NOTES

DISCRIMINATION DURING TRAFFIC STOPS: HOW AN ECONOMIC ACCOUNT JUSTIFYING RACIAL PROFILING FALLS SHORT

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The last decade has seen a noted increase in the amount of traffic-stop data available for researchers hoping to analyze racial profiling on America's highways. A group of economic scholars—Knowles, Todd, and Persico—proposed a bright-line statistical test that asks whether different racial groups have the same hit rate, or to put it differently, are searches of individuals equally efficacious, regardless of their race? Accepting this conception of racial profiling as a minimum floor, I apply the test to a superior and newly-compiled data set of nine million Illinois traffic stops. The Illinois police fail the bright-line test and show signs of discrimination against Hispanic, Asian, and Black motorists. I then examine whether Seventh Circuit equal protection precedent would permit an Equal Protection claim based on that statistical disparity alone, concluding that additional evidence is needed to satisfy the discriminatory intent prong.

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Introduction

In the fall of 1992, the Illinois State Police stopped and searched a White motorist. The search resulted in a large drug bust—the police found over 200 pounds of marijuana in the trunk—but it also set in motion an unexpected series of events. The driver's defense attorney "suspected that state troopers were stopping motorists based on skin tone or travel patterns."[1] As part of the defense strategy, the attorney hired Hispanic private investigator Peso Chavez and asked him to recreate the circumstances of the stop using a rented red car with California license plates, fast food wrappers, a cell phone, open maps, and a gym bag.[2]

Chavez drove the rented car along an Illinois highway while a public defense attorney followed in a separate vehicle. Although the Illinois State Police conceded at trial that Chavez did not violate any traffic laws, a state trooper followed Chavez for half an hour and then pulled him over.[3] The police asked Chavez for consent to search his vehicle (he declined) and detained him for a canine search. The dog failed to pick up any suspicious scents on its first walk-around; a second search, however, indicated a hit. The police searched Chavez's entire car, including the engine. When the search revealed no contraband, the police allowed Chavez to go. The officer who completed the field report misreported Chavez's race as White.[4]

Reports about incidents like the stop and search of Peso Chavez have brought increased public attention to the issue of racial profiling.[5] Commentators sometimes define racial profiling[6] as a law

1 Chavez v. Ill. State Police, 251 F.3d 612, 623 (7th Cir. 2001).

2 *Id.* at 623.

3 *Id.*

4 *Id.* at 624.

5 *See* Noah Kupferberg, Note, *Transparency: A New Role for Police Consent Decrees*, 42 Colum. J.L. & Soc. Probs. 129, 135, 138–39, 142 (2008) (discussing incidents that brought attention to racial profiling in Los Angeles, New Jersey, and New York City).

6 "Racial profiling" refers to the practice or statistical pattern of performing law-enforcement events (like searches) in a disproportionate way. For example, if officers search Blacks more often than Whites (compared to their population benchmarks, which is

enforcement practice based on the assumption that members of targeted racial or ethnic groups are more likely to commit certain crimes than members of other racial or ethnic groups.[7] As a result of this assumption, police focus enforcement on targeted groups disproportionately. Many of the problems with racial profiling are obvious: The practice undermines the legitimacy of law enforcement by selectively enforcing laws, it isolates and targets minority groups,[8] and it humiliates individual suspects who feel they are the targets of racial discrimination.[9]

Increased public scrutiny of racial profiling has helped to spur recent efforts to collect traffic-stop data, which are larger and broader in scope than past data-collection efforts.[10] These data collections aim to measure the extent of racial profiling on American highways. The most expansive new data set is from Illinois, the same state where Peso Chavez attempted a statistics-based racial profiling lawsuit over ten years ago.[11] Then Illinois State Senator Barack Obama, reacting in part to the Seventh Circuit's dismissal of Chavez's case, sponsored legislation[12] that resulted in the collection of data from nearly three million traffic stops annually for almost a decade.

Although currently available traffic-stop data is more comprehensive than ever before, scholars still disagree about the appropriate method of analyzing statistical irregularities in data when attempting

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the percent of people in the entire population who are of a given race) this would qualify as racial profiling. As Part I explains, however, mere profiling is not universally considered wrong. When referring to preferences, biases, or outward behavior that are better explained by the presence of racism than any efficiency rationale, I use the term "discriminatory racial profiling." When referring to a practice or statistical pattern of searches that affects a racial group disproportionately but that can be explained by an efficiency rationale, I use the term "efficient racial profiling." This Note does not take a stance on when (if ever) racial profiling is acceptable. Employing the Knowles, Todd, and Persico (KTP) hit-rate test, however, assumes a baseline practice of efficient profiling beyond which any statistical discrepancy in hit rates among races indicates discriminatory intent.

7 *See, e.g.*, Samuel R. Gross & Debra Livingston, *Racial Profiling Under Attack*, 102 COLUM. L. REV. 1413, 1415 (2002) (using the term "racial profiling" to mean police action taken "because the officer believes that members of that person's racial or ethnic group are more likely than the population at large to commit the sort of crime the officer is investigating").

8 *See* Brandon Garrett, *Remedying Racial Profiling*, 33 COLUM. HUM. RTS. L. REV. 41, 50 (2001) (arguing that when racial profiling "figures show stark racial disparity, their message may be so strong that they cause minorities to feel targeted").

9 *See* Gross & Livingston, *supra* note 7, at 1431–32 (observing that New York City's stop-and-frisk campaign humiliated profiled suspects).

10 Kupferberg, *supra* note 5, at 129, 130 (2008) (noting high-profile stories and public pushback on the issue of racial profiling).

