Racial Disparities in the Allocation of Wiretap Applications across Federal Judges

Thomas J. Miles[*]
University of Chicago Law School

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Abstract

Electronic surveillance or wiretapping is a powerful and intrusive tool in criminal investigations. As with searches for physical evidence, investigators must obtain a warrant from a judge before wiretapping conversations. But before seeking judicial approval for a wiretap, federal investigators must satisfy additional requirements, including obtaining authorization from the Department of Justice. The Department's strict review of wiretap applications implies that federal judges approve all of the wiretap applications they receive. This setting provides a test for whether prosecutors harbor racial bias. When prosecutors are unbiased, judges within a district should receive equal numbers of wiretap applications. If prosecutors are biased, the allocation of wiretap applications across judges within a district should correlate with judges' race. The paper tests these predictions using a data set of all wiretaps used in federal criminal investigations during the years 1997-2006. The results show that African-American judges receive substantially fewer wiretap applications than white judges, even after controlling for other judicial and district characteristics. Yet, prosecutors do not appear to "shop" for favorably disposed judges. Other than race, judicial characteristics including ideology and professional experience do not correlate with the number of wiretap applications received. Nor do prosecutors appear to match the quality of a wiretap to specific judges; the wiretaps approved by African-American and white judges do not differ in the rates at which they produce incriminating evidence, arrests, or convictions. The results suggest that even sophisticated actors in the criminal justice system, such as federal prosecutors, may be biased.

1. Introduction

The criminal justice system is marked by sharp racial disparities, and an important and highly contested question is the degree to which these disparities result from discrimination. Much of the law of criminal procedure seeks to rid the criminal justice system of the influence of arbitrary factors such as race. Judges, for example, must consult a set of guidelines before sentencing a defendant. Police must have reasonable suspicion to stop and search individuals, and must have probable cause to make arrests. Despite these procedural protections, racial disparities in sentencing and policing persist.

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 provides a test for the presence of bias in criminal investigations. As with searches for physical evidence, investigators must obtain a warrant from a judge before wiretapping conversations. But unlike searches for physical evidence, federal investigators must also satisfy additional requirements, including a highly specific probable cause standard and authorization from the Department of Justice (DOJ) before seeking judicial approval. The Department's strict review of wiretap applications implies that federal judges approve all of the wiretap applications they receive. In most jurisdictions, prosecutors have discretion to choose the judge within the district who will review the application for a wiretap warrant. But the stringency of the Department's review implies that prosecutors have no incentive to exercise this discretion by "shopping" for a favorable judge. After the Department has authorized a prosecutor to seek judicial approval for a wiretap, the chance a judge rejects it is effectively zero. Any judge will approve the application. Thus, prosecutors have no

incentive to seek favorably disposed judges or to avoid unfavorably disposed ones. The allocation of wiretap applications across judges should be nearly uniform.

In this setting, the allocation of wiretap applications across judges within a district reveals whether prosecutors are biased. If prosecutors are unbiased, they should pick judges to approve wiretap applications on a roughly random basis. Judges should receive approximately equal numbers of wiretap applications, and the number of applications judges receive should not correlate with any of their characteristics. In contrast, if prosecutors are biased, they may seek approval for wiretaps from minority judges less often. Minority judges' receipt of fewer wiretap applications is consistent with prosecutorial bias.

The paper tests these predictions using a data set of all wiretaps used in federal criminal investigations during the years 1997-2006. The results show that African-American judges receive substantially fewer wiretap applications than white judges, even after controlling for differences in ideology, professional experience, and other factors. The racial gap in wiretap applications does not appear to result from prosecutors' shopping for favorable judges. The allocation of wiretap applications across judges is unrelated to other judicial characteristics that correlate with attitudes toward the criminal justice system and law enforcement. Further evidence that the racial gap in wiretap applications is due to prosecutor attitudes rather than judge-shopping is that the wiretaps approved by African-American and white judges do not differ in the rates at which they produce incriminating evidence, arrests, or convictions. Overall, the results suggest that even sophisticated actors in the criminal justice system, such as federal prosecutors, may suffer from biases.

The paper has much in common with – but also important differences from – the growing literature on "outcome analysis." The outcome approach tests whether racial prejudice figures into the preferences of a decision-maker by examining racial disparities in both inputs and outputs. 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). In contrast, a decision-maker has a *taste for discrimination* or is prejudiced 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. But the statistical discriminator does so in order to maximize the joint outcomes from his interactions with both groups. In contrast, the prejudiced decision-maker tolerates a lower rate of success in the minority group's outcomes. This lower rate of success is the cost of indulging his taste in discrimination, and is the observable difference between statistical and prejudiced discriminators. Leading applications of outcome analysis include Knowles' et al. (2001) examination of racial profiling and Ayres and Waldfogel's (1994) study of bail.[1]

Wiretaps are not a setting in which the now-standard predictions of outcome analysis apply primarily because the identity of the judge who reviews a wiretap application is orthogonal to its subsequent success. The Department's rigorous authorization process assures that judges will approve all wiretap applications that reach them. A competing hypothesis is that the probability of judicial approval is less than one,

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[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 obtains 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 prosecutors "judge shop" to have their application reviewed by a favorably disposed judge. Under this hypothesis, prosecutors would be statistical discriminators who use race to predict judicial attitudes. But the judge-shopping hypothesis can be rejected for several reasons. If it occurred, prosecutors would bring only the strongest applications to disfavored judges, and these wiretaps would yield the highest outcomes. The data reject this hypothesis. Also, judicial characteristics other than race, such as ideology, should predict judicial attitudes and shopping should occur across these dimensions as well. But they do not. Aside from certain institutional features that govern the availability of judges to receive applications, the distribution of wiretap applications across judges correlates only with judicial race. Wiretaps are a perhaps unique setting in which prosecutorial preferences alone should explain any correlations between judicial characteristics and the allocation of wiretap applications.

The plan of the paper follows. Section 2 describes the procedures federal prosecutors must follow in obtaining permission to conduct wiretap surveillance, and it predicts how these procedures might 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 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.[2] As a

practical matter, federal prosecutors always seek approval for wiretap warrants from

federal district judges rather than state judges. 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 that (1) 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 reasons why alternative techniques are

likely to fail or be excessively dangerous (O'Meara 1993).

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[2] 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).

