they include instead fixed effects for each district. The estimate for the "chief judge rule" suggests that judges in districts with this rule receive fractionally fewer wiretap applications (about .44 of an

application) per year than in districts without this rule. But there is no variation over time in this rule, and therefore, it is not possible to distinguish whether these cross-sectional differences in the use of wiretaps are attributable to this rule or to other differences across districts, such as the skill of prosecutors.

Other judicial characteristics have little bearing on the number of wiretap applications a judge receives. Perhaps surprisingly, characteristics that might associate with strong views on the criminal justice system do not correlate with the receipt of wiretap applications. The estimated impact of a judge's ideology score is far from standard levels of statistical significance, and perhaps contrary to what one might expect, its signs in the OLS equations imply that more liberal judicial attitudes correlate with receipt of more wiretap applications. The regressions in columns (3) and (4) show that judges with prior experience as prosecutors receive more wiretap applications. But when the equations include fixed effects for districts, as in the later columns, the estimated effect is cut in half and loses statistical significance. This pattern indicates that the use of wiretaps is more common in districts with more former prosecutors on the bench but that within districts, judges who have been prosecutors do not receive more wiretap applications. (A similar pattern emerges for female judges. Although none of the estimates for the gender of the judge are statistically significant, they suggest that wiretaps are more frequent in districts with more female judges but that within a district, male and female judges receive these applications with equal frequency.) Just as prior work as a prosecutor does not affect the number of wiretap applications a judge receives, a judge's prior service as a public or federal defender has little effect. None of its estimated

coefficients is statistically significant, and they are inconsistent across equations in their signs. At most, they imply a difference of one twentieth of a wiretap.

The numbers of wiretap applications received are count data, and a Poisson regression may be a more appropriate specification. To ease interpretation of the estimates, the paper reports OLS estimates. But column (9) reports results from a Poisson regression on the number of wiretaps received to verify that its estimates are comparable to OLS. The Poisson coefficients are similar to OLS' in their signs and magnitudes. An exception is the coefficient on whether a judge has ever served as her district's chief judge. This coefficient is marginally significant and negative. [more]

In sum, the estimates in Table 3 show that the incidence of wiretap applications correlates with two institutional aspects of a judge's availability to receive wiretap applications and with one judicial characteristic. A judge's retirement status correlates strongly with the number of wiretap applications she receives; judges who have taken senior status receive fewer applications. Where local rules restrict the choice of reviewing judge, prosecutors appear to comply with the rule. In districts that require the chief judges to evaluate all wiretap applications, the chief judges receive virtually all the wiretap warrant applications in the district. The only other judicial characteristic that correlates with wiretap applications is race: African-American judges receive fewer wiretap applications than other judges.

B. Types of Wiretaps.

Table 4 examines whether the estimated impact of race arises in all types of wiretap warrant applications and whether the estimates are an artifact of the unbalanced panel.

Each row of the table reports results from regressions having a different type or subgroup of wiretap warrants as the dependent variable but having the same set of covariates as that in column (7) of Table 3. Only the coefficient on the race variable is shown. To assess the magnitude of the estimates, the columns labeled (a) report the mean and standard deviation of the dependent variable, and the columns labeled (b) show the coefficient on the African-American indicator variable and its standard error. For the purpose of comparison, the first row reproduces the estimates from Table 3. It shows that African-American judges receive on average -.067 fewer wiretap applications per day, relative to an average of .172 wiretap applications received per day for all judges in the data.

The next row shows that similar results are obtained when the equation is estimated on the full data set rather than just the balanced panel of judges who appeared in the data for every year during 1997-2007. The coefficient of -.053 is slightly smaller in size than the baseline estimate, but it is well within the 95% confidence interval of the baseline estimate.

The next two rows show that the overwhelming majority of wiretaps, about 86%, arise in investigations of narcotics offenses. But the estimated impact of race is roughly the same for narcotics investigations and other types of investigations. The estimated race effect at -.060 is about 40% (in absolute value) of the average value for narcotics investigations (.148). The estimate for investigations not involving narcotics offenses is not statistically significant. But it is negative in sign and slightly less than a third of the sample mean.

The patterns for the location of the wiretap are similar. Wiretaps on cellular phones comprise 77% of all the wiretap applications in the sample. Cell phones and narcotics

investigations are highly correlated in the data. Over 82% of all wiretap applications in narcotics investigations seek to monitor cell phones, and over 91% of all applications to monitor cell phones arise in narcotics investigations. These patterns are consistent with the extensive use of cell phones in the narcotics trade. Relative to their sample means, the estimated impacts of race are roughly the same for wiretaps on both cellular and stationary telephones. The coefficients are each about 40% of their sample means, and both estimates are statistically significant.

The next set of rows break out the wiretap applications by the days of the week. The distribution of wiretap warrant applications rises consistently during the work week with the most activity on Fridays. On the weekends, judges authorize almost no wiretaps. The infrequency of wiretap authorization on weekends is likely due to the fact that judges do not regularly go to the courthouse on weekends. The estimated coefficients for race imply that African-American judges receive substantially fewer wiretap applications on each day of the week. The smallest impact occurs on Thursdays, and it is the only weekday for which the race coefficient is not statistically significant. On the whole, the results indicate that race affects the allocation of warrant applications across judges whenever prosecutors seek wiretaps in significant numbers.

The final rows decompose the wiretap warrant applications by the time of year. Prosecutors seek more wiretap warrants in the spring and summer months (March through August). The more extensive use of wiretaps in these seasons may be a response to greater activity in open-air drug markets in warmer weather months. Judicial race has a sizable negative impact in each season, and estimates are statistically significant in each season

except for spring. The results suggest that the influence of race is not limited to a particular time of year.

C. Pervasiveness of the Racial Gap.

Although the regressions in Table 3 included district fixed effects, Table 5 presents additional regressions that explore the pervasiveness of the racial gap in different types of districts. To ease exposition, the table reports only the coefficients for race and ideology and for the additional judicial characteristics. For comparison purposes, the estimates from the baseline regression of column (7) of Table 3 are repeated in the first column of Table 5.

