

## Detecting Bias: Using Statistical Evidence to Establish Intentional Discrimination in Racial Profiling Cases

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In ordering an investigation into possible racial profiling, President Clinton condemned the practice as “the opposite of good police work where actions are based on hard facts, not stereotypes.”<sup>1</sup> But precisely what police work should be considered lawful because based on “hard facts,” as opposed to unlawful because based on “stereotypes?” Some participants in the public debate argue that any racial disparity in law enforcement should be impermissible, while others are willing to tolerate some disparity as long as it does not stem from racial bias and it improves the effectiveness of policing.<sup>2</sup> From a legal viewpoint, ascertaining the extent to which an observed disparity reflects bias is a key requirement in racial profiling litigation. In this paper we report on recent contributions in the field of economics that make progress in this direction.

The economic approach is based on a rational choice model of policing and crime in which, under certain assumptions on the incentives of agents, the existence of bias can be made empirically testable in a precise fashion. While the model is somewhat

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<sup>1</sup> Steven A. Holmes, *Clinton Orders Investigation on Possible Racial Profiling*, NY Times A22 (June 10, 1999).

<sup>2</sup> Randall Kennedy, *Suspect Policy: Racial Profiling Isn't Usually Racist. It Can Help Stop Crime. And It Should Be Abolished*, New Republic 30-34 (Sept 13, 1999) (highlighting the differences between those who believe any use of racial characteristics should be impermissible, and those who believe that using those characteristics “is an essential weapon in the war on crime”).

complicated and technical, the underlying intuition is not. If two groups are differently disposed to carry drugs, and police are equally likely to search members of both groups, then searches of the group with the greater criminal disposition (Group A) will be more likely to yield contraband than searches of the group with the lesser criminal disposition (Group B). In this case, we say that the success rate, or hit rate, is higher in Group A than in Group B.<sup>3</sup> The police therefore will invest more resources in the higher probability searches of Group A, thus deterring additional criminal activity among Group A members but failing to deter Group B. This process will cause criminal activity in Group A to fall until the hit rates for both groups are equal unless the police are biased. If the police are biased they will search even further the group against which they are biased and create a disparity in the hit rates. Thus, the proper way to determine the existence of bias in this setup is to see if the hit rates are statistically different.

This paper provides an overview of the rational choice model and describes an empirical test for bias that is consistent with the approach taken in *Anderson v Cornejo*,<sup>4</sup> a recent Seventh Circuit opinion. We will proceed as follows: First, we will briefly explain the issues involved in determining the legality of racial disparities in policing, concentrating on the Fourteenth Amendment,<sup>5</sup> which requires proof of intentional discrimination.<sup>6</sup> We then discuss the opinion in *Anderson*, which clarifies the requirements for proving intentional discrimination by making the explicit distinction between search and hit rates.<sup>7</sup> Second, we

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<sup>3</sup> Samuel R. Gross and Katherine Y. Barnes, *Road Work: Racial Profiling and Drug Interdiction on the Highway*, 101 Mich L Rev 651, 667 (2002).

<sup>4</sup> 355 F3d 1021 (7th Cir 2004).

<sup>5</sup> We do not focus on the Fourth Amendment. We are not aware of cases in which the Fourth Amendment has been the basis of successful litigation, perhaps because of the decision in *Whren v United States*, 517 US 806, 813 (1996) ("We of course agree with petitioners that the Constitution prohibits selective enforcement of the law based on considerations such as race. But the constitutional basis for objecting to intentionally discriminatory application of laws is the Equal Protection Clause, not the Fourth Amendment."). But see an interesting argument in Albert W. Alschuler, *Racial Profiling and the Constitution*, 2002 U Chi Legal F 163, 268-69, proposing that the Court should hold racial discrimination by the police unreasonable in violation of the Fourth Amendment.

<sup>6</sup> See *Washington v Davis*, 426 US 229, 238 (1976) (noting that the unconstitutionality of a law under the Equal Protection Clause hinges on whether the law reflects a discriminatory purpose, not whether it has a racially disproportionate impact); Alschuler, 2002 U Chi Legal F at 177 (cited in note 5) (noting that, for the Supreme Court, only purposeful discrimination violates the Equal Protection Clause).

<sup>7</sup> See *Anderson*, 355 F3d at 1024-25 (distinguishing between the search rates of black women at an airport and the success rate in finding contraband on them in evaluat-

explain the model we use to determine the existence of bias in police searches. We show why disparities in search rates are not helpful in determining whether there is intentional discrimination. We then demonstrate why disparities in hit rates should establish an inference of discriminatory intent, consistent with *Anderson*. Third, we illustrate our methodology using data from the Maryland I-95 Corridor, which have been analyzed both by John Knowles, Nicola Persico, and Petra Todd (“KPT”)<sup>8</sup> and by Samuel Gross and Katherine Barnes (“Gross and Barnes (2002)”)<sup>9</sup>, and discuss whether the data support the inference of intentional discrimination.

### I. RACIAL DISPARITIES IN POLICING AND EQUAL PROTECTION

In this Part, we will briefly explain the equal protection issues relevant to the model to be discussed in Part II. As such, we are only concerned with how statistical evidence can be used to establish the existence of racial discrimination where the relevant laws and policies are facially neutral.<sup>10</sup> This paper does not deal with the strict scrutiny analysis that might be triggered if racial discrimination can be established.