11 Chavez v. Ill. State Police, 251 F.3d 612 (7th Cir. 2001).

12 *See infra* note 57 (describing the legislation sponsored by then State Senator Obama and noting it may have been introduced in response to the *Chavez* case).

to identify racially biased policing. Knowles, Todd, and Persico (KTP), a trio of economic scholars, studied early collections of traffic-stop data and argued that simple racial discrepancies do not necessarily imply discriminatory behavior.[13] Instead, KTP developed a rational-choice model of patrolling. Their model, which assumes that police maximize the likelihood of drug busts while motorists minimize the risk of getting caught with contraband, leads to a bright-line statistical test: When the odds of a successful police search (the hit rate) are relatively equal across racial groups, then the police are not engaging in discriminatory racial profiling.[14] Applying their model to a set of Maryland traffic-stop data from 1995 to 1999, KTP found no evidence of discriminatory racial profiling because the hit rates were similar across races.[15]

The first central concern of this Note is to engage the KTP model on its own terms by applying the test to the Illinois data set, and determining whether the data indicates discriminatory racial profiling. Many scholars dispute that an economic conception of police behavior adequately captures the magnitude of racial profiling because it accepts efficient racial profiling as permissible and non-discriminatory.[16] However, the KTP model can be a valuable tool even for those concerned that an economic conception of racial profiling fails to represent the full extent of racial discrimination. The test establishes a baseline for law enforcement: A set of traffic-stop data that fails the KTP test, which prohibits as discriminatory only *inefficient* racial profiling, will also fail any more demanding test. This Note presents a newly compiled set of Illinois traffic-stop data, which is over ten times larger than the original data set used by KTP and much better suited for robust statistical analysis. The new Illinois data fails the KTP test, showing disparate hit rates across racial groups and suggesting discriminatory racial profiling that cannot easily be explained by an efficiency rationale. Furthermore, the new Illinois data shows that police discriminate against Hispanics—a group that

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13 John Knowles, Nicola Persico & Petra Todd, *Racial Bias in Motor Vehicle Searches: Theory and Evidence*, 109 J. POL. ECON. 203 (2001) [hereinafter KTP, *Racial Bias*].

14 *Id.* at 205–06.

15 *Id.* at 216, 219.

16 Bernard E. Harcourt, *Rethinking Racial Profiling: A Critique of the Economics, Civil Liberties, and Constitutional Literature, and of Criminal Profiling More Generally*, 71 U. CHI. L. REV. 1275, 1277 (2004) (noting that some "civil liberties advocates" dispute the notion that equal or lower hit rates reflect policing efficiency and argue that it is "'plainly unconstitutional' to use race in the decision to search motorists," even if it does increase efficiency).

was understudied in the original data set tested by KTP[17]—at statistical levels that vastly exceed the levels of discrimination against Blacks.

The second major concern of the Note is to ask whether the profiling claim rejected in *Chavez v. Illinois State Police*, based on the rudimentary statistical evidence available at the time, might turn out differently today in light of new statistical data and the disparate hit-rate finding. Although predicting whether statistics alone will be enough to sustain a legally cognizable claim against the government is an uncertain task, in doing so this Note responds to a challenge laid down by the *Chavez* court. The Seventh Circuit in *Chavez* followed Supreme Court equal protection precedent when granting summary judgment for the Illinois State Police.[18] But the court criticized the statistical evidence presented by Chavez and suggested that better traffic-stop data would have been more probative.[19] The new Illinois traffic-stop data presents an opportunity to test that theory. While I argue that, ultimately, an equal protection claim relying solely on the new hit-rate data will probably not succeed, I propose several ways that racial profiling plaintiffs can make use of the data to advance their claims.

Part I of this Note provides background on the KTP economic model and related literature, and explains why the hit-rate disparity test is worth studying in the first place. Part II presents the Illinois hit-rate results from a set of nearly ten million traffic stops, analyzed for this Note, and posits that police in Illinois are engaging in discriminatory racial profiling. Part III returns to the *Chavez* case, giving further legal background on the rules that govern racial profiling claims and analyzing whether the data assembled in Part II would be sufficient to prove a claim of racial profiling. This Note concludes by proposing several ways for litigants to use hit-rate data and discussing alternative remedies for advocates and racial profiling victims.

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17 *See* KTP, *Racial Bias*, *supra* note 13, at 227 (noting that the paper's results with respect to Hispanics were only "suggestive because [the] data set contain[ed] so few Hispanics").

18 *See infra* Part III.A (discussing equal protection precedents and their application in *Chavez*).

19 *See* Chavez v. Ill. State Police, 251 F.3d 612, 642–45 (7th Cir. 2001) (noting that the type of statistical evidence offered by Chavez was not ideal for proving his claim). See *infra* Part III.B for a complete discussion of the possible improvements.

I
THE ECONOMIC MODELERS

A. Background of the KTP Model

Concerns about racial profiling by law enforcement have changed the landscape of available information on policing patterns. Historically, it has been difficult to obtain data on law enforcement activity. But the past decade has seen a marked increase in lawsuits, settlement decrees, and proactive legislation mandating some kind of data collection by police officers, often in the form of traffic-stop data.[20]

As police departments began to produce traffic-stop data, scholars asked how best to analyze and study that data. KTP wrote an influential article that introduced and defended an economic-modeling approach for evaluating whether discriminatory racial profiling existed in the jurisdiction where data was collected.[21] This Note focuses on the KTP article because it has had wide-reaching academic impact[22] and is especially well-suited to testing the Illinois data.[23]

The KTP approach relies on several premises, not all of which are universally accepted. First, the model assumes that disproportionate search rates of minority motorists are irrelevant. For example, consider a sample of 40,000 traffic-stop searches in a jurisdiction where Blacks make up less than 25% of the population, and assume that 50% of the cars searched by police were driven by Blacks. KTP argue that this fact alone, termed a disparate *search rate*, is not proof of discriminatory racial profiling. Their underlying—and admittedly controversial[24]—rationale is two-fold: First, it is incorrect to assume that all groups carry contraband at an equal rate. Second, if a group carries contraband more often than average, then the police are justified in searching that group more often than average.[25]

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20 For a summary of three recent settlement decrees, see Kupferberg, *supra* note 5, at 133–45.