The regression in column (2) shows that the racial gap in wiretap applications is not limited to the first or second half of the observation period. An interaction of race with an indicator variable for the years 2001-07 is a slightly positive coefficient. It suggests that the racial gap was slightly smaller in more recent years, but the estimate is not statistically different from zero. The regressions in the next two columns include controls for the extent to which prosecutors within a district make use of wiretaps. Wiretap usage was measured in two ways: simply the number of wiretap applications over the observation period, and the number of wiretaps per judge. Districts were ranked according to these measures, and the top ten districts according to each ranking were identified with indicator variables. The regression in column (3) reports the coefficient on the interaction of judicial race with the indicator for the wiretap count measure, and column (4) reports the estimate on the interaction of race and the indicator for the per-judge measure. Both interaction terms have coefficients of about -.030, but neither is statistically significant. The estimate on race itself dips from -.067 to about -.056, but it remains statistically different from zero.

These results hint at a slight expansion in the racial gap in districts with more regular use of wiretaps, but the weakness of these estimates prevents strong conclusions in this regard.

The equations in columns (5) and (6) of Table 5 examine how the racial gap varies with the number of African-American judges on a court. Greater numbers of African-American judges on a court may constrain a prosecutor's ability to ask only white judges to review wiretap applications. The small number of African-American judges on the bench hampers this test. Nearly 47% of the judges in the balanced panel sit in courts with at least two African-American judges, 28% sit on courts with two or three African-American judges, and 18% sit on courts with four or more African-American judges. The regressions in columns (5) and (6) report coefficients on the interactions of judicial race and indicator variables for the racial composition of a court. Their positive signs and the slightly more negative coefficient on the baseline estimate for race are consistent with a slightly larger racial gap in districts with only one African-American on the bench. But again, the coefficients on the interaction terms are not statistically significant.

The final two columns of Table 5 examine the influence of observations with zero values in the dependent variable. Some districts and some judges never receive any wiretap applications. The regression in column (7) excludes all observations in which the district in that year received no wiretap applications. These districts tend to be more rural, and the judges sitting in them are disproportionately white. Column (8) shows a regression that also excludes any judges who did not receive any wiretap applications during the entire observation period. On this basis, African-American and other judges are excluded in roughly equal proportions. The regressions in both columns (7) and (8) show that the racial gap is not attributable to observations with no wiretap applications. If anything, the

racial gap in wiretap applications grows slightly when these zero-valued observations are excluded. On the whole, the results in Table 5 show that the racial gap is pervasive and not limited to any particular time period or district court.

D. Other Dimensions of Judicial Experience.

Table 1 showed that race correlates with other judicial characteristics, such as prior experience, length of service on the bench, and age. These differences raise the possibility that omitted variables bias the estimated race effect. The regressions in Table 6 assess this possibility by including in the baseline regression a variety of control variables for a judge's professional experience. Again, the table reports only the coefficients of race, ideology, and the additional judicial characteristics to conserve space, and the estimates from the baseline regression of column (7) of Table 3 are repeated in the first column of Table 6.

With one exception, none of the estimates correlates strongly with the number of wiretap applications a judge receives. The exception is the indicator variable for when a judge has the least seniority on a district court, and it implies that these judges receive substantially fewer wiretap applications. But the least senior members of a court are typically judges who just been confirmed and join a court in mid-year. The estimate may therefore reflect the reduced exposure time of these judges rather than prosecutors' disfavoring judges with little seniority. But the effect of a judge's being the newest member of a court does not appear to have a differential. When the regression includes an interaction of the least-seniority variable with race, its coefficient is statistically

insignificant and close to zero, and the coefficients on race and least seniority themselves are unchanged.

Other aspects of a judge's professional experience have almost no relationship with the number of wiretaps a judge receives. For example, prior service as a state judge or attendance at an elite law school have estimated coefficients of only -.007 and .005, respectively, and their standard errors are more than seven times larger. Attendance at an Ivy League college correlates with receipt of fewer wiretap applications, but this estimate is marginally statistically significant. Importantly, the inclusion of these additional variables has virtually no impact on the estimates for race and ideology. Additional specifications are not reported here due to space constraints. Overall, omitted dimensions of professional experience do not appear to explain the race effect.

E. Race and Ideology.

Some of the largest observed differences between African-American and other judges are in the measures of ideology. The summary statistics in Table 1 show that about 75% of African-American judges are Democratic appointees and only 41% of other judges are, and the two groups of judges differ substantially in the Poole-Rosenthal score measure of ideology. These differences raise the question whether the gap in the rate of wiretap applications is attributable to race or to ideology. To evaluate this possibility, Table 7 reports the results of regressions that include additional measures of ideology. Again, the table reports only the coefficients on race and ideology in order to conserve space, but the regressions include the full set of covariates as in the equation in column (7) of Table 3. Column (1) of Table 5 repeats the baseline estimates for comparison purposes.

The equation in column (2) tests whether the effect of race is due to the combination of race and ideology. A possibility is that African-American judges are not merely more liberal than other judges on average because they are disproportionately Democratic appointees. But they might also be drawn from the most liberal end of the ideological distribution, and consequently, they might be more liberal than the average white Democratic appointee. If prosecutors avoid the most liberal judges, then they would pass over African-American judges disproportionately. A crude test of this hypothesis is to look at the summary statistics of the common space scores, and they do not provide support for the hypothesis. Table 1 shows that conditional on being appointed by a Democratic president, African-American and other judges have statistically indistinguishable ideology scores. Another test is to interact race and ideology in the regression. Under the hypothesis that the combination of race and ideology matter, an interaction term of race and ideology should have a sizable negative coefficient and the baseline coefficient on race should fall to zero. The equation in column (2) shows that this pattern is not present. Rather, the interaction of ideology is small, positive, and statistically insignificant.

When prosecutor chooses which judge will review the warrant application, he must choose from judges within his judicial district. For that reason, overall ideological scores may matter less than a judge's ideology relative to her peers within the district. Although the regressions include year and district fixed effects – as well as year-district interactions – a more precise measure of the ideological variation within a district is a ranking of judges within each district and year of their common space scores. Column (4) shows that the inclusion of the ranking (from most liberal to least liberal) does not effect the estimated

impact of race or ideology, and the coefficient on the ranking itself is effectively zero. The equation in column (5) includes both the ranking and its interaction with race as another test of the hypothesis that prosecutors avoid the most liberal judges and African-American judges might be disproportionately liberal. This interaction term is positive rather than negative, and far from statistically significant.