In equal protection cases, disparate treatment (also referred to as intentional discrimination), and not merely disparate impact, must be proven.<sup>11</sup> In Part I, we use our approach to propose

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ing a racial profiling claim).

<sup>8</sup> John Knowles, Nicola Persico, and Petra Todd, *Racial Bias in Motor Vehicle Searches: Theory and Evidence*, 109 J Polit Econ 203 (2001) (analyzing the I-95 data for evidence of racial profiling).

<sup>9</sup> See Gross and Barnes, 101 Mich L Rev at 658-60 (cited in note 3) (also analyzing the I-95 data for evidence of racial profiling).

<sup>10</sup> Thus, we assume that there is no law in the jurisdiction (typically a state) expressly condoning a policy of racial profiling, nor does the law enforcement agency have such an express policy. Establishing discrimination facially would obviate the need for this analysis.

<sup>11</sup> An intent to discriminate must be proven in racial profiling and, more generally, in selective prosecution cases. See, for example, *United States v Armstrong*, 517 US 456, 465 (1996) (noting this fundamental rule in a selective prosecution case); *Chavez v Illinois State Police*, 251 F3d 612, 635-36 (7th Cir 2001) (noting that an intent to discriminate must be proven in a racial profiling case); *Bradley v United States*, 164 F Supp 2d 437, 445 (D NJ 2001), *affd*, 299 F3d 197 (3d Cir 2002) (holding that a plaintiff must show “that the difference in treatment was due to Plaintiff’s race”). Consider *Davis*, 426 US at 239 (“[O]ur cases have not embraced the proposition that a law or other official act, without regard to whether it reflects a racially discriminatory purpose, is unconstitutional *solely* because it has a racially disproportionate impact.”) (emphasis in original); Alschuler, 2002 U Chi Legal F at 177 (cited in note 5) (“Under the Supreme Court’s decisions, only purposeful discrimination violates the Equal Protection Clause.”). While these cases typically involve both Equal Protection Clause and Title VI claims, we will not distinguish between the two since the standards required to prove discrimination under each are

a method of distinguishing between disparate treatment and disparate impact. We will then analyze a recent Seventh Circuit case that endorses an earlier version of our model,<sup>12</sup> and suggest that courts should use the Seventh Circuit's approach when deciding racial profiling cases.

A. "Disparate Impact" is to "Disparate Treatment" as Search Rates are to Hit Rates

Disparate impact and disparate treatment are logically and legally distinct.<sup>13</sup> To distinguish disparate impact from disparate treatment in policing cases, we use the search rate for a group, which we define as the number of people in the group searched divided by the total number of people in the group, and the hit rate for that group, which we define as the number of successful searches in that group divided by the number of all searches in that group.

Consider the following simple thought experiment. Assume that there are two groups of equal size in the population, Milanese and Sicilians. We say that the Milanese are disparately *impacted* by the actions of the law enforcement agency whenever their search rate is greater than that of the Sicilians', which simply means that the police are searching a greater proportion of

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equivalent. See US Const Amend XIV, § 1 ("[N]or shall any State . . . deny to any person within its jurisdiction the equal protection of the laws."); US Const Amend V ("[N]or shall any person . . . be deprived of life, liberty, or property, without due process of law."); 42 USC §§ 2000(d)—(d)(7) (2000) (prohibiting recipients of federal funding from discriminating on the basis of race). See also *Adarand Constructors, Inc v Pena*, 515 US 200, 213-18, 224 (1995) (detailing the history of the Fifth Amendment equal protection jurisprudence and requiring congruence between the analysis under the Fifth and Fourteenth Amendments); *Alexander v Choate*, 469 US 287, 293 (1985) ("Title VI itself directly reached only instances of intentional discrimination."); *Guardians Association v Civil Service Commission*, 463 US 582, 610, 612, 642 (1983) (holding that a Title VI claim must include proof of discriminatory purpose); *Rodriguez v California Highway Patrol*, 89 F Supp 2d 1131, 1138-39 (N D Cal 2000) (addressing a Title VI claim as part of a racial profiling case); *Chavez v Illinois State Police*, 1999 US Dist LEXIS 11976 (N D Ill Aug 2, 1999), affd, 251 F3d 612 (7th Cir 2001) (same).

<sup>12</sup> See *Anderson*, 355 F3d at 1025, citing Jeff Dominitz, *How Do the Laws of Probability Constrain Legislative and Judicial Efforts to Stop Racial Profiling?*, 5 Am L & Econ Rev 412 (2003) (extending the analysis in Knowles, Persico and Todd, *Racial Profiling in Motor-Vehicle Searches*, 109 J Polit Econ at 227-28 (cited in note 8), and finding that differences in search rates do not necessarily indicate disparate treatment).

<sup>13</sup> The potential breadth of disparate impact not indicating purposeful discrimination was elucidated by Justice White in 1976 in *Davis*, 426 US at 248 ("A rule [invalidating] a statute . . . if in practice it benefits or burdens one race more than another would . . . perhaps invalidate, a whole range of tax, welfare, public service, regulatory, and licensing statutes that may be more burdensome to the . . . average black than to the more affluent white.").

Milanese than Sicilians. More generally, a racial group can show disparate impact if its members are searched more frequently than members of another race. In contrast, the proper measure of disparate *treatment*, we assert, is a difference in *hit* rates, not in *search* rates. A racial group may be able to demonstrate disparate treatment if their hit rates are significantly lower than that of other groups. For example, if the search rates for Milanese and Sicilians were 50 percent each, but searches of Milanese were successful 70 percent of the time, while those of Sicilians were successful only 30 percent of the time, we ought to conclude that the Sicilians are receiving disparate and unfavorable treatment.