21 KTP, *Racial Bias*, *supra* note 13.

22 *See infra* note 39 and accompanying text (discussing citations of KTP's article in economic and legal scholarship).

23 *See infra* notes 54–56 and accompanying text (explaining why hit-rate analysis requires a large sample of stops).

24 Not everyone agrees that minorities are more likely to carry drugs. *See, e.g.*, Daniel S. Korobkin, Racial Profiling: A New Challenge in Public Policy 22–23 (Mar. 1, 2002) (unpublished thesis, Swarthmore College) (on file with the *New York University Law Review*) (noting that Whites are almost as likely to use drugs, and that statistics showing otherwise can be misleading).

25 KTP, *Racial Bias*, *supra* note 13, at 206 (noting that some statistical patterns of differential searching could be due to "statistical discrimination and not to racial prejudice").

But if differences in search rates across racial and ethnic groups are insufficient to indicate discriminatory racial profiling, what type of data does suffice to do so? KTP argue that disparities in *hit rates* are the answer, and that these disparities serve as a bright-line test for discriminatory racial profiling.

In order to understand hit rates, it is easiest to begin at the micro level. When an officer pulls someone over, she begins a traffic stop. At some point during that stop, the officer decides whether to initiate a search of the car or the driver (or both). If the officer decides to conduct a search, that search is recorded on the traffic-stop form either as a successful "hit," meaning that the officer found drugs or other contraband, or as an unsuccessful search with no "hit." After police collect a set of traffic-stop forms and enter them into a database, they can produce a macro-level hit rate. This hit rate simply reflects the odds that a search, if performed, will be successful. Hit rates for traffic-stop programs typically range from about 10% to 20%.[26] KTP argue that a comparison of each racial group's hit rate provides a bright-line statistical test, because "[w]hen the hit rates are the same across racial or ethnic lines . . . the police are not bigoted in their searches because they have no incentives to search more or fewer motorists of any particular race."[27] For example, we should be able to compare the hit rate of Blacks to the hit rates of Whites and Hispanics and, in the absence of discrimination, find that the three hit rates are equal.

Other key assumptions underlying the KTP theory are that police try to "maximize successful searches" and that "race helps predict criminality."[28] What follows from these assumptions is a prediction that rational officers will search such that "the returns from searching [are] equal across"[29] racial groups—in other words, so that the hit rates are identical. If the hit rates become momentarily unequal, then the officers will respond by conducting more searches of the groups with higher hit rates and fewer searches of the groups with lower hit rates.

KTP also assume "that motorists take into account the probability of being searched in deciding whether to carry

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The summary of KTP's article that follows above is borrowed, in part, from Harcourt, *supra* note 16.

[26] *See* Oren Bar-Gill & Barry Friedman, *Taking Warrants Seriously*, 106 Nw. U. L. Rev. (forthcoming 2012) (noting hit rates in various law enforcement programs).

[27] Harcourt, *supra* note 16, at 1276 & n.3 (citing the KTP research).

[28] KTP, *Racial Bias*, *supra* note 13, at 205.

[29] *Id.* at 206.

contraband."[30] If police search one race less than average, then that race will respond by carrying contraband more often. As a result, the odds of a police officer conducting a successful search of that race will increase. As the officers and motorists react to each other, the end result in a nondiscriminatory world ought to be hit-rate equilibrium.[31] The KTP model does not predict what the actual equilibrium rate will be—finding that 5% of searches are successful is just as consistent with KTP's theory as finding that 25% of searches are successful. But if the data indicates a discrepancy in hit rates across racial groups, KTP characterize the agency as engaging in race-based discrimination.[32] This conclusion follows because the police would be more efficient (meaning they would have more successful searches) if they searched the groups with higher hit rates more often and searched the groups with lower hit rates less often.[33]

B. Critiques and Advantages of the KTP Model

The legal literature criticizes the KTP model for failing to capture the essence of the racial profiling problem because the model suggests it is permissible for officers to engage in rational racial profiling. Even if this is an efficient approach from an economic perspective, obtaining efficiency may be undesirable if it comes at the cost of permitting public officials to act in a way that is not race-neutral. To some extent, our anti-discrimination norms value equal and race-neutral treatment despite the potential costs of such programs.[34]

Professor Bernard Harcourt criticizes the KTP approach for misunderstanding social costs and the proper aims of policing. At its core, the KTP model assumes that an effective law enforcement program seeks to maximize successful searches. But sometimes, Harcourt argues, "minimizing the social costs of crime is at odds with maximizing search success rates."[35] According to Professor Harcourt, the

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[30] *Id.*

[31] *Id.*

[32] *See id.* at 206, 210 (noting that the test can be performed by looking at the "aggregations" of racial subgroups and that police officers are defined as "racially prejudiced if he or she exhibits a preference for searching motorists of one race").

[33] *See id*. at 215 (arguing that, given resource constraints, the "police would target its available resources on those groups in which the fraction of motorists carrying drugs is highest").