Columns (6) and (7) repeat this exercise using an indicator variable for whether the judge is the most liberal judge in her district. This specification tests whether prosecutors avoid the most left-leaning judge in the district, and as Table 1 showed, African-American judges are more likely to occupy this ideological position. The point estimate for being most liberal implies a reduction in the number of wiretaps a judge receives, but the coefficient and its interaction with race are statistically insignificant. Where a judge's ideology lies relative to her colleagues on the district court does not appear to explain the estimated effect of race. Although not reported here in order to conserve space, a reverse order ranking – ranking from most to least liberal – also produces estimates that are statistically insignificant and that do not diminish the baseline effect of race. Moreover, the presence of the alternative ideological measures has little effect on the coefficients on race and ideology. The estimated impact of judicial race does not appear to result from location of African-American judges on the ideological spectrum.

The measure of ideology employed thus far –common space scores – may be too crude in that it encompasses views on a variety of legal issues, such as the Commerce Clause, the Sherman Antitrust Act, or a host of federal policy issues not directly implicating law, which may be orthogonal to how a judge would review a wiretap warrant application. Schanzenbach and Tiller (2008) find in a sample of district courts that

Republican appointees assign longer sentences for certain crimes than Democratic appointees, and the Poole-Rosenthal scores may not capture these differences. As alternatives, there are two proxies of judicial attitudes that are more specific to the criminal justice system and law enforcement: the frequency with which the criminal defendants appearing before a judge receive sentences of incarceration and the average length of those sentences.

The equations in columns (7) and (9) include incarceration rates and average sentence lengths as additional control variables. Both of these variables associate with a lower incidence of wiretap applications, but their effects are statistically insignificant. The regressions in columns (8) and (10) include interactions of race and these criminal justice measures. The sign on the interaction terms suggest that African-American judges who mete out longer prison sentences and who give them out more often receive more wiretap warrant applications. But the sizes of these effects are modest and not statistically significant. The presence of these interaction terms raise rather than shrink the size of the race coefficient and suggest that race has an influence on the incidence of wiretap warrants separate from judicial attitudes on criminal justice. Although not shown in order to conserve space, equations in which sentences are measured in levels rather than natural logarithms produce similarly modestly-sized estimates of sentences and robust estimates of race. On the whole, the estimates of Table 6 indicate that the estimated race effects are not the product of poorly measured judicial ideology.

F. Additional District-level Characteristics.

I. U.S. Attorneys.

A growing literature shows that in a variety of contexts, decision-makers are prone to favor persons with characteristics similar to their own. Donohue and Levitt (2001) found that when the number of police belonging to a particular race rose, arrests of people of the opposite race increased. Antonovics and Knight (2004) reported that police search vehicles of drivers of a different race at higher rates than drivers of their own race. Price and Wolfers (2007) showed that referees in professional basketball games are more likely to call fouls against players of a different race than players of their own race. Kumar and Wolfers (2008) find that male equity analysts make consistently less favorable forecasts of the earnings of firms with female CEOs. The wiretap data do not permit a direct test whether such "own group bias" explains the racial gap in wiretap applications because the data do not report the identity of the line prosecutor who applied for the warrant.

But two other potential dimensions of own-race bias can be tested: the race of the supervising prosecutor and the race of the average defendant. A first hypothesis is that the race of the United States Attorney (USA), the head prosecutor in each district, may influence how race affects line attorneys' decisions. This influence may occur in several ways. The USA may monitor line attorneys, and when monitoring is not close, race may influence the decision-making of line attorneys. Alternatively, the race of the USA may affect prosecutors' decisions by changing their awareness of how race influences them.

The author collected the names and dates of service of all USAs during the observation period from the Library of Congress' Thomas Legislative Records file, and obtained biographical and demographic information about the USAs from Martindale-

Hubbell and internet searches. Table 6 reports results from regressions including USAs' characteristics as controls. In the monitoring account, the racial gap should be larger when a USA is less experienced or during transitions between USAs. Columns (2) and (3) show that the length of time a USA has served in the position does not substantially affect the racial gap in wiretap applications. The average tenure of a USA in these data is 3.2 years. The coefficient on indicator variable for the first or final years of a USA's service is close to zero and statistically insignificant. The coefficient on its interaction with a judge's race implies that the racial gap is larger during a transition in leadership. But neither of these estimates is statistically significant. The transition variables do not have a substantial effect on the estimated impact of a judge's race, and the results from these measures suggest that the racial gap is not the product of poor monitoring by lead prosecutors.

The equations in the next columns test another version of the own-group bias hypothesis: whether the racial gap in wiretap applications is more severe under white USAs. The number of African-American USAs between 1997 and 2007 was small. The author's research identified twenty-two of the 254 USAs who served in this time period as African-American. In column (8), the indicator for the race of the USA is close to zero and statistically insignificant, and the coefficient on the race of the judge is robust to its inclusion. In column (4), the interaction of the judge's race and the USA's race is positive at .004 and statistically insignificant. If the point estimates are taken at face value, they suggest that when the USA is African-American, line prosecutors still ask African-American judges to approve wiretap warrants less often than they do other judges (a difference of -.060 = -.067 + .011 + .004). Still, none of the coefficients on the USA

variables are statistically significant, and the coefficient on the judge's race remains unchanged and highly significant in the presence of these other controls.

A further possibility is that the racial gap may arise solely or most severely when USAs belong to one political party. A large literature documents that the judicial appointees of Republican presidents favor civil rights plaintiffs less often than Democratic appointees (Cox and Miles 2008, Sunstein et al. 2006). Also, Schanzenbach and Tiller (2007, 2008) find that a federal judge is more likely to depart from the Sentencing Guidelines in a direction consistent with her presumed ideological preference when her political affiliation aligns with the majority of the reviewing appellate court. Aware of these judicial proclivities, prosecutors may be sensitive to the racial and ideological composition of the bench. The observation period, 1997-2007, covers the change in administration from Clinton to Bush, and the political affiliation of the USAs correlates closely with time. The fraction of USAs who are Democratic appointees is nearly 100% in the earliest years of the data and nearly zero in the later years. Unsurprisingly, the turnover in political affiliation is concentrated in 2001, the year in which the administration changed. In view of this pattern and the fact that the regressions already include district and year fixed effects and district-year interactions, it is unlikely that political affiliation explains the racial gap. In an effort to capture more variation prosecutorial ideology, Poole-Rosenthal scores were calculated for each USA taking account of senatorial courtesy in the manner of Giles et al. (2001). The estimates in columns (6) and (7) include this measure of a USA's ideology together with the indicators for the USA's race. The estimates are small and statistically insignificant, and the coefficient on the baseline effect of race is unchanged.