Thus, we say that police search practices disparately and unfavorably *impact* a group when that group's search rate is greater than the search rate for other groups. Police search practices exhibit disparate and unfavorable *treatment* of a group when that group's hit rate is lower than the hit rate for other groups. In the next Subpart we show that our use of these terms corresponds to their use in a recent judicial opinion.

#### B. *Anderson v Cornejo*

In *Anderson*, a group of African-American women challenged the constitutionality of customs searches performed on them, pointing to a General Accounting Office ("GAO") study that African-American women were by far the most likely racial group to be searched at O'Hare Airport in Chicago.<sup>14</sup> The study showed that x-ray search rates for black women were almost twelve times those of white men and more than eight times those of white women.<sup>15</sup> Judge Easterbrook held that "[t]hese and similar data from the GAO's report do not support any constitutional claim . . . for at least four reasons."<sup>16</sup> The most relevant to our model, and to future cases, is the fourth objection,<sup>17</sup> made on the grounds that "these statistics show disparate impact, not dispa-

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<sup>14</sup> 355 F3d at 1023.

<sup>15</sup> United States General Accounting Office, *U.S. Customs Service: Better Targeting of Airline Passengers for Personal Searches Could Produce Better Results*, GAO/GGD-00-38 13 (Mar 2000), available at <<http://www.gao.gov/new.items/gg00038.pdf>> (last visited Apr 13, 2005). We do not pass judgment on the methodology used in this specific study, but rather focus on how the court viewed the importance of search rates and hit rates in determining whether the plaintiffs were intentionally discriminated against.

<sup>16</sup> *Anderson*, 355 F3d at 1023.

<sup>17</sup> The first three objections relate to the quality of the data collected in that specific study, id at 1023-24, and are not of broader relevance.

rate treatment, and the equal protection guarantee is concerned *only* with the latter.”<sup>18</sup>

The study also showed hit rates of 27.6 percent for black women, 25.1 percent for white men, 19.5 percent for white women, 61.6 percent for black men, 58.8 percent for Hispanic men, and 45.7 percent for Hispanic women.<sup>19</sup> Judge Easterbrook noted that these data imply that “black women seem to have been *treated similarly* to both white men and white women.”<sup>20</sup> He held that “the success rate of strip searches . . . show that Customs officials search black women with (on average) the same degree of suspicion that leads them to search white women or white men.”<sup>21</sup>

The court’s analysis is consistent with our model. First, Judge Easterbrook declined to use search rates to infer disparate treatment.<sup>22</sup> Second, he deduced the absence of disparate treatment between different groups from roughly equal hit rates. Indeed, our analysis (detailed below) further suggests that although African-American women received no disparate treatment relative to white men and white women, all three groups received disparate unfavorable treatment relative to African-American men and Hispanic men and Hispanic women.<sup>23</sup>

<sup>18</sup> Id at 1024 (emphasis added) (explaining that *Personnel Administrator of Massachusetts v Feeney*, 442 US 256 (1979) requires a showing of disparate treatment to demonstrate a violation of the equal protection clause).

<sup>19</sup> *Anderson*, 355 F3d at 1023.

<sup>20</sup> Id (emphasis added).

<sup>21</sup> Id at 1024-25. Judge Easterbrook later added that “[i]f about 0.1% of black women returning from foreign travel are smuggling, and the agents select so carefully that 28% of those searched are caught with contraband, where’s the beef?” Id at 1025.

<sup>22</sup> Other courts have also taken the approach that statistical analyses examining search rates establish only disparate impact, and not different treatment. See, for example, *Chavez v Illinois State Police*, 251 F3d 612, 641 (7th Cir 2001) (questioning the sufficiency of statistics that would show that minority “motorists are stopped at a significantly higher rate than are white motorists”); *Hurn v United States*, 221 F Supp 2d 493, 501 (D NJ 2002) (finding that general statistics demonstrating higher search rates for African-American women did not demonstrate specific discriminatory intent in the case at hand).

<sup>23</sup> The court did not find discrimination for two additional reasons. The first is simply that the methodological problems with the statistical evidence were sufficient for Judge Easterbrook to refuse to find discrimination based on it. *Anderson*, 355 F3d at 1023-24. The second, which he discusses briefly, is the inframarginality problem caused by the relative ease with which different groups can be caught. Thus, equal marginal success rates, which are not measured, will lead to different average success rates, which are measured. Id at 1025. Although we will not address the question of inframarginality in this paper, it can be shown that the problem of inframarginality does not arise in the KPT model. See Knowles, Persico, and Todd, 109 J Polit Econ at 212-15 (cited in note 8) (detailing how the KPT model assumes motorists respond to the probability of being searched, thereby testing for prejudice without having data on all the characteristics police use in the decision to search). In any case, even though the court did not find discrimination on this basis, it still noted that “[t]he GAO recommended that the Customs

We propose that *Anderson* reflects the best approach for courts to take when evaluating claims of racial disparities in policing within a policing environment like the one described in Part II.<sup>24</sup> If a court is satisfied as to the reliability and accuracy of the statistical evidence,<sup>25</sup> it can then simply use the hit rates to determine whether law enforcement officers are treating the members of a protected class differently and proceed accordingly. Having outlined the approach the courts should take with respect to the statistical data before it, we will delve into a discussion of the model itself.