[34] *See generally* Korobkin, *supra* note 24, at 30 ("[I]t is difficult to deny that a law-abiding individual who is treated with more suspicion by police officers because of the color of her skin has a legitimate complaint . . . . One's basic expectation in a free society is to be treated fairly by one's own government, and being the victim of racial profiling cuts against that expectation."); *supra* notes 7–9 and accompanying text (describing problems with discriminatory racial profiling).

[35] Harcourt, *supra* note 16, at 1295.

KTP model ignores "the effect of racial profiling on the absolute number of motorists transporting illicit drugs."[36] Essentially, the concern is that adherence to a model like KTP's may result in a long-term increase in crime.

Another criticism of the KTP hit-rate test is that it fails to reflect the entirety of discriminatory behavior by law enforcement.[37] Police officers can discriminate against an individual during a traffic stop in several ways, such as deciding whether to stop the car at all or whether to give a ticket instead of a warning. The KTP test only detects racial discrimination in traffic stops where police decide to perform searches.

This Note opts not to dwell on these criticisms for two reasons. First, almost all critics of KTP attack the model as an under-inclusive conception of racial profiling: KTP fail to capture the full extent of racial profiling and the problems it creates. Because those critics argue for a more expansive conception of the costs generated by profiling, they would likely agree that KTP provide, at the very least, a racial-profiling baseline: If a set of traffic-stop data fails the KTP test—as the Illinois data presented in Part II does[38]—then both KTP and their critics can agree that the jurisdiction is engaging in discriminatory racial profiling, even if there is genuine debate about what it means if a data set passes the KTP test.

Second, while the KTP approach draws its fair share of legal critics, it has been hugely influential in the economic literature.[39] The test's influence may derive from its advantages over the alternative statistical techniques available to legal scholars and policy analysts. One advantage is that the KTP approach accounts for an omitted-variable bias that plagues both simple comparisons of disparate search rates and more complicated statistical techniques such as regression analysis.[40]

Omitted-variable bias occurs when officers stop and search drivers for reasons that are not adequately captured by traffic-stop

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[36] *Id.* at 1296. Harcourt proceeds to provide mathematical support for his argument by considering the effects of comparatively lower elasticity (that is, ability to be deterred by policing patterns) among racial groups. *Id*. at 1298–1303.

[37] For example, KTP notes that their model "abstracts from the issue of the thoroughness of searches;" essentially it ignores the length of time that individuals are subject to police searches. *See* KTP, *Racial Bias*, *supra* note 13 at 215.

[38] *See infra* Part II.B (discussing findings from the Illinois data).

[39] A search on Google Scholar revealed 278 citations to the KTP, *Racial Bias* article. *See* Racial Bias In Motor Vehicle Searches: Theory and Evidence, GOOGLE SCHOLAR, http://scholar.google.com (search "Knowles Todd Persico," then follow "Cited by 278" hyperlink) (last visited July 17, 2012).

[40] *See infra* notes 44–48 and accompanying text (discussing regression analysis).

forms but which may be correlated to the race of the driver. Such a correlation could give the false impression that police stop and search minority drivers more often than average on account of their race, even though the actual reason for stopping and searching drivers is race-neutral.[41] For example, suppose that police learn that drivers with air fresheners on their rearview mirrors are extremely likely to carry drugs in their cars, but that the traffic-stop form does not ask the officer whether she notices an air freshener in the car. The presence of an air freshener, an important variable for officers, is thus *omitted* from the data collection. But suppose further that the use of air fresheners is higher among Asian drivers than non-Asians. Because the omitted variable correlates with a recorded variable—whether the driver is Asian—this could bias the statistical analysis and erroneously lead to the conclusion that police search Asians more often than average *because they are Asian*. In fact, the police could be searching all people with air fresheners in their cars at the same rate, regardless of race. A statistician who could control for the presence of air fresheners would find that, in this scenario, race does not predict searches at all; rather, air fresheners alone explain why Asians are searched more frequently than non-Asians.

KTP avoid omitted-variable bias by grounding the test in an efficiency rationale.[42] In considering the example above, search rate analysis might conclude that Asians suffer disproportionate searches or discriminatory racial profiling.[43] But beneath the apparent impropriety lies the fact that the police officers search motorists based on the presence of air fresheners. And, in this scenario, air fresheners are a very good indicator of criminality. The KTP test allows the police to search Asians more often than average as long as the hit rate for those searches equals the hit rate for searches of non-Asians. If air fresheners truly are a good proxy for criminality and the police consider nothing else, then the equal hit-rate relationship between Asians and non-Asians will hold true, and the officers' practices will pass the KTP test.

The most obvious alternative to the KTP approach is multivariate regression analysis, a statistical technique often used to study traffic-

[41] KTP, *Racial Bias*, *supra* note 13, at 204–05.

[42] For KTP, *Racial Bias*'s explanation of how they avoid omitted-variable bias, see *supra* note 13, at 212.

[43] Search-rate analysis compares the rate at which a race is searched (as opposed to the rate at which those searches result in hits). *See supra* note 24 and accompanying text.

stop data.[44] Briefly, regression analysis involves collecting as many variables as possible during the data collection stage and then trying to predict the outcome of traffic stops from the variables collected.[45] Researchers look to see if certain variables—for example, race, the age of the driver, or the type of traffic violation committed—have an effect on the predicted outcome of the stop that exceeds a predefined level of statistical significance.[46] Under this model, if race has a statistically significant effect on the outcome of the stop, that effect would indicate discriminatory racial profiling by officers.