II. Defendant Characteristics.

Own-group bias may operate in other ways. When a judge and a surveillance target belong to the same race, a judge may scrutinize the warrant application more closely than when they belong to different races. Knowing this, a prosecutor may bring a wiretap application to a judge whose race is different than that of the surveillance target. This response could produce the racial gap in wiretap warrant applications. The data do not record the race of the surveillance target, but it can be approximated with the United States Sentencing Commission's data on the racial distribution of defendants sentenced in each district in each year (U.S. Sentencing Commission various years). The percentage of African-American defendants is a rough proxy for the probability that the surveillance target is African-American.

Columns (8) through (10) display the results of regressions that control for this proxy for the race of the wiretap target. The regression in column (8) includes the percentage of sentenced defendants in each district and year who are African-American males. The subsequent two columns show the results of interacting this variable with the judge's race, and then with both the judge's race and ideology. Evaluated at the sample mean (32% of the sentenced defendants in the data are African-American males), the estimates imply that districts and years with more African-American male defendants have a slightly higher use of wiretaps. But the coefficients on the interaction of this variable with a judge's race and ideology are positive but close to zero. The estimates for this variable and its interactions are statistically insignificant. Their inclusion expands the size of the racial gap in wiretap applications but greatly increases the imprecision of the estimate. The standard error on the race coefficient doubles with the result that the

baseline race coefficient is marginally statistically significant in the presence of the race-defendant demographic interactions. On the whole, these versions of the own-group bias hypothesis do not appear to explain the racial gap in wiretap applications.

G. Wiretap Outcomes.

The results from the allocation of wiretap applications across judges show a sizable and robust racial disparity that is consistent with prosecutorial bias. The data do not support alternative explanations for the disparity such as omitted judicial characteristics, the confluence of race and ideology, the behavior of lead prosecutors, or own-group bias. Moreover, other judicial characteristics do not correlate with the number of wiretaps a judge receives. The correlation between wiretaps and judicial race – and exclusively judicial race – is consistent with racial bias by prosecutors.

This section further tests whether statistical discrimination (or judge-shopping) rather than utility-based discrimination explains the results. It examines the outcomes of the wiretaps. Under the judge-shopping hypothesis, prosecutors seek to minimize the probability that a judge will reject a wiretap application by bringing stronger wiretap applications to disfavored judges and weaker ones to favored judges. This account predicts that the average outcomes of wiretaps approved by African-American judges should be higher than those of other judges.

Table 9 tests the prediction by analyzing a series of wiretap outcomes. In these regressions, the unit of analysis is individual wiretaps rather than judge-by-year observations. To conserve space, only selected coefficients are reported.[17] The

[17] The regressions also include controls for a judge's proceutorial experience, criminal defense experience, sex, age, senior status, any service as chief judge, the interaction of chief judge service with the "chief judge

regressions in columns labeled (a) present baseline estimates, and those in column (b) are augmented with additional controls pertaining to the type and timing of the wiretap. Panel A of the table presents analyses for duration of the wiretap's surveillance (in number of days), the (log) number of conversations intercepted by the wiretap, and the (log) number of the intercepted conversations that were incriminating. The duration of wiretap surveillance can be considered a measure of its productivity because Title III provides that a court may authorize a wiretap for up to 30 days and may extend it for an additional thirty days upon reapplication (18 U.S.C. § 2510(5)). The statute does not limit the number of times a prosecutor may obtain an extension on a wiretap, but it requires that surveillance terminate as soon as the investigative objective is met. Investigators define the investigation broadly, such as identification of all conspirators, in order to avoid terminating the wiretap upon interception of the first evidence of criminality (O'Meara 1993).[18]

Panel B displays results for the (log) number of arrests and convictions and the number of motions to suppress the evidence. The results in Panel B should be viewed with some skepticism because the author's conversations with federal prosecutors raised doubts about the reliability of these measures. During the wiretap's operation, prosecutors collect information on the number of intercepts it picks up. They have a strong incentive to report this information accurately because they include this information in the 10-day reports they must file with the court during the duration of surveillance. In contrast, arrests,

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rule," and fixed effects for years, districts, district-year interactions, and the day of the week and month of the year in which the wiretap was approved.

[18] *See, e.g.*, *United States v. Armocida*, 515 F.2d 29, 38 (3d Cir.) ("Although the government has actual knowledge of a conspiracy and evidence sufficient to prosecute one of the conspirators, it is unrealistic to require the termination of an investigation before the entire scope of the [criminal enterprise] is uncovered."), cert. denied, 423 U.S. 858 (1975).

convictions, and motions to suppress are outcomes realized only long after the wiretap is concluded. By that time, other cases and priorities likely occupy the prosecutor who conducted the wiretap surveillance, and once surveillance ends, there appear to be no ill consequences for failing to report the number of resulting arrests and convictions to the Administrative Office of the Courts. The variables often have zero values, and it is unclear whether this reflects the low rate at which wiretaps result in arrests and prosecutions or whether it is due to inaccurate reporting by prosecutors. For example, 67% of wiretaps result in no reported arrests, 78% in no reported convictions, and 96% in no reported motions to suppress.[19, 20]

While there are many zero values, the nonzero values have large variances. To assure that the estimates are not driven by outliers, several variables are expressed in natural logarithms. Where the outcomes were zero, the dependent variables expressed in logs were assigned values of zero, and the regression included an indicator variable taking the value one whenever the wiretap outcome was zero (Pakes and Griliches 1980).

With these caveats in mind, the results do not support the judge-shopping hypothesis. The coefficients on a judge's ideology are not statistically significant in any of the regressions and that on race is not significant in most regressions. The judge-shopping hypothesis predicts that the outcomes of wiretaps approved by African-American judges

[19] These figures overstate arrest, conviction, and motion-to-dismiss outcomes of some individual wiretaps and understate those of others. Where several wiretaps are related or part of the same investigation, the Administrative Office reports only the aggregate outcomes of individual wiretaps within the family of related wiretaps. The outcomes of individual wiretaps within a family cannot be determined. For these wiretaps, the average outcome within the family has been assigned to each family member. Other outcomes – duration and the number of intercepts and incriminating intercepts – are known for each individual wiretap.