## II. MODELING POLICE BIAS

The model is adapted from the rational choice model of policing originally developed by Knowles, Persico, and Todd.<sup>26</sup> The goal of the modeling exercise is to provide a simple test that would detect the presence of a discriminatory intent based on statistical evidence of police behavior. The test must be able to distinguish disparities reflecting discriminatory intent from those that are inevitably generated in the bona fide pursuit of crime.

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Service increase the number of searches in high-success-rate categories.” *Anderson*, 355 F3d at 1025.

<sup>24</sup> We, of course, understand that the Seventh Circuit’s opinion is federal law, and only the law in that circuit. We merely suggest that any court, state or federal, use this method to decide racial profiling issues, assuming that the standard is intentional discrimination and not simply disparate impact.

<sup>25</sup> There is a general recognition that statistical evidence, subject to rebuttal and cross-examination, can be used to support a discrimination claim. See *International Brotherhood of Teamsters v United States*, 431 US 324, 339-40 (1977) (holding that statistical analyses are valid evidence in proving discrimination, but are not irrefutable and can be rebutted like all other evidence). See also *McCleskey v Zant*, 580 F Supp 338, 360-62 (N D Ga 1984) (discussing the specific problems with a statistical model used to prove discrimination while stating that a sufficiently predictive model would have supported an inference of discrimination), *affd en banc as McCleskey v Kemp*, 753 F2d 877 (11th Cir 1985), *affd*, 481 US 279, 291 n 7, 295 n 15 (1987) (assuming the validity of the study used by the district court but questioning its applicability where a large number of semi-autonomous entities are responsible for setting the challenged policy). However, some courts have been resistant to using statistical analyses to prove discriminatory intent. See, for example, *McCleskey*, 481 US at 293 (1987) (“[S]tatistical proof normally must present a ‘stark’ pattern to be accepted as the sole proof of discriminatory intent under the Constitution.”); *Chavez*, 251 F3d at 648 (holding that “statistics may not be the sole proof of a constitutional violation”); *Hurn*, 221 F Supp at 501 (“Statistical data, by itself, can support an inference of discrimination, but must be coupled with additional evidence to permit a finding a discriminatory intent.”). While we recognize that courts may struggle with whether to use statistical evidence as the sole proof of discrimination, we simply maintain that if a court uses statistical analyses to decide a racial profiling issue, it should do so by examining the hit rates rather than the search rates.

<sup>26</sup> Knowles, Persico, and Todd, 109 J Polit Econ at 209-15 (cited in note 8).

## A. A Simple Model of Crime, Policing, and Bias

The model in KPT is quite technical, but the key idea behind it can be expressed simply. Consider a stylized model of narcotics interdiction on highways. To illustrate the argument, let us assume that the propensity to traffic drugs is a function of one's socioeconomic status. Assume that high status people have little incentive to traffic drugs both because they have more lucrative occupations available to them and because the opportunity cost of being punished if detected is higher. Lower status people have worse opportunities and have less to lose if detected. Therefore, it is reasonable to assume that a certain fraction of them will consider trafficking drugs. We assume that the police can distinguish low status people on the basis of their appearance; let's say, for simplicity, that they wear ragged clothes, whereas high status people wear fancy clothes.<sup>27</sup> Let us also assume that low status citizens cannot afford fancy clothes.<sup>28</sup>

Assume that we have two equally-sized groups of motorists which are observably different: Sicilians and Milanese. Let us assume that Sicilians have a lower socioeconomic status on average. This implies that a larger fraction of them will wear ragged clothes and will have a higher propensity to carry drugs compared to the Milanese.

Assume that individual police officers are free to tailor their search strategy as they wish. We assume that police officers are homogeneous, in other words, they share the same preferences. All police officers seek to find drugs, but police with an intent to discriminate are also driven by a desire to harass members of a group, for example, Sicilians. We assume that an unbiased police officer will make decisions about whom to search only in the pursuit of successful searches. Thus, unbiased officers will focus their searches on whichever group presents the highest likelihood of success. A biased police officer, on the other hand, is assumed also to take pleasure in searching Sicilian citizens. This means that the biased officer will favor searching Sicilians even when this group presents a somewhat lower likelihood of success. The extent to which a biased officer is willing to trade a de-

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<sup>27</sup> To keep the analysis streamlined, we simplify the model as much as possible. The same is true at several points in this Part. The model can be made more realistic, at the cost of additional space and complexity.

<sup>28</sup> This assumption is not necessary for our results and could be relaxed, but it simplifies the analysis. We return to this point in Part II-D below.



creased likelihood of success for the pleasure of searching Sicilians represents a measure of the intensity of the officer's bias.

We have modeled intent to discriminate as bias: if officers are biased against Sicilians they will indulge their bias and search Sicilians, unless searching Milanese presents a distinctly higher likelihood of success. In contrast, unbiased officers will search whomever presents the highest probability of success. Thus, we should expect Sicilians to be searched more intensely if the police are biased, or in other words, have an intent to discriminate.

The goal of the analysis is to infer intent to discriminate from statistical data of police behavior. To this end, we need to predict how an unbiased police force would behave, how a biased one would behave, and then use the data to distinguish the two patterns of behavior. The available data are the number of Sicilian and Milanese citizens searched and the outcome of the searches (whether drugs were found). The data, however, does not record the sartorial appearance of those searched.