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. § 1516(1)). The Department of Justice responded to this requirement by creating a relatively complex bureaucratic process for reviewing wiretap warrants before they proceed to the high-ranking officials for approval. The purpose of this review and the goal of the attorneys who conduct it is to achieve meticulous compliance with the statute in order to prevent any future restrictions on the use of wiretaps.[3]

This process consists of roughly four 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 Office of Enforcement Operations in the Department of Justice (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

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[3] 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).

application can 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 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 are 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).[4] 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.

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

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[4] 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).

be chosen. Some districts have adopted under 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."[5] But these districts tend to be smaller, and judges sitting

in such districts account for only 9.5% of the observations in the judge-by-year 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 coordinating these teams can be a "logistical nightmare" (Styron 1997). The

prosecutor must file periodic reports with the court, typically every 10 days, describing

continuing need for surveillance and any progress toward the investigation's goals (18

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[5] 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.

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.[6]

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 ten years studied in this paper, judges rejected only 4 of 5,592 wiretap applications, a rejection rate of less than

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[6] 18 U.S.C. §§ 1515, 2518(10)(a); *Katz v. United States*, 389 U.S. 347 (1967).

one tenth of one percentage point (U.S. Administrative Office various years).[7] 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, and 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 the now-classic model of prosecutorial behavior due to Landes (1971),

prosecutors maximize expected sentences less the cost of the prosecutor's time. Glaeser

et al. (2000) specified a model in which prosecutors' objective functions included both

crime control and career-enhancing human capital. Cases offering the greatest

opportunities for career advancement are not necessarily those with the lowest probability

of success, and 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

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[7] 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.

U.S. Attorneys' Offices apply to wiretap applications, Richman (2003) states that

"prosecutors have an interest in building a 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). But this tendency

may not exist in determining probable cause. In experiments with state court judges,

Wistrich et al. (2005) found that 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 to 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 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 1,240 judges, 541 (or 43.6%) had senior status at some point during the observation period. Another 528 (or 42.6%) did not serve on the bench for the entire ten years of the observation period; 370 joined the bench after 1997; 154 left it (through either death or retirement) before 2006; and 4 both arrived after 1997 and left before 2006. The main analysis of the paper uses an unbalanced panel of judge-by-year observations in which judges appear in the panel in each of the years in which they served on the bench. A concern with this approach is that the observed impact of race may be due to changes in the composition of judges in the data. For that reason, the main results were re-estimated with the data restricted to the set of 712 judges who served on the bench for the entire ten-year period, and as shown below, the estimates were similar.

Table 1 presents mean differences between white and African-American judges in the data. Of the 1,240 judges, 111 (or 8.95%) are African-American, and in the panel of judge-by-year observations, African-American judges account for 9.4% of the observations. Table 1 shows that African-American and white 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 white judges to be Democratic appointees. Democratic presidents appointed over 70% of the African-American judges in the data and only 40% of white judges.

A 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.[8] By this measure, African-American judges are significantly more liberal on average than white judges. The gap shrinks considerably when conditioning on party of the appointing president; from a difference of 26 to about 5 points on this -1 to +1 scale. Most of the variation in the ideology scores thus 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 district. 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 were less often the most conservative judge within their districts and more often the most liberal 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 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

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[8] 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.

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.[9] 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 ten years studied here, should be roughly the same across judges. Any differences in these outcomes across judges should therefore reflect their attitudes on the criminal justice system. Neither of these variables differs across the two racial groups. The average sentences of white judges are nearly two months longer than those of African-American judges. But this difference is not statistically significant, and it is small relative to the average sentence length of more than sixty months. The incarceration rates differ by less than a percentage point. 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 slightly more likely to be female. They were on average five years younger, and their average service on the federal bench was 2.5 years shorter than their white colleagues. Consistent with their shorter tenure on the federal bench, African-American judges spent less of their time during the observation period on senior status, a

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[9] 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).

form of semi-retirement in which federal judges may reduce their caseload. [10] Their

shorter average tenure is also reflected is 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.[11]

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 more than 60% of them had served as

prosecutors. In addition, African-American judges who had been prosecutors tended to

have nearly an additional year of prosecutorial experience than their white colleagues. In

contrast, experience as either a public or federal criminal defender was rare. Only 5% of

judges had performed this type of work. But the incidence of it was higher among

African-American judges; nearly 10% 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. Just under 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. The incidence of other types governmental experience is not reported

in the table because there were no meaningful differences across the racial groups in

these experiences. About 18% of judges previously worked in the legislative or

executive branches of state government, and with respect to the federal government,

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[10] 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.
[11] 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.

about 14% previously worked in the legislative branch or in non-prosecutorial positions with the executive branches.

No differences are apparent in the types of educational institutions attended. Fewer than 20% of the judges attended an elite law school,[12] and slightly more than 10% attended Ivy League colleges. Differences across the racial groups in these educational categories were not significant. Service as a law clerk to a judge is commonly considered an important form of legal training, and the selection process for many clerkships is highly competitive (Avery et al. 2001, 2007). About 22% of all the judges the data previously served as law clerks, but this figure was about 5 percentage points lower for African-American judges.

Finally, African-American and white 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 21 judges while the average white judge served on a court that had only 17 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 white judges to serve in judicial districts in the northeast (36.0% versus 27.2%) and less likely to serve in districts in the south (26.1% versus 34.7%). But African-American and white judges served at equal rates in districts in the mid-west and west. These patterns implied no difference in the procedural rules governing wiretap applications. White and African-American judges were about equally likely to serve in a district with a local rule requiring the chief judge to review all wiretap applications.

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[12] 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

$$Y_{ijt} = Z_{ij}\delta + X_{ijt}\beta + \alpha_j + \alpha_t + \alpha_n + \epsilon_{ijt}, \tag{1}$$

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. The denominator corrects for judge's partial years of judicial service; it takes a value fewer than 365 only in years when a judge joins or leaves the bench.

The term Z_{ij} is a vector of 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.[13] 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. Rather than control for the number of judges in district i and year t with a continuous variable, the baseline equation also includes a fixed effect for the number of judges sitting on the district court in a given year, α_n. Standard errors are clustered at the level of judicial districts because they led to more conservative inferences than clustering at the level of the judge or year did.

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

Table 2 presents the first set of regression results. In column (1), the judge's race is the only explanatory variable, and regressions in columns (2) through (5) add progressively more fixed effects, specifically fixed effects for years, judicial districts, the number of judges in the district, and year-district interactions. District fixed effects are potentially quite important given the wide variation in the frequency of wiretap applications across districts (Minzner and Anderson 2005). Fixed effects for the number of judges in the district are included because the opportunity to forum-shop across judges may rise with the number of judges in a district. The last five columns of the table repeat the exercise while including additional explanatory variables for other demographic and experiential characteristics of the judges.