[20] The rate of motions to suppress is overstated. The Administrative Office reported motions granted, denied, and pending in each annual update, but the updates do not distinguish whether the grants and denials pertain to new motions or previously pending motions. The actual rate at which defendants challenge wiretap evidence is therefore less than 4%. By comparison, Nardulli (1983) reported that in a sample of criminal prosecutions in county courts defendants made motions to suppress physical evidence 3.1% of cases.

should be higher, but the estimated race effects are negatively signed. These patterns remain even when the regressions control for other features of the wiretaps such as the type of device monitored and the nature of the investigation.

The coefficients on the other characteristics of the wiretaps provide some insights into the use of this investigative technique. The estimates indicate that wiretaps in narcotics investigations have durations that are shorter by about 10 days, and according to estimates that are statistically significant at the 10% level, they are more likely to result in arrests and convictions. The outcomes of wiretaps that are spinoffs or offspring from earlier wiretaps are consistent with diminishing marginal returns. They generate fewer intercepts, incriminating intercepts, arrests, and convictions. Wiretaps that are the parents of these offspring have on average substantially longer durations, more intercepts, and fewer arrests and convictions. The longer durations and more intercepts of parent wiretaps are consistent with their furthering the investigation and leading to further monitoring. Their lower productivity in terms of arrests and convictions is attributable to the assignment of average values to each member of the family of related wiretaps. The final variable reported is the number of wiretaps authorized nationwide in that week. This measure attempts to control for whether congestion in the OEO influences the quality of wiretaps the DOJ allows to proceed to judicial review. In most of these regressions, its coefficient is statistically insignificant and small (in view of its sample mean of 14). Although these data do not permit an evaluation of whether federal prosecutors use wiretaps efficiently, the results for these characteristics accord with common intuitions.

5. Discussion and conclusion.

Discriminatory enforcement is commonly suspected in the criminal justice system, but its presence is difficult to detect. The process of obtaining judicial approval furnishes an opportunity to test whether a particular set of actors in that system, federal prosecutors, has discriminatory preferences. The demanding standards and rigorous review of wiretap applications implies that all applications that proceed to the stage of judicial approval exceed the relevant legal standards and receive approval. In this environment, the competing hypothesis of statistical discrimination can be ruled out because prosecutors have no incentive to shop for favorably disposed judges. The evidence presented in this paper supports this view. Judicial characteristics such as experience and ideology do not correlate with the frequency with which prosecutors seek a judge's review of a wiretap application. In addition, judicial characteristics do not correlate with the outcomes of wiretaps, such as the rates at which they produce evidence or arrests. The racial gap in wiretap applications is plausibly interpreted to reflect Beckerian utility-based discrimination.

The nature of the process and the kind of decision-maker studied in this paper provide good reason to believe that the estimated effects understate the influence of race on other actors in the criminal justice system. Federal prosecutors are in many ways the elites of law enforcement. They are lawyers, often trained at prestigious law schools, who typically come to the job only after several years of legal practice. They are well aware that virtually all of their decisions could be scrutinized by defense counsel and provide the basis of an appeal. The decision they make here, the choice of which judge will review and inevitably sign a warrant, is not terribly consequential. It is a decision made within an

office building, often without time pressure. In contrast, other actors such as police, have far less training in the law and receive it at less elite institutions. Their decision-making is frequently not monitored and its consequences are often not immediately apparent. Their choices must sometimes be made rapidly and under conditions that are highly pressurized or that present imminent danger to themselves and others. In view of these differences, the effect of race estimated in this paper is likely a lower bound on the impact of race in the criminal justice system generally.

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Table 1. Judge Characteristics: Summary Statistics

	Judge's Race:				Judge's Race:		
Characteristic:	African-American:	Other:	Difference:	Characteristic:	African-American:	Other:	Difference:
Ideology (1= most conservative; -1 = most liberal)	-.205 (.039)	.093 (.016)	-.298** (.051)	Ever Served as Chief Judge	.328 (.059)	.441 (.020)	-.113* (.065)
Democratic Appointee	.750 (.055)	.413 (.020)	.337** (.064)	Prosecutorial Experience	.578 (.062)	.460 (.020)	.118** (.065)
Ideology (if Democratic Appointee)	-.363 (.020)	-.318 (.009)	-.045* (.023)	Years as Prosecutor (if prosecutorial experience)	6.919 (.912)	6.248 (.290)	.671 (.875)
Liberal Rank within District	2.484 (.207)	3.356 (.701)	-.872** (.230)	Criminal Defense Experience	.109 (.039)	.038 (.008)	.071** (.027)
Liberal Rank within District (if Democratic Appointee	1.688 (.112)	1.850 (.047)	-.163 (.120)	Served as State Judge	.516 (.063)	.359 (.019)	.157** (.063)
Most Liberal within District	.375 (.061)	.160 (.015)	.215** (.050)	Years as State Judge (if served as state judge)	8.242 (.842)	9.407 (.390)	-1.165 (1.070)
Percentage of Convicted Defendants the Judge Sentenced to Prison	77.656 (1.614)	79.413 (.553)	-1.757 (1.805)	Served in State Legislative or Executive Branch (non-prosecutorial)	.219 (.052)	.195 (.016)	.024 (.052)
Median Length of Sentence Given by Judge (years)	2.418 (.133)	2.671 (.055)	-.253 (.177)	Served in Federal Legislative or Executive Branch (non-prosecutorial)	.219 (.052)	.149 (.014)	.070 (.048)
Female	.188 (.049)	.146 (.014)	.041 (.047)	Attended Elite Law School	.250 (.055)	.173 (.015)	.077 (.050)
Age	56.078 (1.227)	62.117 (.403)	-6.039** (1.324)	Attended Ivy League College	.109 (.039)	.133 (.014)	-.024 (.044)
Years on the Bench	9.125 (.909)	12.646 (.330)	-3.521** (1.076)	Served as Law Clerk to Another Judge	.125 (.042)	.219 (.016)	-.094* (.053)
Years as Lawyer	29.563 (1.138))	36.102 (.415)	-6.539** (1.351)	Chief Judge Rule	.125 (.042)	.202 (.016)	-.077 (.052)
Fraction of Time on Senior Status	.109 (.039)	.277 (.018)	-.168** (.057)	Number of Judges in District	18.969 (1.223)	15.679 (.427)	3.289** (1.340)

Note: Except where otherwise specified, the total number of observations is 694 with 630 white judges and 64 African-American judges.