#### B. Difficulties with Using Search Rate Disparity to Detect Bias

Perhaps our first instinct is to look at the search patterns. After all, we know that, *ceteris paribus*, a biased police officer is more likely to search a Sicilian than a Milanese. So, we might be tempted to infer intent to discriminate by comparing search rates of Sicilians and Milanese. This approach, however, is flawed. Indeed, recall that we assumed that a higher fraction of Sicilians have a propensity to commit a crime. This means that, *ceteris paribus*, criminals will be more frequent among Sicilians than among Milanese. Thus, even if the police have no discriminatory intent and are unable to distinguish between Sicilians and Milanese, but are capable of observing which motorists were criminals, Sicilians will end up being searched more frequently. Such search behavior would entail a disparate impact, but, coming from "ethnic-blind" police, would not be considered evidence of intent to discriminate.

The argument shows that a certain amount of disparate impact might simply reflect the goal-oriented behavior of individual police officers pursuing crime in the presence of heterogeneous groups. On the other hand, an "excessive" amount of disparate impact might also reflect a discriminatory intent against Sicilians. Determining whether the disparity is excessive requires data on the fraction of individuals per population that a conscientious, unbiased police officer should have deemed suspicious

enough, based on the evidence at the officer's disposal, to warrant a search. In practice, this type of evidence is almost surely not available, and even if it were available, it would be extremely contentious because it would require the court to make a determination as to the degree of suspicion warranted by any number of investigative clues.

Worse yet, the degree of suspicion warranted by a certain set of characteristics is itself endogenous, in other words, it is a function of police behavior. If, for example, the police rarely searched individuals wearing ragged clothes, these individuals will be tempted to carry drugs. In this case, ragged clothes would warrant suspicion. But if the police, perhaps at the behest of a judge, were to intensify the search of individuals with ragged clothes, these individuals would probably be deterred, or they would invest in disguising their appearance, and thus ragged clothes would become a non-suspicious indicator.<sup>29</sup>

These difficulties illustrate, in a nutshell, the controversy over racial profiling. The argument demonstrates why disparate impact is not and should not be per se evidence of an intent to discriminate. Rather, the disparate impact might simply reflect the goal-oriented behavior of individual police officers pursuing crime. On the other hand, an "excessive" amount of disparate impact might also reflect a discriminatory intent against Sicilians. Since determining whether the disparate impact is excessive is very difficult in practice, analysis of disparate impact alone cannot guide an inquiry into how to detect intent to discriminate.

### C. Advantages of Using Hit Rate Disparity to Detect Bias

Our contention is that, within our model, the intent to discriminate can be ascertained easily, not by looking at the disparity in search rates, but by looking at the disparity in hit rates. We are going to show that, in equilibrium, there is no disparity in hit rates if and only if the police are unbiased. In other words, the disparity in hit rates provides a simple, bright line test for the presence of intent to discriminate.

To understand the above statement, it is necessary to understand what we mean by "in equilibrium." In a rational choice model such as ours, behavior is dictated by the preferences of

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<sup>29</sup> In a more complex model, even race could be "disguised." For example, an individual could hire a courier of a different race, which effectively would serve to disguise his or her race.

agents, in this model citizens and police officers. The rational choice approach assumes that agents choose their behavior to maximize their utility. The effectiveness of one actor's particular course of action depends on what every other actor is doing. When each agent behaves in a way that maximizes her utility given what all other agents are doing, we say that we are in equilibrium.

Our strategy for finding the equilibrium is to start from a polar allocation that is clearly not in equilibrium: we start by assuming that the police utility is not maximized. We then imagine a notional adjustment process where, one by one, police are allowed to change their behavior. Rational police will take the opportunity to change their behavior if that improves their utility. When no additional improvement can be obtained, the adjustment process will come to a rest and equilibrium will have been reached.

Assume for now that the police are unbiased. Let us start out from a polar allocation in which the police only search Milanese, and not Sicilians. Furthermore, let us assume that the police will only search Milanese with ragged clothes because the police assume that Milanese with fancy clothes will almost never carry drugs.

If the police will only search Milanese with ragged clothes, we can conclude that the crime rate will be lower among Milanese with ragged clothes than among the Sicilians because the Milanese expect to be searched with high probability. Among the Sicilians with ragged clothes, the crime rate will be high because these citizens do not expect to be searched at all. This is not a stable outcome, however, because unbiased officers can get a higher likelihood of success—a higher hit rate—by searching Sicilians with ragged clothes instead of Milanese. Thus, officers who are allowed to change their behavior will switch to searching Sicilians with ragged clothes.<sup>30</sup> This switch will cause the crime rate among Milanese to increase a little bit, while among Sicilians with ragged clothes the crime rate will go down a little bit. Still, unbiased police prefer to search Sicilians with ragged clothes, who still have a higher hit rate. The adjustment process therefore continues until no unbiased officer wants to search Si-

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<sup>30</sup> We assume implicitly that the police are aware of the hit rates in different groups. See the empirical results discussed in Part III, where the observed equalization of the hit rates appears consistent with this assumption.

cilians over Milanese, that is, when the hit rate is the same for Sicilians with ragged clothes and Milanese with ragged clothes.