Regression analysis in general can suffer from omitted-variable bias because it is difficult to measure many factors that affect outcomes. But traffic-stop data may be especially susceptible to omitted-variable bias. For example, an elaborate Arizona traffic-stop study examined about twenty variables but could only explain about 26% of the variation in whether drivers received warnings and only about 19% of the variation in whether stops were likely to result in searches.[47] As the authors of that study explain, "[t]he search models are relatively weak in predictive power, indicating that multiple additional factors predicting whether or not a search is conducted are not measured in these data."[48] The weak ability of regression analysis to explain traffic stops and account for omitted-variable bias is a strong argument in favor of considering other statistical tests, including the KTP hit-rate test.

C. The Original KTP Results: No Racial Profiling

Applying their test to a set of Maryland State Police traffic data, KTP found "nearly identical" search success rates between Whites and Blacks and concluded that the data showed "result[s] that [are] consistent with the hypothesis of no racial prejudice. . . . [O]ur findings suggest that police search behavior is not biased against African-American drivers."[49] While KTP found statistically significant

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44 *See, e.g.*, ROBIN S. ENGEL ET AL., UNIV. OF CINCINNATI POLICING INST., TRAFFIC STOP DATA ANALYSIS STUDY: YEAR 3 FINAL REPORT 72–104 (2009) (utilizing regression techniques to study Arizona traffic-stop data).

45 *See* JOHN E. FREUND & BENJAMIN M. PERLES, MODERN ELEMENTARY STATISTICS 396–99, 410–11 (12th ed. 2007) (describing regression analysis and noting that "we try to express, or approximate, relationships between known quantities and quantities that are to be predicted in terms of mathematical equations").

46 *Id.* at 412–13 (discussing the t-statistic that results from calculating the standard error of a given coefficient, which allows the researcher to infer the odds that the statistical relationship occurred as a result of pure chance).

47 ENGEL, *supra* note 44, at 77, 92.

48 *Id.* at 103.

49 KTP, *Racial Bias*, *supra* note 13, at 219, 222.

hit-rate differences for Hispanics, they cautioned against drawing conclusions about discriminatory racial profiling given the small number of Hispanics in their sample, and called for "[f]urther investigation . . . with a larger data set."[50]

Some follow-up articles confirmed the equal hit-rate conclusion.[51] Notably, an article coauthored by Persico and Todd presents a table summarizing many other traffic-stop studies to claim "that there is not a large disparity in hit rates for Black and White drivers."[52] Others have disputed the model and the empirical findings.[53]

One important statistical limitation that should be kept in mind is that the current literature uses relatively small or limited data sets and continues to recycle Maryland State Police data.[54] Criticizing

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50 *Id.* at 227. *See infra* Part II.B (discussing my larger sample of Hispanic drivers in Illinois that shows racial profiling under the KTP test).

51 Joseph A. Schafer et al., *Decision Making in Traffic Stop Encounters: A Multivariate Analysis of Police Behavior*, 9 POLICE Q. 184, 200 (2006) (analyzing over 60,000 stops in an anonymous mid-sized city and finding that "[t]he effect of race/ethnicity on the likelihood that a search yielded contraband was not statistically significant"). *See also* Kate Antonovics & Brian G. Knight, *A New Look at Racial Profiling: Evidence from the Boston Police Department*, 91 REV. ECON. & STAT. 163, 171–72 (2009) (concluding that, "[l]ike [KTP], we find no evidence that the probability of guilt conditional on search differs by the race of the driver," but finding other results that are consistent with preference-based discrimination and racial profiling).

52 Nicola Persico & Petra Todd, *Generalizing the Hit Rates Test for Racial Bias in Law Enforcement, with an Application to Vehicle Searches in Wichita*, 116 ECON. J. F351, F361, F362, F364 (2006) (stating that "the empirical results show that the hit rates are very similar across groups of motorists no matter how these groups are defined. . . . [This is] consistent with the notion" that police search Blacks and Hispanics more in order to maximize effectiveness).

53 When Knowles and Hernández-Murillo applied the model to Missouri stops, they rejected the equal hit-rate theory after finding that search success rates were "much higher for white drivers" and therefore suggestive of discriminatory racial profiling against minorities. Ruben Hernández-Murillo & John Knowles, *Racial Profiling or Racist Policing? Bounds Tests in Aggregate Data*, 45 INT'L ECON. REV. 959, 972 (2004). The authors concluded that their "tests reject unbiased policing as an explanation of the disparate impact of motor-vehicle searches on minorities in Missouri." *Id.* at 959. Other modelers have questioned the KTP model more generally and proposed alternatives, suggesting that the profiling models and their applications remain disputed. *See* Shamena Anwar & Hanming Fang, *An Alternate Test of Racial Prejudice in Motor Vehicle Searches: Theory and Evidence*, 96 AM. ECON. REV. 127 (2006) (developing an alternate model that considers the officer's race and, after applying that model to Florida traffic-stop data, finding no evidence of racial prejudice); Dhammika Dharmapala & Stephen L. Ross, *Racial Bias in Motor Vehicle Searches: Additional Theory and Evidence*, 3 CONTRIBUTIONS TO ECON. ANALYSIS & POL'Y 1, 14 (2004) (using Maryland data and cautioning against the KTP approach to studying racial discrimination, in part, because multiple model equilibria are possible, some of which are "consistent with no racial bias" but others which "raise substantial questions . . . by showing that the data" is consistent with statistically significant levels of racial profiling); Hernández-Murillo & Knowles, *supra* at 966–67 (developing a new test that differs from the KTP test for discerning racial profiling).