Table 2. Estimates of Racial Gap in Wiretap Applications: Mean Differences

	Total Number of Wiretaps			Total Number of Wiretaps per Number of Days the Judge was a Member of the Court (x 100)		
		Retirement Status of Judge:			Retirement Status of Judge:	
Race of Judge:	All (1a)	Active Status (1b)	Senior Status (1c)	All (2a)	Active Status (2b)	Senior Status (2c)
(A) African-American:	.557	.679	.083	.153	.186	.023
	(.044)	(.054)	(.039)	(.012)	(.015)	(.011)
	[704]	[560]	[144]	[704]	[560]	[144]
(B) Other:	.635	.914	.256	.175	.251	.070
	(.021)	(.034)	(.017)	(.006)	(.009)	(.005)
	[7,634]	[3,996]	[2,933]	[7,634]	[3,996]	[2,933]]
Difference of (A) – (B):	-.079	-.236**	-.173**	-.022	-.065**	-.047**
	(.068)	(.093)	(.078)	(.019)	(.026)	(.021)

Note – An asterisk * denotes difference statistically significant at the 10% level, and double asterisks ** denote differences statistically significant at the 5% level.

Table 3. Regression Estimates of Racial Gap in Wiretap Applications

Explanatory Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
African-American Judge	-.030	-.071**	-.067**	-.067**	-.067**	-.067**	-.067**	-.067**	-.254**
	(.024)	(.024)	(.022)	(.022)	(.025)	(.025)	(.025)	(.026)	(.118)
Judge's Ideology			-.010	-.010	-.015	-.015	-.015	-.015	.075
			(.028)	(.028)	(.035)	(.035)	(.035)	(.037)	(.110)
Prosecutorial Experience			.050**	.050**	.027	.027	.027	.027	.074
			(.023)	(.024)	(.024)	(.022)	(.022)	(.023)	(.087)
Criminal Defense Experience			.009	.009	-.012	-.012	-.012	-.012	-.012
			(.047)	(.047)	(.037)	(.044)	(.044)	(.047)	(.181)
Female			.044	.044	.018	.018	.018	.018	.018
			(.038)	(.039)	(.037)	(.037)	(.037)	(.039)	(.127)
Age (in years)			-.0005	-.0005	-.0006	-.0005	-.0005	-.0003	-.006
			(.0015)	(.0016)	(.0017)	(.0017)	(.0017)	(.0019)	(.007)
Fraction of Year on Senior		-.186**	-.138**	-.138**	-.141**	-.142**	-.142**	-.146**	-1.055**
Status		(.021)	(.033)	(.033)	(.031)	(.032)	(.032)	(.034)	(.155)
Ever Served as Chief Judge			-.057**	-.056**	.004	.003	.003	.001	-.246*
			(.014)	(.014)	(.026)	(.026)	(.026)	(.027)	(.147)
Fraction of Year Spent as Chief			-.019	-.020	-.031	-.028	-.027	-.027	.041
Judge			(.022)	(.022)	(.023)	(.023)	(.026)	(.024)	(.124)
Chief Judge Rule			-.121**	-.121**	--	--	--	--	--
			(.013)	(.013)					
(Chief Judge Rule) x (Fraction			.530*	.528**	.613*	.612**	.618*	.640*	3.276**
of Year Spent as Chief Judge)			(.209)	(.209)	(.243)	(.243)	(.247)	(.268)	(.248)
Fixed Effects:									
Year	Y	Y		Y	Y	Y	Y	Y	Y
District	Y	Y			Y	Y	Y	Y	Y
Number of Judges	Y	Y				Y	Y	Y	Y
District x Year (continuous)							Y		Y
District x Year (fixed effects)	Y	Y						Y	
Estimation Method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	Poisson
R-square	.0871	.1167	.0708	.0747	.1330	.1402	.1543	.1946	--

Note – N=7,634. The dependent variable in each regression is the total number of wiretap applications received by a judge per the number of days the judge was a member of the court in each year (multiplied by 100). Standard errors are clustered by judge. An asterisk * denotes coefficients statistically significant at the 10% level, and double asterisks ** denote coefficients statistically significant at the 5% level.

Table 4. Regression Estimates of Racial Gap by Types and Timing of Wiretap Applications

	Balanced Panel Estimate of Taps per Days Judge Worked (1)		Balanced Panel Estimate of Taps (2)	
Dependent Variable:	Mean [S.D.] of Dependent Variable (1a)	Race Coeff. (standard error) (1b)	Mean [S.D.] of Dependent Variable (2a)	Race Coeff. (standard error) (2b)
Total Wiretap Applications	.172 [.474]	-.067** (.025)	.628 [1.728]	-.243** (.091)
Total Wiretap Applications (Full Panel; N=10,962)	.171 [.465]	-.053** (.019)	.610 [1.633]	-.187** (.071)
Offense Investigated:				
Applications in Narcotics Investigations	.148 [.431]	-.060** (.023)	.539 [1.571]	-.218** (.084)
Applications in Other Investigations	.024 [.117]	-.007 (.006)	.089 [.429]	-.025 (.020)
Type of Device:				
Applications to Wiretap Cellular Telephones	.133 [.407]	-.053** (.021)	.486 [1.483]	-.191** (.078)
Applications to Wiretap Other Devices	.039 [.142]	-.015** (.006)	.142 [.518]	-.052** (.022)
Day of Week of Wiretap Application:				
Monday	.027 [.108]	-.008** (.004)	.098 [.392]	-.028* (.016)
Tuesday	.030 [.117]	-.017** (.005)	.108 [.428]	-.060** (.020)
Wednesday	.032 [.123]	-.015** (.006)	.116 [.448]	-.054** (.022)
Thursday	.036 [.136]	-.007 (.007)	.133 [.496]	-.024 (.027)
Friday	.045 [.162]	-.020** (.008)	.164 [.590]	-.072** (.028)
Weekend	.003 [.030]	-.0015 (.0010)	.010 [.108]	-.006 (.004)
Season of Wiretap Application:				
Winter	.036 [.138]	-.014** (.007)	.098 [.404]	-.040** (.020)
Spring	.054 [.203]	-.015 (.010)	.161 [.632]	-.044 (.032)
Summer	.045 [.177]	-.022** (.008)	.137 [.551]	-.069** (.024)
Autumn	.037 [.148]	-.017** (.007)	.111 [.467]	-.055** (.022)

Note: N = 7,634. Columns labeled (a) report the mean and standard deviation of the dependent variable, and columns labeled (b) report the coefficient on a judge's race in regressions with the same set of covariates as column (7) of Table 3. Coefficients on other variables are not reported in order to conserve space.