Let us now examine the adjustment process when the police are biased against Sicilians. What will be the equilibrium? Starting from the original polar allocation where only Milanese are searched, the adjustment process will unfold much like in the previous case, except that the process will not come to a rest when the hit rates are the same between the Sicilians and Milanese. Police will still switch to searching Sicilians with ragged clothes because biased police derive an extra measure of utility from harassing Sicilians. In order for the adjustment process to come to a rest, the hit rate needs to be lower for the Sicilians than for the Milanese citizens who are searched. The adjustment process comes to a rest when the difference in hit rates exactly offsets the bias. At equilibrium, therefore, the disparity in hit rates reflects the size of the police bias.

This simple thought experiment shows that the hit rates between Sicilians and Milanese searched should be the same if the police lack intent to discriminate. Note that the search rate may well be higher among Sicilians, especially if, as in our example, individuals with ragged clothes are more common among Sicilians. However, we have demonstrated that there is an easy way to check if that disparity reflects an intent to discriminate. If the police intend to discriminate against Sicilians, the hit rate will be lower among the Sicilians who are searched than among the Milanese who are searched. In other words, there will be a disparity in hit rates. Absent a disparity in hit rates, any disparity in search rates must be ascribed to the heterogeneity across groups (in our example, the fraction of citizens with ragged clothes).

#### D. Discussion

The analysis of our model yields a bright line test: intent to discriminate is detected from the disparity in hit rates across protected groups. In the presence of intent to discriminate, there will also be an *unjustified* disparate impact across protected groups, i.e., there will be discriminatory effect.

The reader may wonder how much the assumptions of our model can be relaxed, thereby making the model more realistic, while still retaining the validity of the hit rates test. The model can be extended considerably, for example, to accommodate any number of protected categories (instead of only Sicilians and Milanese) and of subgroups (instead of only ragged- and fancy-

clothed). The bias can also be modeled differently, for example, as an extra preference for detecting *guilty* members of a protected group instead of simply for searching them. Finally, one can also allow for mutable characteristics, namely, for characteristics that can be changed at a cost. For example, one can allow a potential criminal the option to disguise himself to reduce the probability of being searched. In our model, this would mean allowing the ragged-clothed citizens to invest in fancy clothes to reduce the probability of detection.<sup>31</sup> Yet even after adding all these features in order to make the model more realistic, the basic test remains intact: any intent to discriminate against a group translates into lower hit rates on that group in equilibrium.<sup>32</sup>

### III. THE MARYLAND CASE STUDY

To illustrate how the methodology would be applied in a court case, we report the results of statistical analyses carried out on data collected by the police as a result of the settlement agreement and consent decree in *Wilkins v Maryland State Police*.<sup>33</sup> The settlement entailed the payment of money to the plaintiff, the formulation of a statement by the police renouncing racial profiling, and the collection of the data presented here.<sup>34</sup> The tables reported in this Part are based on the analysis originally performed in KPT, which is based on data from 1,590 vehicular searches performed between January 1995 and 1999 on a stretch of I-95 in Maryland.<sup>35</sup> We also report the findings of subsequent analyses performed by Gross and Barnes (2002)<sup>36</sup> and by Barnes<sup>37</sup> that use larger and more up-to-date data sets on Mary-

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<sup>31</sup> In a more realistic model, this would include the ability to hire a courier of another race. See note 30.

<sup>32</sup> See Nicola Persico and Petra Todd, *Using Hit Rates Tests for Racial Bias in Law Enforcement: Vehicle Searches in Wichita*, National Bureau of Economic Research Working Paper no. 10947 (Dec 2004), available at <<http://www.nber.org/papers/w10947>> (last visited Apr 13, 2005).

<sup>33</sup> Consent Decree Dated Apr 2, 2003, *Wilkins v Maryland State Police*, Civil Action No CCB-93-468 (D Md 2003).

<sup>34</sup> *Id* at 9 (referring to Settlement Agreement included in the Consent Decree as Attachment A).

<sup>35</sup> Knowles, Persico, and Todd, 109 J Polit Econ at 215 (cited in note 8).

<sup>36</sup> Gross and Barnes, 101 Mich L Rev at 653 (cited in note 3).

<sup>37</sup> Katherine Y. Barnes, *Assessing the Counterfactual: The Efficacy of Drug Interdiction Absent Racial Profiling* \*2-6 (Oct 8, 2004) (unpublished manuscript, on file with authors).

land State Police stops and searches, and discuss the interpretations that they offer for their findings.

A first look at the data reveals a familiar pattern of disparate impact. Table 1 reveals that, of those searched, 63 percent were African-Americans, unquestionably a much higher percentage than the fraction of African-American motorists on the road. We also note that men are disproportionately more likely to be searched than women (93 percent of those searched are men). Other features of the data, while of lesser interest because not directly related to protected categories, are still instructive about police behavior. For example, older vehicles represent 22 percent of all searches, luxury vehicles 8 percent, third-party vehicles 18 percent, and 31 percent of searches were made at night. Many searches resulted in marijuana finds (23 percent), while 8 percent resulted in cocaine finds.