The estimated impact of a judge's race in the first five regressions is large. The point estimates of -.032 and -.039 represent a movement of about 20% in the mean value of the dependent variable (.173; standard deviation = .467). In other words, the average African-American judge receives about 20% fewer wiretap applications per day than her white colleagues. When the regressions include additional covariates, as in columns (6) through (10), the estimate becomes larger in magnitude: -.065. It implies a 38% difference in the number of wiretap applications per day.

The first five columns show that the estimate is not sensitive to the progressive introduction of more fixed effects. The presence of fixed effects for judicial districts slightly lowers the estimate from -.032 to -.039, and the other fixed effects, including year-district interactions, have virtually no impact on the estimate. When the regressions include other covariates, the estimated impacts are also robust to the inclusion of the various fixed effects.

The movement from -.032 to -.065 is due almost entirely to the presence of one other variable: the percentage of time a judge spent on senior status. When senior status is the only other explanatory variable included, the coefficient on a judge's race jumps to -.057 (s.e.=.016). 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 importance of senior status may reflect the preferences of prosecutors who may prefer to present their wiretap applications to active judges. More likely is that senior status judges are less often available to receive wiretap applications. Senior judges may take a reduced workload, including the review of wiretap applications. They may simply spend fewer days in the office and not be present when wiretap applications need reviewing.

In addition to race and senior status, another variable implying a large impact on the number of wiretap applications received is whether the judge currently serves a 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 are about .5, and this value implies that in years in which a judge serves as chief, she receives about 1.8 wiretap applications on average. This result is consistent with the pattern observed in summary statistics; in districts with this "chief judge rule," chiefs receive 1.96 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.

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Other aspects of service as chief judge bear little relationship to the number of wiretap applications received. The regressions in column (6) and (7) show that judges who eventually serve as the chief judges of their districts receive fewer applicants during the years in which they are not the chief. But once the regressions include district fixed effects, as in columns (8) through (10), 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 hovers between -.02 and -.03, except in the specification in column (10), where it is very close to zero. In none of the regressions does it approach standard levels of statistical significance.

The specifications in columns (6) and (7) 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 fewer wiretap applications, about .5 fewer applications per year than in districts without this rule. But there is no variation over time in this rule,

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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 sign implies that more liberal judicial attitudes correlate with receipt of more wiretap applications. The equations in columns (6) and (7) show that judges with prior experience as a prosecutor 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 reveal 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. The estimates imply that a judge who previously was a public or federal

defender receives .06 fewer wiretaps per year. But this estimate is not statistically different from zero.

In sum, the estimates in Table 2 show that the incidence of wiretap applicants correlates with a relatively small number of judicial characteristics. 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 judge 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 white judges.

B. Types of Wiretaps.

Table 3 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 (10) of Table 2. 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 2. It shows that African-American judges receive on average -.063 fewer wiretap applications per day,

relative to an average of .173 wiretap applications received per day for all judges in the data.

The next two rows show that the overwhelming majority of wiretaps, about 84%, 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 -.053 is about a third (in absolute value) of the average value for narcotics investigations (.145). The estimate for investigations not involving narcotics offenses is also about a third of the sample mean. The estimate for narcotics investigations is statistically different from zero at the 2% level, and that for non-narcotics investigations in marginally statistically significant.

The patterns for the location of the wiretap are similar. Wiretaps on portable devices, such as cellular phones, comprise 82% of all wiretaps. Relative to the sample means, the estimated impact of race is roughly the same for wiretaps on both portable and non-portable telephones. The coefficients are each about a third 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 is fairly uniform across the weekdays. With the exception of Thursdays, the fraction of all wiretap warrants sought each day hovers between 15% and 20%. Thursdays experience a small uptick in applications of wiretap warrants; Thursdays account for 25% of all wiretap applications. The most striking feature of the weekly distribution is that applicants on weekends are exceedingly rare. Prosecutors presented fewer than one percent of the wiretap warrants to judges on the weekends.

The estimated coefficients for race imply that African-American judges receive substantially fewer wiretap applications on each weekday. The largest impacts occur on Mondays and Tuesdays, when the estimated effects of race are roughly half the sample means. For Thursdays and Fridays, the estimated impacts are about a third of the sample means, and for Wednesday applications, it dips to about 20%. Except for Wednesday and the weekends, all of the estimates are 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 slightly more wiretap warrants in the spring and summer months (March through August). But judicial race has a sizable and statistically significant impact in each season. According to these results, the influence of race is not limited to particular time of year.

The remaining columns of Table 3 display estimates when the data are limited to the balanced panel of judges who have served for the entire ten-year sample period. These results warrant only brief mention. In column (2), the dependent variable remains the number of wiretap applications received per judge per days served on the bench in each year, and in column (3), it is simply the number of wiretap applications received per judge in each year. These estimates reveal patterns similar to those in column (1), and they confirm that any bias in the race coefficient from use of the unbalanced panel is not substantial.

C. 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 4 assess this possibility by including in the baseline regression a variety control variables for a judge's professional experience. To ease exposition, the table reports only the coefficients for race and ideology and for the additional judicial characteristic. For comparison purposes, the estimates from the baseline regression of column (10) of Table 2 are repeated in the first column of Table 4.

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.

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 -.002 and -.003, respectively, and their standard errors are more than seven times larger. The number of years served as a prosecutor has the largest estimated magnitude, but even this coefficient is well below standard significance levels. 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. For example, interactions of these experience variables with race are also statistically insignificant, and the baseline coefficient on race is robust to their presence. Omitted dimensions of professional experience do not appear to explain the race effect.

D. Race and Ideology.

Some of the largest observed differences between white and African-American judges are in the measures of ideology. The summary statistics in Table 1 show that about 70% of African-American judges are Democratic appointees and only 40% of white 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 5 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 (10) of Table 2. Column (1) of Table 5 repeats these baseline estimates for comparison purposes.

Although political scientists have vigorously debated whether the political party of the appointing president or the common space score is the better measure of judicial ideology (Pinello 1999, Epstein and King 2002, Sisk and Heise 2005), the equation in column (2) shows that this choice matters little in these data. Moreover, the inclusion of both measures of ideology, which captures variation in judicial ideology conditional on partisan affiliation, does not affect the estimated effect of race.