Table 5. Alternative Specifications of O.L.S. Estimates: Pervasiveness of the Racial Gap

Explanatory Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
African-American Judge	-.067**	-.080**	-.058**	-.056**	-.083**	-.083**	-.074**	-.085**
	(.022)	(.027)	(.023)	(.026)	(.041)	(.041)	(.026)	(.038)
Judge's Ideology	-.010	-.015	-.030	-.015	-.015	-.015	-.025	-.049
	(.028)	(.035)	(.038)	(.035)	(.035)	(.035)	(.039)	(.052)
(African-American) x (Year after 2001)		.020						
		(.030)						
(African-American Judge) x (One of Ten Districts with Most Wiretap Applications)			-.030					
			(.058)					
(African-American Judge) x (One of Ten Districts with Most Wiretap Applications per Judge)				-.027				
				(.047)				
(African-American Judge) x (District with Two or More African American Judges)					.021			
					(.049)			
(African-American Judge) x (District with Two or Three African American Judges)						.035		
						(.052)		
(African-American Judge) x (District with Four or More African American Judges)						.001		
						(.054)		
Exclude Districts without any Wiretap Applications in the year?							Y	Y
Exclude Judges who Never Receive any Wiretap Applications?								Y
N	7,634	7,634	7,634	7,634	7,634	7,634	6,428	4,733
R-square	.1543	.1543	.1543	.1543	.1543	.1544	.1758	.1918

Note: The dependent variable is the total number of wiretap applications received by a judge per the number of days the judge was a member of the court in each year (multiplied by 100). Except where specified, the equations include with the same set of covariates as column (7) of Table 3. Only selected coefficients on other variables are reported in order to conserve space. An asterisk * denotes coefficients statistically significant at the 10% level, and double asterisks ** denote coefficients statistically significant at the 5% level.

Table 6. Alternative O.L.S. Specifications of Racial Gap: Judicial Characteristics

Explanatory Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
African-American Judge	-.067**	-.071**	-.069**	-.072**	-.068**	-.071**	-.073**	-.083**	-.081**	-.083**
	(.022)	(.028)	(.025)	(.026)	(.025)	(.026)	(.026)	(.030)	(.028)	(.028)
Judge's Ideology	-.015	-.018	-.022	-.021	-.016	-.017	-.015	-.021	-.026	-.026
	(.035)	(.036)	(.035)	(.036)	(.035)	(.036)	(.038)	(.040)	(.039)	(.040)
Years on the Bench		.003						.003		
		(.002)						(.002)		
Years as a Lawyer Before Joining the Bench		-.002						-.002		
		(.006)						(.006)		
Most Senior Active Judge within District			.017	.014					.017	.014
			(.045)	(.049)					(.045)	(.049)
Least Senior Active Judge within District			-.077**	-.078**					-.077**	-.078**
			(.029)	(.032)					(.029)	(.032)
(African-American Judge) x (Most Senior Active Judge within District)				.027						.027
				(.077)						(.077)
(African-American Judge) x (Least Senior Active Judge within District)				.006						.006
				(.059)						(.059)
Years as Prosecutor (if served as prosecutor)					.002			.003	.002	.002
					(.002)			(.002)	(.002)	(.002)
Served as State Judge						-.007		-.003	-.007	-.007
						(.023)		(.024)	(.024)	(.024)
Served in State Legislative or Executive Branch (non-prosecutorial)						.056		.057	.058	.058
						(.039)		(.038)	(.038)	(.039)
Served in Federal Legislative or Executive Branch (non-prosecutorial)						.016		.013	.017	.017
						(.032)		(.033)	(.032)	(.032)
Attended Elite Law School							.003	.005	.005	.004
							(.032)	(.031)	(.032)	(.032)
Attended Ivy League College							-.041*	-.040*	-.040	-.040
							(.024)	(.024)	(.026)	(.026)
Served as Law Clerk							-.022	-.018	-.023	-.023
							(.025)	(.024)	(.024)	(.025)
R-square	.1543	.1546	.1552	.1552	.1545	.1561	.1552	.1586	.1584	.1584

Note: N = 7,634. The dependent variable is the total number of wiretap applications received by a judge per the number of days the judge was a member of the court in each year (multiplied by 100). Except where specified, the equations include with the same set of covariates as column (7) of Table3. Only selected coefficients on other variables are reported in order to conserve space. An asterisk * denotes coefficients statistically significant at the 10% level, and double asterisks ** denote coefficients statistically significant at the 5% level.

Table 7. Alternative O.L.S. Specifications of Racial Gap: Ideology and Race

Explanatory Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
African-American Judge	-.067**	-.063**	-.065**	-.087**	-.063**	-.062**	-.073**	-.226**	-.071**	-.128**
	(.025)	(.018)	(.024)	(.042)	(.024)	(.024)	(.026)	(.095)	(.027)	(.052)
Judge's Ideology	-.015	-.016	-.078	-.078	-.045	-.045	-.015	-.014	-.009	.010
	(.035)	(.037)	(.086)	(.086)	(.052)	(.052)	(.036)	(.036)	(.037)	(.037)
(African American Judge) x (Judge's Ideology)		.021 (.064)								
Liberal Rank within District			.016 (.015)	.016 (.015)						
(African American Judge) x (Liberal Rank within District)				.009 (.013)						
Most Liberal with District					-.054 (.038)	-.054 (.039)				
(African American Judge) x (Most Liberal within District)						-.002 (.047)				
Fraction of Criminal Convictions Sentenced to Incarceration							-.016 (.080)	-.030 (.082)		
(African-American Judge) x (Fraction of Criminal Convictions Sentenced to Incarceration)								.198 (.125)		
(Log) Median Sentence Length (in years)									-.049 (.045)	-.055 (.049)
(African-American Judge) x ((Log) Median Sentence Length (in years))										.071 (.047)
N	7,634	7,634	7,634	7,634	7,634	7,634	7,227	7,227	7,084	7,084
R-square	.1543	.1543	.1548	.1549	.1554	.1554	.1552	.1554	.1534	.1538

Note: The dependent variable is the total number of wiretap applications received by a judge per the number of days the judge was a member of the court in each year (multiplied by 100). Except where specified, the equations include with the same set of covariates as column (7) of Table 3. Only selected coefficients on other variables are reported in order to conserve space. An asterisk * denotes coefficients statistically significant at the 10% level, and double asterisks ** denote coefficients statistically significant at the 5% level.