The first two rows in Table 2 report the hit rates by race. Of the white motorists searched, 32 percent possessed illegal drugs. Of the African-American motorists searched, 34 percent possessed illegal drugs. Despite the wide disparity in search rates, hit rates are very close. In fact, a Pearson chi-square test cannot reject the hypothesis that the two hit rates are the same.<sup>38</sup> According to our analysis, this suggests that the police have no intent to discriminate against African-Americans.<sup>39</sup> To see why, let us apply the logic of our model to this case. Presumably, police officers are aware or assume that, at the current status quo, the hit rates are approximately the same from searching the two groups (whites and African-Americans). Then, a biased officer would know that switching some of her searches to African-Americans would allow her to indulge in her prejudice at no cost in terms of success rate. Thus, if officers were biased, they would all switch from the status quo towards searching more African-Americans, and this process would continue until the African-American hit rate were sufficiently lower than the white hit rate that the bias was offset. But in the data we cannot reject the hy-

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<sup>38</sup> The p-value is 0.33.

<sup>39</sup> For Hispanics, however, the hit rate (not reported in Table 2, *infra*) is lower, possibly indicating a bias against Hispanics. An alternative explanation for this finding could be that Hispanics are more likely to be “mules,” in other words, to transport high-value shipments of drugs not for personal use. To address this issue, one can perform more sophisticated, though somewhat more subjective, analyses based on the quantities of drugs found. See Knowles, Persico, and Todd, 109 *J Polit Econ* at 224-27 (cited in note 8) (applying an alternative analysis to the original data, where the definition of “a successful search” is based on four different categories of “guilt,” each of which is determined by the quantity of drugs with which an individual is found).

pothesis that the hit rate is the same for whites and African-Americans. Therefore, we cannot reject the hypothesis that the police are not biased.

The next two rows ask the same question with regards to sex. Again, despite the wide disparity in search rates, hit rates are very close and the chi-square test does not reject the hypothesis that the two hit rates are the same.<sup>40</sup> According to our analysis, this suggests that the police have no intent to discriminate against women.

The remaining rows compare hit rates along other dimensions, such as the time of day at which the search occurred, the age of the car, etc. Hit rates are similar across these dimensions, too.<sup>41</sup> That this similarity in hit rates is found consistently across a variety of characteristics is particularly remarkable in light of the wide disparity in search rates. The equalization of hit rates is exactly what our model would predict when the police are unbiased and are pursuing a search strategy that maximizes the probability of a successful search. Thus, the fact that hit rates are equalized in our data suggests that our model of police behavior accurately approximates the actual behavior of the Maryland State Police, at least the part of it that is represented in our data. As an important corollary, the evidence suggests that the disparity in search rates between African-Americans and whites does not reflect an intent to discriminate on the part of the police.

Gross and Barnes (2002) as well as Barnes analyze data obtained from the Maryland State Police.<sup>42</sup> Using larger data sets,<sup>43</sup> they report similar findings concerning the relative similarity in hit rates across races.<sup>44</sup> Barnes, in particular, reports the same striking absence of disparity in hit rates reported in our data.<sup>45</sup> Gross and Barnes (2002), however, come to the conclusion that

<sup>40</sup> The p-value is 0.37.

<sup>41</sup> The only time that the Pearson test rejects equality of hit rates is for luxury cars, which are somewhat less likely to result in a find. According to our model, this indicates some preference on the part of the police for searching luxury cars. It is interesting, however, that the disparity disappears when we focus on “large” drug finds. See Knowles, Persico, and Todd, 109 J Polit Econ at 226 (cited in note 8).

<sup>42</sup> See Barnes, *Assessing the Counterfactual* at \*38 (cited in note 38); Gross and Barnes, 101 Mich L Rev at 653 (cited in note 3).

<sup>43</sup> Their data sets subsume those used in KPT.

<sup>44</sup> See Gross and Barnes, 101 Mich L Rev at 669, Table 8 (cited in note 3) (finding that the hit rates for “[a]ll [d]rugs” were similar for whites and African-Americans).

<sup>45</sup> See Barnes, *Assessing the Counterfactual* at \*40, Table 4 (cited in note 38). We note that the hit rates in Barnes are computed as a fraction of stops and not, as in KPT, of searches. Moreover, Table 4 restricts attention to hit of “hard drugs.” Id.

the behavior of the Maryland State Police is not likely to be found permissible by the courts.<sup>46</sup> In our reading of their argument, these authors do not argue that police behavior suggests bias. In fact, we note that these authors point out that the police are primarily motivated by the desire to maximize the capture of drugs and drug traffickers.<sup>47</sup> They argue, however, that in so doing, the police choose whom to stop on the basis of race.<sup>48</sup> These authors do not consider the possibility that the police use other characteristics in deciding whom to stop, and that these characteristics simply happen to be correlated with race. We find this possibility to be entirely plausible, although we know of no evidence speaking to this issue. In the absence of evidence on this specific point, and in the presence of evidence that the police are unbiased, we think the prima facie argument for intent to discriminate cannot be successfully established. As we suggested, we believe that the argument for intent to discriminate instead must be made based on the disparity in hit rates, if such a disparity exists.

#### IV. CONCLUSION

The legal approach to using statistics to prove racial profiling has been somewhat muddled, largely because there has been no bright-line test of how to interpret such statistical evidence to infer the existence, or nonexistence, of intentional discrimination. Proving intent has long been a crucial part of the law, for mere disparate impact could occur even in the absence of bias. We have shown that statistical evidence can be used to show intent, provided that it is analyzed properly and under a model with strong economic underpinnings. Our analysis shows that hit rates, and not search rates, are the key variables necessary to prove intent to discriminate within our policing model. The intuition from the analysis extends to “auditing” environments, such as customs searches,<sup>49</sup> in which it is meaningful to talk about hit rates.