The equations in the next two columns test 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 white 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 white 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 equations in columns (3) and (4) show that this pattern is not present. Under both measures of ideology, the interaction of ideology is small and statistically insignificant. Moreover, the presence of the interaction term has almost no 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.

When prosecutors choose which judge will review the warrant application, they must choose from judges within their 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 ideological variation within district is a ranking

of judges within each district and year of their common space scores. Column (5) shows

that the inclusion of the ranking (from most conservative to least conservative) does not

effect the estimated impact of race or ideology, and the coefficient on the ranking itself is

effectively zero. The equation in column (6) 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. Moreover, its

inclusion makes the estimated effect on race itself more negative. 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.

The measures of ideology employed thus far – party of appointing president and

common space scores – may be too crude in that they encompass views on a variety of

legal issues, such as the Commerce Clause, 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. The section uses 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 warrants, 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 independent of judicial attitudes on criminal justice. Although not shown in order to conserve space, equations in which sentences are measured in natural logarithms produce similarly modestly-sized estimates of sentences and robust estimates of race. On the whole, the estimates of Table 5 indicate that the estimated race effects are not the product of poorly measured judicial ideology.

E. U.S. Attorney Characteristics.

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 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.

However, the wiretap data can be matched to the dates of service and characteristics of the United States Attorneys, who are the head prosecutors in each district. If own-group bias characterizes the behavior of prosecutors, a U.S. Attorney (USA) of the opposite race may mitigate its effect. Moreover, the ability of a USA to control the choices of her line prosecutors may vary with the length of time she has held office. The President appoints and the Senate confirms USAs, and Library of Congress' Thomas Legislative Records file contains the Senate records of their confirmations, including their names and dates of service. The author 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.

A first hypothesis is that a USA's effectiveness as a monitor of line prosecutors' bias varies with the length of service. In this account, the racial gap should be larger when a USA is less experienced or during transitions between USAs. Columns (2) through (4) 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. But tenure does not correlate the number of wiretap

applications a judge receives, nor does the racial gap shrink meaningfully as a USA's

tenure rises. The coefficient on the interaction of a judge's race and tenure implies that

an African-American judge receives .048 more wiretap applications per year when the

USA in the district has average tenure (3.3 years).[14] But this gain is less than a fifth of

the racial gap of -.274.[15] The regressions in columns (4) and (5) relax the assumption that

the effect of an USA's tenure is linear, and test for whether the racial gap is worse during

transitions in the leadership of prosecutors' offices. The coefficients on indicator

variables for the first and final years of an USA's service are close to zero and

statistically insignificant. The coefficients on their interaction with a judge's race imply

that the racial gap is smaller but does not fully close during an USA's first year in office,

and it is larger during her final year in office. But neither of these estimates is

statistically significant. None of the tenure variables 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.

Another possibility is that the racial gap may arise solely or most severely when

USAs belong 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.

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[14] Where .048 = (.004 * 3.3) * 365.25 / 100.
[15] Where .274 = .075 * 365.25 / 100.

Aware of these judicial proclivities, prosecutors may be sensitive to the racial and ideological composition of the bench. The observation period, 1997-2006, 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. But the estimates in columns (6) and (7) test the impact of an USA's partisan affiliation directly. The estimates are small and statistically insignificant. If taken at face value, the point estimates imply that USAs appointed by Democratic presidents use wiretaps slightly less often than Republicans. But the racial gap in wiretap warrant applications persists under both Democratic and Republican USAs.

The equations in the next two columns test a variant 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 2006 was small. The author's research identified twenty-two of the 254 U.S. Attorneys 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 (9), the interaction of the judge's race and the USA's race is positive at .034 but statistically insignificant. If the point estimates are taken at face value, they suggest that when the USA is African-American rather than white, line

prosecutors still ask African-American judges to approve wiretap warrants less often than they do white judges – but the size of the racial gap is smaller (a difference of -.022 = -.067 + .011 + .034). Still, none of these estimates are statistically significant, and the coefficient on the judge's race remains unchanged and highly significant in the presence of these other controls.

The regression in final column of Table 6 includes an indicator variable for the eleven USAs whose termination by the Bush Administration was controversial. Political motivations rather than job performance were allegedly the basis of the firings.[16] The statistically significant estimate on this indicator variable implies that a reduced use of wiretaps marked the tenure of these USAs. But the presence of this control has no impact on the magnitude or precision of the coefficient on a judge's race. Fewer than 1% of the observations in the data involved African-American judges serving in the districts and years when these USAs held office, and therefore, it is not possible to obtain reliable estimates of whether the racial gap in wiretap warrants was larger or smaller during their tenure. Also, the data end in 2006, the year in which the Department of Justice terminated most of these 11 USAs, and therefore, it is not possible to determine whether the racial gap in wiretap applications changed during the stewardship of their replacements. But, the estimates indicate that the racial gap in wiretap applications is not an artifact of this controversy.

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[16] The USAs (and their judicial districts) were David Iglesias (New Mexico), Kevin V. Ryan (Northern California), John McKay (Western Washington), Paul K. Charlton (Arizona), Carol Lam (Southern California), Daniel Bogden (Nevada), Margaret Chiara (Western Michigan), Todd Graves (Western Missouri), Bud Cummins (Eastern Arkansas), Thomas M. DiBagio (Maryland), and Kasey Warner (Southern West Virginia).

F. Characteristics of the Defendant Pool.

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 applications more closely than when they belong to different races. Knowing this, a prosecutor may bring 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. These sentencing data are available only through 2005, and therefore, this analysis excludes the last year of wiretap data.

Table 7 displays the results of regressions that control for this proxy for the race of the wiretap target. Column (1) shows that the exclusion of 2006 makes only a small difference in the baseline estimates. The regression in the next column includes the percentage of sentenced defendants in each district and year who are African-American. 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), the estimates imply that districts and years with more African-American 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 close to zero. The estimates for this variable and its interactions are statistically insignificant.