54 *See, e.g.*, Stan Becker, *Assessing the Use of Profiling in Searches by Law Enforcement Personnel*, J. CRIM. JUST. 183, 186 (2004) (using Maryland data with fewer than 9000

researchers who perform quantitative analysis on police data for having small samples is a bit unfair, given the lack of data that police departments make available. But empirical researchers should continue to keep an eye out for new sources of data and avoid reusing the same Maryland data if possible. In fact, recent legislation in Maryland produced new traffic-stop reports that could be compiled into newer and larger data sets.[55] Still, policy and legal analysts should remain appropriately skeptical when evaluating empirical claims that come from one law enforcement department: Not only do these samples tend to be smaller, but they may also portray a picture that is representative of only one locality or department. And while a sample consisting of 50,000 traffic stops sounds plentiful at first, the reality is that this sample quickly decreases during analysis, especially when examining searches alone. For example, less than 10% of stops result in a search and, of those searches, even fewer involve Hispanic drivers. If one wishes to study situations in which police ask Hispanics for consent to search but Hispanics refuse, 50,000 stops will likely be too few to produce robust conclusions.[56]

II
Applying the KTP Model to a Larger Set of Traffic Stops

A. Construction of the Illinois Data Set

KTP's analysis of Maryland traffic-stop data yields the provocative claim that, despite racial disparities in search rates, an underlying equality in hit rates shows that law enforcement officers are engaged in efficient (rather than discriminatory) racial profiling. The primary concern of this Note is to apply the KTP test to a much larger set of Illinois traffic stops, and determine whether traffic stops in Illinois pass the equal hit-rate test.

Since 2004, the Illinois Traffic Stop Statistics Act has required Illinois police departments to record details of traffic stops that occur within the state[57] and submit these records to the Illinois Department

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stops); Dharmapala & Ross, *supra* note 53 (using Maryland data with fewer than 1600 searches); Persico & Todd, *supra* note 52 (analyzing Wichita, Kansas data that includes fewer than 3000 searches).

[55] The new Maryland traffic-stop reports are available at http://www.goccp.maryland.gov/msac/law-enforcement.php.

[56] For an illustration of the subsets problem, see Anwar & Fang, *supra* note 53, at 141. They start with a data set of over 900,000 stops that is then reduced to fewer than 9000 searches.

[57] 625 Ill. Comp. Stat. Ann. 5/11-212 (West 2011) (amended 2007, 2010, 2012). Former State Senator Barack Obama sponsored the act, introducing the bill by saying, "[T]his is the racial profiling legislation that we've been working on for quite some

of Transportation (DOT), which then analyzes them in annual reports.[58] The Annual Reports, by Alexander Weiss and Professor Dennis P. Rosenbaum, make available statistics on the number of traffic stops that take place in Illinois (about 2.5 million annually),[59] the number of citations issued (about 1.4 million in 2010),[60] and the number of consent searches (about 20,000 in 2010).[61] With the help of those authors and the Illinois DOT, I obtained computerized records of the stop statistics collected since 2004. I compiled a data set that included all stops since the reporting format changed between 2006 and 2007.[62] While some inevitable loss of data occurred in the importing process, the resulting data set contained about 10 million traffic stops. I provide methodological details and sufficient information for replication below.[63]

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time. . . . [I]t . . . provides for data collection." 93 State of Ill. S. Transcript 70 (Mar. 27, 2003) (statement of Sen. Barack Obama), *available at* http://www.ilga.gov/senate/transcripts/strans93/09300027.pdf. There is evidence that Obama introduced the law as a response, at least in part, to the shortcoming of the *Chavez* litigation. *See* Will Guzzardi, *ACLU: Illinois State Police Show Racial Bias in Traffic Stops*, HUFFINGTON POST (June 7, 2011), http://www.huffingtonpost.com/2011/06/07/aclu-illinois-state-polic_n_872586.html (quoting the legal director of the ACLU Illinois as saying that the a result of court's ruling "that there wasn't sufficient data to prove the [*Chavez*] case[,]. . .[was that] then-state senator Barack Obama and others pushed through the [law]").

58 *See Illinois Traffic Stop Study*, ILL. DEP'T OF TRANSP., http://www2.dot.il.gov/rpa2007/splashscreen.aspx (last visited July 17, 2012) (providing traffic-stop data).

59 Alexander Weiss & Dennis P. Rosenbaum, *Illinois Traffic Stops Statistics Act of 2010 Annual Report: Executive Summary*, ILL. DEP'T OF TRANSP. 4 fig.1 (July 1, 2011), *available at* http://www.dot.il.gov/travelstats/2010%20ITSS%20Executive%20Summary.pdf.

60 *See Illinois Traffic Stop Study, 2010*, ILL. DEP'T OF TRANSP., http://www.dot.il.gov/travelstats/2010%20ITSS%20Statewide%20and%20Agency%20Reports.pdf (last visited Aug. 7, 2012) (showing about 880,000 citations for Whites and 480,000 citations for minority motorists in 2010).

61 *See id.* (showing about 11,000 consent searches for Whites and about 9000 consent searches for minority motorists).

62 Traffic-stop forms completed since 2007 include more information than older reporting formats. *Compare* ILL. DEP'T OF TRANSP., RACIAL PROFILING DATABASE FORMAT (2007), *available at* http://www.dot.il.gov/trafficstop/database07.html (describing new data reporting format), *with* ILL. DEP'T OF TRANSP., ILLINOIS TRAFFIC STOP STUDY (2005), *available at* http://www.dot.il.gov/trafficstop/2005summary.pdf (showing that older data collection efforts were more modest).