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<sup>46</sup> See Gross and Barnes, 101 Mich L Rev at 737-38, 744 (cited in note 3) (claiming that courts increasingly do not want to hear that officers have been making stops on the basis of race). It must be pointed out that Gross and Barnes could not foresee the decision in *Anderson*.

<sup>47</sup> Gross and Barnes, 101 Mich L Rev at 753 (cited in note 3).

<sup>48</sup> *Id.*

<sup>49</sup> See *Anderson*, 355 F3d 1021 (analyzing the constitutionality of racial disparities in customs searches).



In order to combat racial discrimination in policing, we must assess the extent to which police work is tainted by racial bias. This paper has proposed a rigorous empirical methodology, based on recent advances in economic analysis, for detecting bias in police interdiction. The bright-line test we propose is consistent with the doctrinal approach espoused by the Seventh Circuit in *Anderson*.

It is our hope that courts, practitioners, and experts will look to this methodology when evaluating the merits of racial profiling claims in drug interdiction cases, which affect not just highway stops, but customs as well. By using this approach to find whether or not there is intentional discrimination, the court system can mete out true justice for all of the parties involved.

TABLE 1  
Means and Standard Deviations of Variables used in Analysis  
(standard error of the mean shown in parentheses)

	All Obs.	By Race				By Sex	
		African- American	Hisp.	White	Other	Female	Male
<b>African-American</b>	0.63 (0.01)	1.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.64 (0.04)	0.63 (0.01)
<b>White</b>	0.29 (0.01)	0.00 (0.00)	0.00 (0.00)	1.00 (0.00)	0.00 (0.00)	0.35 (0.04)	0.29 (0.02)
<b>Hispanic</b>	0.06 (0.01)	0.00 (0.00)	1.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.07 (0.01)
<b>Female</b>	0.07 (0.01)	0.07 (0.01)	0.00 (0.00)	0.09 (0.01)	0.30 (0.05)	1.00 (0.00)	0.00 (0.00)
<b>Cocaine</b>	0.08 (0.01)	0.10 (0.01)	0.03 (0.02)	0.04 (0.01)	0.15 (0.08)	0.09 (0.03)	0.08 (0.01)
<b>Marijuana</b>	0.23 (0.01)	0.23 (0.01)	0.04 (0.02)	0.26 (0.02)	0.20 (0.09)	0.21 (0.04)	0.23 (0.01)
<b>Crack Cocaine</b>	0.04 (0.01)	0.05 (0.01)	0.01 (0.01)	0.01 (.004)	0.05 (0.05)	0.06 (0.02)	0.04 (0.01)
<b>Heroin</b>	0.02 (.004)	0.02 (.004)	0.03 (0.02)	0.03 (0.01)	0.05 (0.05)	0.06 (0.02)	0.02 (.003)
<b>Morphine</b>	0.001 (.001)	0.00 (0.00)	0.00 (0.00)	0.002 (.002)	0.00 (0.00)	0.00 (0.00)	0.001 (.001)
<b>Other Drugs</b>	0.01 (.003)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.01 (0.01)	0.01 (.003)
<b>Paraphernalia</b>	0.01 (.002)	0.003 (0.002)	0.010 (0.01)	0.02 (0.01)	0.00 (0.00)	0.01 (0.01)	0.007 (.002)
<b>Older Vehicle (10 years or older)</b>	0.22 (0.011)	0.20 (0.013)	0.247 (0.044)	0.28 (0.021)	0.20 (0.092)	0.18 (0.036)	0.23 (0.011)
<b>Luxury Model</b>	0.08 (.007)	0.10 (0.01)	0.09 (0.03)	0.05 (0.05)	0.05 (0.01)	0.08 (.025)	0.08 (.007)
<b>Third party vehicle</b>	0.18 (0.010)	0.22 (0.013)	0.25 (0.044)	0.09 (0.014)	0.20 (0.092)	0.20 (0.027)	0.18 (0.010)
<b>Night (12am-6am)</b>	0.31 (0.01)	0.35 (0.02)	0.44 (0.05)	0.23 (0.02)	0.25 (0.10)	0.35 (0.04)	0.43 (0.01)
<b>Number of Observations</b>	1590	1007	97	466	20	117	1473

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*DETECTING BIAS*

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TABLE 2  
Proportion of Vehicles Searched Found to be Carrying Drugs

	<b>Guilty #1 (includes drugs in any amount)</b>	<b>Guilty #2 (excludes marijuana &lt; 2 grams)</b>	<b>Guilty #3 (excludes marijuana in any amount)</b>	<b>Guilty #4 (Felony)</b>
<b>African-American</b>	0.34	.26	.16	.13
<b>White</b>	0.32	.21	.07	.03
<b>Male</b>	0.32	.23	.12	.09
<b>Female</b>	0.36	.27	.17	.15
<b>Day</b>	.32	.24	.13	.09
<b>Night</b>	.33	.24	.13	.10
<b>Luxury</b>	.25	.19	.12	.10
<b>Not Luxury</b>	.33	.24	.13	.09
<b>Older Car (&gt;= 10 years old)</b>	.32	.16	.13	.16
<b>Newer Car (&lt; 10 years old)</b>	.33	.15	.13	.15
<b>Third party vehicle</b>	.29	.22	.19	.22
<b>Own vehicle</b>	.33	.14	.11	.14