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Most criminal defendants in federal courts are male, and the equations in columns (5) through (7) repeat the exercise using the percentage of sentenced defendants who were African-American males. The results are similar. The percentage of African-American male defendants correlates weakly with a more frequent use of wiretaps. But this variable does not meaningfully interact with a judge's race or ideology. In all the regressions in Table 7, the effect of a judge's race remains negative, sizable, and statistically significant while the impact of ideology is close to zero and statistically insignificant. Own-group bias does 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 judge-shopping rather than prosecutorial bias 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 selecting judges favorably disposed to the government's use of wiretaps and avoiding judges who view wiretap applications more critically. Prosecutors should

bring stronger wiretap applications to disfavored judges and weaker one to favored

judges. This account predicts that the average outcomes of wiretaps approved by

African-American judges should be higher than those of white judges.[17]

Table 8 tests the prediction by presenting wiretap outcomes at two levels of

analysis. To maintain comparability with the earlier results, Panel A reports regressions

in which the units of analysis are again judge-by-year observations. In this analysis,

when a judge received more than one wiretap in a year, the dependent variable is the

average outcomes for these wiretaps. In Panel B, the units of analysis are wiretaps

themselves. The first column of Table 8 reports the mean and standard deviation of each

dependent variable. The subsequent columns numbered (1) through (10) report the

coefficient on the race variable in the same set of regression specifications used in Table

2, where each equation adds progressively more controls.

The first row reports results regarding the number of days a wiretap lasts, and the

second row provides another measure of the duration of the wiretap, the number of

extensions. Title III provides that a court may authorize a wiretap for up to 30 days, and

may extend it for an additional thirty days it upon reapplication (18 U.S.C. § 2510(5)).

The statute does not limit number of times prosecutors may obtain extensions on a

wiretap, but it requires that surveillance terminate as soon as the investigative objective is

met. Investigators define the investigation broadly, such identification of all conspirators,

in order to avoid terminating the wiretap upon interception of the first evidence of

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[17] 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).

criminality (O'Meara 1993).[18] Longer-lasting wiretaps may therefore involve more complex criminal enterprises or offenses.

For the number of days a wiretap actually lasts, the estimated coefficients on race are small, at most 1.4 days relative to a sample mean of 37.6 days. They are also negative indicating that if anything, the wiretap applications brought to African-American judges involve slightly less complex crimes. For the number of extensions, the estimates are positive, but they are well below standard significance levels.

The next four rows present results on the number of intercepts and incriminating intercepts normalized by the number of days of the wiretap's operation.[19] The standard deviations indicate that these outcomes are highly variable, and to verify that the estimates are not driven by outliers, the next two rows examine the incidence of intercepts expressed in natural logarithms. Wiretaps that generated no intercepts 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). The coefficients on race for these outcomes are generally small relative to the sample means and are often inconsistent in sign. In Panel B, the racial gap in the number of incriminating intercepts per day is statistically significant in some specifications. According to the statistical discrimination account, its sign should be positive, but here it is negative. When the regressions include the full set of controls, as shown in column (10), none of the race coefficients are statistically significant.

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[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).

[19] Title III defines an intercept as "the aural or other acquisition of the contents of any wire, electronic, or oral communication though the use of any electronic, mechanical, or other devise." 18 U.S.C. § 2510(4).

The final two rows present estimates from regressions in which the dependent variables are the number of arrests and convictions resulting from the wiretap, again normalized by the wiretap's days of operation. The author's conversations with former federal prosecutors raised doubts about the reliability of these two measures. During the wiretap's operation, prosecutors collect information on the number of intercepts it picks up. The need to include this information in the 10-day reports they must file with the court provides an incentive for prosecutors to assure its accuracy. In contrast, arrests and convictions are outcomes that are realized often 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. With this caveat in mind, the estimates for arrests and convictions show no sharp racial disparities between African-American and white judges. In a few specifications in Panel A, the estimates for arrests per day of the wiretap achieve statistical significance, but none of the corresponding estimates in Panel B do. The estimates for convictions are consistently small and fail to attain statistical significance. On the whole, there is no consistent relationship between judicial race and wiretap outcomes.

5. Discussion and conclusion

A large literature documents that judges' preferences influence their decision-making. But 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 that approval. In this environment, prosecutors have no incentive to shop for favorably disposed judges. The evidence presented in this paper is largely consistent with this view.

With two exceptions, the rates at which judges receive wiretap applications do not vary with their characteristics. One exception is the set of variables that simply affect a judge's availability to receive wiretap warrants, such as the chief judge rule and senior status. Importantly, characteristics that may reflect a judge's attitude toward law enforcement, such as ideology and past experience as a prosecutor, do not correlate with the number of wiretap applications received.

As judicial approval of wiretap applications is certain, this setting provides a test for whether prosecutors harbor racial biases. When prosecutors are not biased, the allocation of wiretap applications across judges in each judicial district should not correlate with a judge's race. But the estimates show a sizable and persistent racial gap in wiretap applications. The gap is robust to numerous proxies for judicial attitudes and institutional explanations such as USA behavior. In addition, if a judge's race were an important dimension of a prosecutor's search for a favorably disposed judge, race should correlate with wiretap outcomes such as intercepts and arrests. The estimates do not support this shopping account; wiretap outcomes are not correlated with the race of the reviewing judge.

In sum, the evidence suggests that racial bias partly influences the prosecutors' choice of reviewing judges.

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Table 1. Summary Statistics

Characteristic:	Judge's Race:			Characteristic:	Judge's Race:		
	White:	African-American:	Difference:		White:	African-American:	Difference:
Ideology (1= most conservative; -1 = most liberal)	.076 (.012)	-.184 (.035)	.260** (.039)	Ever Served as Chief Judge	.346 (.014)	.261 (.042)	.085* (.047)
Democratic Appointee	.402 (.015)	.721 (.043)	-.319** (.014)	Fraction of Years Chief Judge (if ever served as chief judge)	.266 (.015)	.277 (.049)	-.011 (.057)
Ideology (if Democratic Appointee)	-.345 (.008)	-.392 (.014)	.047** (.020)	Prosecutorial Experience	.470 (.015)	.613 (.046)	-.142** (.050)
Most Conservative Judge within District	.223 (.012)	.116 (.025)	.117** (.031)	Years as Prosecutor (if prosecutorial experience)	6.450 (.217)	7.397 (.604)	-.949 (.643)
Least Conservative Judge within District	.137 (.010)	.245 (.039)	-.107** (.034)	Criminal Defense Experience	.042 (.006)	.099 (.028)	-.057** (.021)
Percentage of Defendants Sentenced by Judge Incarcerated	63.071 (.813)	61.273 (2.516)	1.798 (2.710)	Served as State Judge	.365 (.014)	.526 (.048)	-.158** (.048)
Average Length of Sentence Given by Judge (months)	47.500 (.862)	46.571 (2.419)	.929 (2.853)	Years as State Judge (if served as state judge)	11.328 (2.418)	9.345 (.707)	-1.983 (6.455)
Female	.152 (.011)	.234 (.040)	-.082** (.036)	Attended Elite Law School	.152 (.011)	.180 (.037)	-.028 (.036)
Age	65.016 (.359)	60.608 (1.039)	4.407** (1.189)	Attended Ivy League College	.114 (.009)	.117 (.031)	.003 (.032)
Years on the Bench	14.863 (.314)	12.365 (.832)	-2.498** (1.034)	Served as Law Clerk to Another Judge	.234 (.013)	.153 (.034)	-.081* (.042)
Years as Lawyer	37.980 (.366)	33.050 (.959)	4.931** (1.207)	Chief Judge Rule	.183 (.012)	.126 (.032)	.057 (.038)
Fraction of Time on Senior Status	.353 (.013)	.238 (.039)	.115** (.044)	Number of Judges in District	17.387 (.345)	20.901 (1.129)	-3.519** (1.154)