63 I received the data in very large comma separated (CSV) text files, with some malformations. I used a Java-based computer program to parse the text files, deal with as many malformations as possible, and store the subsequent stops in a database. I dropped stops that could not be parsed from the set (see below). I then used the open-source and commonly accepted statistical program, R, to access the database of 10 million stops through the use of the RMySQL package. *See generally* THE R PROJECT FOR STATISTICAL COMPUTING, http://www.r-project.org/ (last visited May 29, 2012); RMYSQL: R INTERFACE TO THE MYSQL DATABASE, http://cran.r-project.org/web/packages/RMySQL/index.html (last visited May 29, 2012). Finally, I used R to perform the statistical analysis described in this Note. The original CSV files, the computer code used to import them, and the database and resulting database are all on file with the *New York University Law Review*. The entire

While Weiss and Rosenbaum's analysis of the data provided basic tabulations and summaries of the Illinois data, the newer data set I compiled expands on their research in important ways. First, by aggregating all of the stops from the past four years for which data was available, the new data set is better equipped for robust analyses on subsets of the sample that might otherwise be limited by sample-size problems. The new data set also better explains variation in the yearly reports. Second, the new data set includes variables that were not reported in either the original reports or the posted "raw data."[64] For example, the official reports do not discuss the quantities of drugs found, but my analysis includes this variable.[65] Third, organizing data such that each stop is a separate entry in the larger data set allows for confirmation of some of the official reports' findings *and* inclusion of the corresponding levels of statistical significance of differences, which curiously are missing from the official reports.[66] Finally, the construction and public availability of the data in this format allows future researchers to replicate this Note's statistical methods or design new methods for testing other theories.[67] For example, while this Note opted not to employ regression analysis (given the relative advantages that the KTP test offers over regression),[68] aggregating the data and

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process can be replicated by downloading the CSV files and running the Java program. I was forced to drop some stop data due to poor formatting. For the worst year, 2006, I was unable to import about 15,000 of the over 2 million stops. In more recent years like 2009 and 2010, when data entry appears to have been more uniform, I dropped only about 4000 cases per year. I also dropped some stops when I inserted the data into an table, *see supra*, but in minimal amounts only (about 50 stops per year). Finally, I dropped 57,227 stops from 2007 because Chicago reported some of their stops using the old format. While dropping so many stops from a potentially non-average jurisdiction is problematic, I was able to import about 150,000 Chicago stops from 2007. And although I acknowledge room for reasonable disagreement, I concluded that the value of adding another year (and 2.5 million stops) to the data set exceeded the potential cost of the exclusion bias.

64 *See* ILL. DEP'T OF TRANSP., *supra* note 58 (providing a link to the raw data).

65 *Compare*, *e.g.*, ALEXANDER WEISS & DENNIS P. ROSENBAUM, THE UNIV. OF ILL. AT CHI. CTR. FOR RESEARCH IN LAW AND JUSTICE, ILLINOIS TRAFFIC STOPS STATISTICS ACT 2010 ANNUAL REPORT: EXECUTIVE SUMMARY (2011) (making no mention of drug quantities), *available at* http://www.dot.il.gov/travelstats/2010%20ITSS%20Executive%20Summary.pdf, *with infra* Part II.C (showing drug-quantity analysis). The quantities of drugs found were in the data sent to me by the Illinois Department of Transporation, but are not published online.

66 *Compare* WEISS & ROSENBAUM, *supra* note 65 (showing no tests of statistical significance), *with* ENGEL ET AL., *supra* note 44 (showing tests of statistical significance and p-values, the odds of obtaining the result by mere chance).

67 For instructions on how to access the data, see *supra* note 63.

68 *See* text accompanying *supra* notes 44–48 (discussing the comparative advantages of the KTP test over regression analysis in explaining traffic stops).

publishing it in this format allows future researchers to employ the regression technique.[69]

B. *Findings from Illinois Data*

TABLE 1:

Race:	Hit Rate During All Searches (n = 498,680)	Hit Rate During Non-Discretionary Searches[70] (n = 121,471)	Hit Rate During Consent Searches (n = 79,1070)
Whites	23.8%***	36.5%***	24.6%***
Non-Whites	15.1%***	26.6%***	16.2%***
Blacks	19.1%***	29.5%***	18.2%***
Hispanics	10.2%***	21.0%***	13.2%***
Asians	11.2%***	21.5%***	12.6%***
Native Americans	13.9%***	26.9%	19.1%

***Represents that the difference in mean is statistically significant from the comparison group (all stops not of the given race) at level $p < .001$

The second column of Table 1, "Hit Rate During All Searches," shows the primary empirical contribution of this Note: There are statistically significant differences in the hit rates across every racial group studied in the Illinois data, except for Native Americans. Interestingly, the data suggests the largest amount of profiling in Illinois is targeted not against Blacks but rather against Hispanics and Asians. This result appears even more intriguing in light of the fact that KTP's original paper, focusing on data gathered in Maryland, reported discrimination against Hispanics but cautioned against crediting the result because of the small sample size.[71] For the purpose of determining the statistical significance of the hit-rate differential, I

[69] *See* ENGEL ET AL., *supra* note 44 (looking at only one agency and many fewer stops); Schafer et al., *supra* note 51, at 194 (looking at only 61,000 stops).

[70] I describe the definition and inclusion of "Non-Discretionary Searches" in the text accompanying *infra* notes 77–80. Miscoding in the raw data set led me to drop some field reports that had conflicting information about the basis for a search. For more information, see *supra* note 63 and accompanying text (describing the data importation procedures and providing the internet link to detailed instructions of database notes). I dropped about 50,000 out of 430,000 searches. Omitting problematic data points from column 2, "Hit Rate During All Searches" would be 25.8%; 15.4%; 20%; 10.5%; 11.4%; and 15%. The 20% figure for Blacks has a greatly reduced p-value, moving from .00018 if problematic data points are included to .073 if they are all removed.