Table 8. Alternative O.L.S. Specifications of Racial Gap: District-level Characteristics

Explanatory Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
African-American Judge	-.067**	-.067**	-.059**	-.067**	-.067**	-.072**	-.074**	-.067**	-.087*	-.084*
	(.025)	(.025)	(.026)	(.025)	(.027)	(.025)	(.027)	(.025)	(.051)	(.052)
Judge's Ideology	-.015	-.015	-.015	-.015	-.015	-.015	-.015	-.015	-.015	-.016
	(.035)	(.035)	(.035)	(.035)	(.035)	(.035)	(.035)	(.035)	(.035)	(.035)
U.S. Attorney's First or Last Year in Office		-.0004	.002							
		(.0115)	(.012)							
(African-American Judge) x (U.S. Attorney's First or Last Year in Office)			-.026							
			(.026)							
African-American U.S. Attorney				.010	.011		.015			
				(.025)	(.026)		(.026)			
(African-American Judge) x (African-American U.S. Attorney)					-.004		.015			
					(.058)		(.061)			
U.S. Attorney's Ideology						.029	.030			
						(.029)	(.029)			
(African-American Judge) x (U.S. Attorney's Ideology)						.030	.034			
						(.033)	(.034)			
Fraction of District's Defendants are African-American Males								.216*	.212*	.214*
								(.025)	(.112)	(.111)
(African-American Judge) x (Fraction of District's Defendants are African-American Males)									.053	.046
									(.122)	(.130)
(Judge's Ideology) x (Fraction of District's Defendants are African-American Males)										-.022
										(.113)
R-square	.1543	.1543	.1543	.1543	.1545	.1545	.1545	.1545	.1545	.1545

Note: N = 7,634. The dependent variable is the total number of wiretap applications received by a judge per the number of days the judge was a member of the court in each year (multiplied by 100). Except where specified, the equations include with the same set of covariates as column (7) of Table 3. Only selected coefficients on other variables are reported in order to conserve space. An asterisk * denotes coefficients statistically significant at the 10% level, and double asterisks ** denote coefficients statistically significant at the 5% level.

Table 9.A. Judicial Race and Wiretap Outcomes

	Dependent Variable:					
	Days Wiretap Was in Operation (1)		(Log) Number of Conversations Intercepted (2)		(Log) Number of Incriminating Conversations Intercepted (3)	
Explanatory Variable:	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
African-American Judge	-2.586	-2.324	-.108	-.106	-.157*	-.158*
	(1.798)	(1.752)	(.095)	(.092)	(.082)	(.083)
Judge's Ideology	1.719	1.980	.025	.019	.013	.007
	(1.472)	(1.430)	(.075)	(.075)	(.077)	(.076)
Wiretap Produced Spin-off Wiretaps ("Parent")		-17.788**		.493**		.503**
		(1.059)		(.074)		(.089)
Wiretap was a Spin-off from another Wiretap ("Offspring")		-1.322		-.226**		-.224**
		(1.517)		(.063)		(.057)
Wiretap of a Cellphone		-3.140**		-.232**		.067
		(1.562)		(.078)		(.069)
Narcotics Investigation		-9.767**		.029		.074
		(2.750)		(.101)		(.094)
Number of Other Wiretaps Approved Nationwide that Week		.080		.011*		-.006
		(.118)		(.006)		(.006)
Mean [Standard Deviation] of Dependent Variable (in logs if applicable)			6.708 [2.134]		4.779 [2.081]	
Mean [Standard Deviation] of Dependent Variable (unlogged)	40.422 [33.449]		2,673.9 [4,980.6]		473.6 [1,914.0]	
R-square	.0689	.1160	.4495	.4545	.4856	.4944

Note: N=4,796. The regressions also include controls for a judge's prosecutorial experience, criminal defense experience, sex, age, senior status, any service as chief judge, the interaction of chief judge service with the "chief judge rule," and fixed effects for years, districts, district-year interactions, and the day of week and month of year in which the wiretap application was approved. Coefficients on these other variables are not reported in order to conserve space. An asterisk * denotes coefficients statistically significant at the 10% level, and double asterisks ** denote coefficients statistically significant at the 5% level.

Table 9.B. Judicial Race and Wiretap Outcomes

	Dependent Variable:					
	(Log) Number of Arrests (1)		(Log) Number of Persons Convicted (2)		Number of Motions to Suppress Wiretap Evidence (3)	
Explanatory Variable:	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
African-American Judge	-.052	-.048	-.072**	-.069**	-.012	-.009
	(.043)	(.041)	(.031)	(.031)	(.080)	(.080)
Judge's Ideology	-.023	-.024	-.020	-.019	-.041	.049
	(.032)	(.032)	(.025)	(.026)	(.056)	(.058)
Wiretap Produced Spin-off Wiretaps ("Parent")		-.240**		-.164**		.180
		(.041)		(.027)		(.117)
Wiretap was a Spin-off from another Wiretap ("Offspring")		-.047**		-.045**		-.164
		(.017)		(.012)		(.043)
Wiretap of a Cellphone		.003		.040*		-.045
		(.028)		(.023)		(.089)
Narcotics Investigation		.072*		.046*		-.005
		(.039)		(.028)		(.061)
Number of Other Wiretaps Approved Nationwide that Week		.004**		.0009		.009
		(.002)		(.0018)		(.007)
Mean [Standard Deviation] of Dependent Variable (in logs if applicable)	.782 [1.123]		.369 [.852]			
Mean [Standard Deviation] of Dependent Variable (unlogged)	4.306 [8.795]		1.955 [5.906]		.166 [1.611]	
R-square	.6657	.6701	.6843	.6882	.0864	.0902

Note: N=4,796. The regressions also include controls for a judge's prosecutorial experience, criminal defense experience, sex, age, senior status, any service as chief judge, the interaction of chief judge service with the "chief judge rule," and fixed effects for years, districts, district-year interactions, and the day of week and month of year in which the wiretap application was approved. Coefficients on other variables are not reported in order to conserve space. An asterisk * denotes coefficients statistically significant at the 10% level, and double asterisks ** denote coefficients statistically significant at the 5% level.