Note: Except where otherwise specified, the total number of observations is 1,240 with 1,129 white judges and 111 African-American judges.

Table 2. Incidence of Wiretap Applications and Judicial Characteristics

Explanatory Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
African-American	-.032** (.016)	-.032** (.016)	-.039** (.016)	-.039** (.016)	-.039** (.016)	-.065** (.016)	-.065** (.016)	-.063** (.017)	-.062** (.017)	-.063** (.017)
Ideology						-.009 (.024)	-.009 (.024)	-.005 (.025)	-.003 (.024)	-.002 (.025)
Prosecutorial Experience						.045* (.024)	.045* (.024)	.023 (.021)	.023 (.021)	.023 (.021)
Criminal Defense Experience						-.016 (.029)	-.016 (.029)	-.014 (.028)	-.014 (.027)	-.012 (.027)
Female						.045 (.029)	.045 (.029)	.016 (.029)	.017 (.030)	.018 (.030)
Age (in years)						-.0002 (.0012)	-.0003 (.0013)	-.0004 (.0014)	-.0004 (.0015)	-.0004 (.0015)
Fraction of Year on Senior Status						-.135** (.034)	-.135** (.034)	-.135** (.037)	-.138** (.039)	-.138** (.040)
Ever Served as Chief Judge						-.040** (.015)	-.040** (.015)	.019 (.025)	.018 (.026)	.018 (.027)
Fraction of Year Spent as Chief Judge						-.020 (.026)	-.020 (.026)	-.027 (.028)	-.028 (.028)	-.003 (.003)
Chief Judge Rule						-.138** (.016)	-.138** (.016)	--	--	--
Chief Judge Rule * Fraction of Year Spent as Chief Judge						.490* (.261)	.491* (.261)	.544* (.311)	.546* (.312)	.548* (.315)
Fixed Effects:										
Year		Y	Y	Y	Y		Y	Y	Y	Y
District			Y	Y	Y			Y	Y	Y
Number of Judges			Y	Y					Y	Y
Year * District				Y	Y					Y
R-square	.0004	.0040	.0635	.0686	.0776	.0594	.0629	.1161	.1214	.1303

Note – N=9,798. The dependent variable in each OLS regression is the total number of wiretap applications received by a judge per days the judge served on the bench in each year. An asterisk * denotes coefficients statistically significant at the 10% level, and double asterisks ** denote coefficients statistically significant at the 5% level.

Table 3. Estimates of Impact of Judicial Race: Types and Timing of Wiretap Applications

Dependent Variable:	Baseline Estimate of Taps / Days (1)		Balanced Panel Estimate of Taps per Days (2)		Balanced Panel Estimate of Taps (3)	
	Mean [S.D.] of Dependent Variable (1a)	Race Coefficient (standard error) (1b)	Mean [S.D.] of Dependent Variable (2a)	Race Coefficient (standard error) (2b)	Mean [S.D.] of Dependent Variable (3a)	Race Coefficient (standard error) (3b)
Total Applications	.173 [.467]	-.063** (.017)	.174 [.476]	-.065** (.026)	.635 [1.731]	-.235** (.093)
Offense Investigated:						
Total Narcotics Applications	.145 [.417]	-.053** (.016)	.148 [.431]	-.056** (.025)	.539 [1.569]	-.202** (.089)
Total Non-narcotics Applications	.027 [.148]	-.009* (.005)	.026 [.121]	-.009* (.005)	.095 [.439]	-.033* (.019)
Location of Wiretap:						
Total Wiretaps on Portable Devices	.142 [.408]	-.051** (.015)	.142 [.422]	-.050** (.023)	.519 [1.541]	-.182** (.083)
Total Wiretaps on Non-portable Devices	.031 [.152]	-.011** (.005)	.032 [.127]	-.016** (.005)	.117 [.462]	-.057** (.017)
Day of Week of Application:						
Total Applications on Monday	.030 [.117]	-.016** (.004)	.030 [.118]	-.017** (.005)	.110 [.429]	-.062** (.020)
Total Applications on Tuesday	.033 [.150]	-.015** (.005)	.032 [.124]	-.012** (.006)	.117 [.451]	-.045** (.023)
Total Applications on Wednesday	.037 [.138]	-.008 (.005)	.037 [.137]	-.007 (.007)	.134 [.499]	-.024 (.026)
Total Applications on Thursday	.044 [.159]	-.014** (.007)	.046 [.163]	-.021** (.007)	.167 [.594]	-.074** (.027)
Total Applications on Friday	.028 [.114]	-.010** (.003)	.029 [.111]	-.008* (.005)	.104 [.405]	-.029* (.018)
Total Applications on Weekend	.001 [.026]	-.001 (.001)	.001 [.017]	-.000 (.001)	.003 [.061]	.000 (.000)
Season of Application:						
Total Applications in Winter	.036 [.147]	-.014** (.006)	.036 [.136]	-.013** (.006)	.131 [.495]	-.046** (.024)
Total Applications in Spring	.052 [.195]	-.022** (.087)	.055 [.207]	-.015 (.011)	.200 [.747]	-.054 (.040)
Total Applications in Summer	.047 [.196]	-.014** (.007)	.046 [.177]	-.021** (.008)	.166 [.647]	-.077** (.030)
Total Applications in Autumn	.039 [.157]	-.012** (.005)	.038 [.149]	-.016** (.007)	.138 [.542]	-.057** (.025)

Note: 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 (10) of Table 2. Coefficients on other variables are not reported in order to conserve space. In the estimates in columns (1), N = 9,798, and in those of columns (2) and (3), N=7,040 because they include only the judges who appear in the data for the entire observation period, 1997-2006. An * denotes coefficients statistically significant at the 10% level, and ** denotes significance at the 5% level.

Table 4. Alternative Specifications of Judicial Characteristics

Explanatory Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
African-American	-.063**	-.064**	-.063**	-.064**	-.063**	-.064**	-.066**	-.063**	-.063**	-.066**
	(.017)	(.017)	(.017)	(.018)	(.017)	(.018)	(.018)	(.017)	(.017)	(.019)
Ideology	-.002	-.002	-.002	-.002	-.002	-.002	-.002	-.002	-.002	-.002
	(.025)	(.025)	(.026)	(.025)	(.025)	(.026)	(.025)	(.025)	(.025)	(.025)
Years on the Bench		.003								
		(.002)								
Most Senior Active Judge within District			.0003							
			(.053)							
Least Senior Active Judge within District			-.084**							
			(.031)							
Served as State Judge				-.002	-.002					
				(.015)	(.015)					
Years as State Judge (if served as state judge)					-.0000					
					(.0001)					
Years as Lawyer						-.002				
						(.004)				
Served in State Legislative or Executive Branch (non-prosecutorial)							.040			
							(.027)			
Served in Federal Legislative or Executive Branch (non-prosecutorial)							-.007			
							(.020)			
Years as Prosecutor (if served as prosecutor)								.018		
								(.020)		
Attended Elite Law School									.016	
									(.029)	
Attended Ivy League College									-.003	
									(.022)	
Served as Law Clerk										-.018
										(.021)
R-square	.1303	.1311	.1325	.1303	.1303	.1304	.1313	.1304	.1305	.1305

Note: N = 9,798. The dependent variable is the total number of wiretap applications received by a judge per days the judge served on the bench in each year. Except where specified, the equations include with the same set of covariates as column (10) of Table 2. 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 5. Alternative Specifications of Judicial Ideology and Race

Explanatory Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
African-American	-.063**	-.061**	-.057**	-.059**	-.063**	-.080**	-.071**	-.134**	-.072**	-.116**
	(.017)	(.018)	(.017)	(.027)	(.017)	(.032)	(.018)	(.064)	(.017)	(.043)
Ideology	-.002		-.004		-.002	-.002	.005	.005	.006	.007
	(.025)		(.027)		(.023)	(.024)	(.027)	(.027)	(.028)	(.027)
Democratic Appointee		-.004		-.004						
		(.022)		(.024)						
Ideology * African American			.028							
			(.048)							
Democratic Appointee * African American				-.003						
				(.036)						
Conservative Rank within District					-.0002	-.0008				
					(.0061)	(.0063)				
Conservative Rank within District * African American						.005				
						(.008)				
Percentage of Criminal Cases Resulting in Incarceration							-.0001	-.0002		
							(.0005)	(.0005)		
Percentage of Criminal Cases Resulting in Incarceration * African American								-.0009		
								(.0008)		
Average Sentence Length (Months)									-.0004	-.0005
									(.0004)	(.0004)
Average Sentence Length (Months) * African American										.0008
										(.0007)
R-square	.1303	.1315	.1304	.1316	.1304	.1305	.1316	.1317	.1318	.1319

Note: N = 9,798. The dependent variable is the total number of wiretap applications received by a judge per days the judge served on the bench in each year. Except where specified, the equations include with the same set of covariates as column (10) of Table 2. 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. Impact of the Characteristics of United States Attorneys

Explanatory Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
African-American Judge	-.063**	-.063**	-.075**	-.063**	-.059**	-.063**	-.063**	-.063**	-.067**	-.063**
	(.017)	(.017)	(.025)	(.017)	(.018)	(.017)	(.017)	(.017)	(.019)	(.017)
Judge's Ideology	-.002	-.002	-.001	-.002	-.002	-.002	-.002	-.002	-.002	-.002
	(.025)	(.025)	(.025)	(.025)	(.025)	(.024)	(.025)	(.025)	(.025)	(.029)
U.S. Attorney's Tenure (in years)		-.0002	-.0001							
		(.0029)	(.0028)							
U.S. Attorney's Tenure (in years) * African American Judge			.004							
			(.005)							
U.S. Attorney's First Year				.002	-.001					
				(.016)	(.015)					
U.S. Attorney's Final Year				.011	.016					
				(.018)	(.036)					
U.S. Attorney's First Year * African American Judge					.031					
					(.026)					
U.S. Attorney's Final Year * African American Judge					-.051					
					(.036)					
U.S. Attorney is Democratic Appointee						-.033	-.033			
						(.031)	(.031)			
U.S. Attorney is Democratic Appointee * African-American Judge							.004			
							(.022)			
African-American U.S. Attorney								.014	.011	
								(.025)	(.026)	
African-American U.S. Attorney * African-American Judge									.034	
									(.033)	
U.S. Attorney is Controversial Bush Administration Firing										-.068**
										(.029)
R-square	.1303	.1303	.1304	.1304	.1306	.1305	.1305	.1303	.1304	.1305

Note: N = 9,798. The dependent variable is the total number of wiretap applications received by a judge per days the judge served on the bench in each year. Except where specified, the equations include with the same set of covariates as column (10) of Table 2. 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. Impact of Characteristics of the Defendant Pool

Explanatory Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
African-American Judge	-.072**	-.072**	-.086**	-.082**	-.072**	-.089**	-.087**
	(.021)	(.021)	(.035)	(.035)	(.021)	(.036)	(.040)
Judge's Ideology	-.017	-.017	-.016	-.0004	-.017	-.016	-.006
	(.033)	(.033)	(.034)	(.0498)	(.033)	(.034)	(.051)
Percent of District's Defendants are African-American		.0013	.0012	.0013			
		(.0009)	(.0008)	(.0008)			
(Percent of District's Defendants are African-American) x (African-American Judge)			.0004	.0002			
			(.0008)	(.0010)			
(Percent of District's Defendants are African-American) x (Judge's Ideology)				-.0005			
				(.0011)			
Percent of District's Defendants are African-American Males					.0023	.0023*	.0023*
					(.0013)	(.0012)	(.0012)
(Percent of District's Defendants are African-American Males) x (African-American Judge)						.0005	.0004
						(.0008)	(.0010)
(Percent of District's Defendants are African-American Males) x (Judge's Ideology)							-.0003
							(.0011)
R-square	.1091	.1092	.1092	.1092	.1092	.1093	.1093

Note: N = 8,792. The dependent variable is the total number of wiretap applications received by a judge per days the judge served on the bench in each year. Except where specified, the equations include with the same set of covariates as column (10) of Table 2. 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.A. Judicial Race and Measures of Wiretap Outcomes: Judge-by-Year Observations

Dependent Variable:	Mean [S.D.]	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Days in Operation	37.64 [29.49]	-.622 (1.944)	-.482 (1.885)	-1.126 (1.894)	-1.025 (1.929)	-1.163 (1.929)	-1.233 (1.947)	-1.028 (1.892)	-1.353 (1.889)	-1.224 (1.916)	-1.385 (1.922)
Number of Extensions	.108 [.552]	.061 (.090)	.071 (.092)	.070 (.098)	.073 (.097)	.079 (.101)	.058 (.096)	.066 (.096)	.072 (.104)	.076 (.103)	.080 (.106)
Total Intercepts per Days in Operation	18.36 [40.64]	-2.377 (2.061)	-.804 (1.504)	-1.502 (1.831)	-1.915 (1.844)	-.482 (1.762)	-1.512 (2.126)	.611 (1.474)	-.152 (1.640)	-.626 (1.681)	.801 (1.685)
Incriminating Intercepts per Days in Operation	.648 [5.812]	-.132 (.171)	-.148 (.173)	-.367 (.318)	-.388 (.315)	.158 (.300)	-.120 (.159)	-.133 (.173)	-.382 (.343)	-.408 (.343)	.154 (.308)
Log Total Intercepts per Days in Operation	1.251 [1.799]	-.071 (.099)	.040 (.051)	.038 (.053)	.026 (.053)	.034 (.051)	-.094 (.117)	.056 (.053)	.051 (.053)	.040 (.058)	.046 (.060)
Log Incriminating Intercepts per Days in Operation	.094 [.517]	.025 (.025)	.023 (.025)	.012 (.025)	.009 (.025)	.014 (.027)	.029 (.026)	.027 (.027)	.016 (.027)	.013 (.026)	.016 (.028)
Arrests per Days in Operation	.033 [.131]	.008 (.010)	.010 (.010)	.019** (.009)	.019** (.010)	.017** (.009)	.009 (.011)	.012 (.010)	.020** (.010)	.020** (.010)	.018* (.010)
Persons Convicted per Days in Operation	.007 [.049]	.0004 (.005)	.001 (.005)	.003 (.005)	.003 (.005)	.003 (.005)	.0007 (.006)	.002 (.005)	.003 (.005)	.003 (.006)	.003 (.006)
Other Covariates?							Y	Y	Y	Y	Y
Fixed Effects?											
Year			Y	Y	Y	Y		Y	Y	Y	Y
District			Y	Y	Y				Y	Y	Y
Number of Judges				Y	Y					Y	Y
Year * District					Y						Y
R-square											

Note: N=9,798. The first column reports the mean and standard deviation of the dependent variable, and subsequent columns, labeled (1)-(10), report the coefficient on a judge's race in regressions with the same set of covariates as column (10) of Table 2. 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.

Table 8.B. Judicial Race and Measures of Wiretap Outcomes: Wiretap-level Observations

Dependent Variable:	Mean (S.D.)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Days in Operation	44.54 [36.68]	-1.695 (1.724)	-1.237 (1.667)	-1.391 (1.735)	-1.171 (1.551)	-1.128 (1.589)	-1.590 (1.987)	-1.408 (1.947)	-.846 (1.906)	-.837 (1.718)	-.936 (1.712)
Number of Extensions	.167 [1.006]	.052 (.126)	.101 (.130)	.101 (.135)	.112 (.140)	.122 (.143)	.079 (.129)	.102 (.135)	.109 (.141)	.121 (.145)	.129 (.149)
Total Intercepts per Days in Operation	16.740 [38.66]	-2.953** (1.704)	-1.281 (1.227)	-1.559 (1.223)	-1.916 (1.312)	-1.811 (1.365)	-.007 (1.682)	-.288 (1.155)	-.577 (1.168)	-.945 (1.229)	-.964 (1.314)
Incriminating Intercepts per Days in Operation	.719 [5.501]	-.358** (.113)	-.243** (.127)	-.385** (.167)	-.381** (.152)	-.258 (.187)	-.261** (.109)	-.283** (.122)	-.406** (.156)	-.381** (.145)	-.258 (.174)
Log Total Intercepts per Days in Operation	1.162 [1.743]	-.149 (.102)	-.028 (.041)	-.024 (.040)	-.030 (.041)	-.044 (.047)	.121 (.114)	.011 (.031)	.051 (.034)	.038 (.033)	.026 (.038)
Log Incriminating Intercepts per Days in Operation	.110 [.534]	-.031** (.014)	-.014 (.014)	-.025 (.016)	-.028 (.016)	-.040** (.017)	-.005 (.023)	-.020 (.013)	-.007 (.018)	-.010 (.016)	-.012 (.014)
Arrests per Days in Operation	.031 [.186]	.001 (.007)	.003 (.006)	.009 (.006)	.006 (.007)	.005 (.007)	.003 (.007)	.003 (.006)	.009 (.006)	.006 (.005)	.005 (.007)
Persons Convicted per Days in Operation	.007 [.067]	-.003 (.002)	-.001 (.002)	-.001 (.002)	-.002 (.003)	-.002 (.003)	-.002 (.003)	-.001 (.003)	-.001 (.003)	-.002 (.003)	-.002 (.004)
Other Covariates?							Y	Y	Y	Y	Y
Fixed Effects?											
Year			Y	Y	Y	Y		Y	Y	Y	Y
Month			Y	Y	Y	Y		Y	Y	Y	Y
Day of Week			Y	Y	Y	Y		Y	Y	Y	Y
District				Y	Y	Y			Y	Y	Y
Number of Judges					Y	Y				Y	Y
Year * District						Y					Y
R-square											

Note: The first column reports the mean and standard deviation of the dependent variable, and subsequent columns, labeled (1)-(10), report the coefficient on a judge's race in regressions with the same set of covariates as column (10) of Table 2. 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.