[71] *See* KTP, *Racial Bias*, *supra* note 13, at 227 ("We regard our results concerning Hispanics as only suggestive because our data set contains so few Hispanics. Further investigation is needed with a larger data set.").

treated each racial group's overall hit rate as a mean, and performed a difference of means test (t-test). The t-test compares the hit rates of stops including members of the group under study with the hit rates of all stops not including the members of that group.[72] In Table 1, I used a cut-off of .001, meaning that the odds of any relationship reported as statistically significant occurring by chance is less than one in one thousand. But the actual p-values[73] in most of the tests were astonishingly low, indicating that the odds of those differences in hit rate occurring by chance is less than one in a billion.[74] The statistical differences across racial groups reflected in Table 1 show that Illinois law enforcement agencies fail the KTP bright-line test for discriminatory racial profiling. As mentioned earlier,[75] the KTP test can be seen as a discriminatory racial-profiling baseline: Traffic-stop data that passes the KTP test could still reflect undesirable racial profiling tendencies, according to some advocates. But when a set of traffic-stop data fails the KTP test, as the Illinois traffic-stop data does, even the economists that support "efficient" racial profiling must concede that law enforcement is engaging in impermissible, discriminatory racial profiling.[76] This conclusion follows from the fact that Illinois law enforcement officers could increase their efficiency by searching groups with low hit rates (like Hispanics [10.2% hit rate], Asians [11.2%], and Blacks [19.1%]) less often, and instead devoting search resources to groups with high hit rates (like Whites [23.8%]). I discuss the legal implications of this conclusion in Part III.

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[72] Readers might wonder why the difference of means test compared Blacks to non-Blacks (non-Blacks, as a group, include other minorities as well as Whites) instead of examining the difference in hit rates between Blacks and Whites. A few concerns animated this methodological decision. First, as a practical matter, the vast majority of stops in the comparison groups (i.e., non-Hispanics, non-Blacks) *are* stops of White drivers, so this choice only slightly affects the results. Second, I believe that the formal theory of the KTP model points in this direction: The efficiency of police officer behavior should be shown by an equal hit rate across *all* racial groups in *all* stops, not just one racial group as compared to Whites. KTP's article goes in a slightly different direction: They classify all of the stops as containing either a Black or White motorist. *See id.* at 209–10 (discussing only White and Black motorists in the formal definition of their model). I suspect that KTP would have adopted my approach if their data had more instances of other racial groups, like Hispanics or Asians, as did the Illinois traffic stop data. *See id.* at 226 (stating that KTP did not believe they had enough Hispanic data to reach conclusions).

[73] For an explanation of p-values, see *supra* note 66.

[74] The most common social science convention is to report only p-values under .05 (the odds of the statistical relationship appearing by mere chance is less than one-in-twenty) as statistically significant. The p-values in the Illinois data set were much lower, around $1*10^{11}$ in some instances.

[75] *See supra* note 38 and accompanying text.

[76] *Cf.* KTP, *Racial Bias*, *supra* note 13, at 227 ("Our equilibrium model of police and motorist behavior provides a test for whether racial disparities in motor vehicle searches reflect prejudice . . . .").

Table 1 also includes two other columns: Hit rates during non-discretionary searches and hit rates during consent searches. These columns add detail to the profiling picture. Previous analyses of traffic-stop data categorized searches by the degree of discretion afforded to the officer.[77] For example, an Arizona report classified searches by three different degrees of discretion: mandatory (little to no discretion for officers), discretionary (guided by case law with medium discretion), and consent only (the highest degree of discretion).[78] I adopted a modified version of this framework and coded the searches as either discretionary[79] or non-discretionary.[80] I further subdivided the category of discretionary searches into consent searches and non-consent discretionary searches.[81] Consent searches are a hot topic in Fourth Amendment legal literature because they are widespread and because they represent a high-water mark of officer discretion.[82] Thus, columns 3 and 4 offer a contrast: They compare the hit rates for searches that occur in non-discretionary contexts (column 3) with the hit rates for consent searches, the most discretionary context of all (column 4).

Across all racial groups, the results for non-discretionary searches (column 3) are consistent with an intuitive story: As the context of the search becomes more discretionary, the efficacy of the search decreases. Non-discretionary searches have the highest hit rates: A non-discretionary search of a member of any given race is between 50% and 80% more successful than a consent search of a member of that same race. However, somewhat surprising is that the hit rates for consent searches (column 4) are, depending on the race, either not much lower, or even a bit higher, than the overall hit rate for the same

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77 *See* Schafer et al., *supra* note 51, at 196 (describing the coding of discretionary and non-discretionary searches).

78 ENGEL ET AL., *supra* note 44, at 118.

79 Discretionary searches are those where the reported rationale of the vehicle search is listed in the traffic-stop data as: consent; probable cause; drug dog alert; reasonable suspicion; or a *Terry* stop.

80 Non-discretionary searches are those where the reported rationale of the vehicle search type was a search incident to arrest.

81 The category of non-consent discretionary searches does not appear in Table 1.

82 *See, e.g.*, Korobkin, *supra* note 24, at 77 (stating that "[c]onsent searches represent the majority of searches during traffic stops" because they are discretionary and require no probable cause); Ric Simmons, *Not "Voluntary" but Still Reasonable: A New Paradigm for Understanding the Consent Searches Doctrine*, 80 IND. L.J. 773, 773 (2005) ("Over 90% of warrantless police searches are accomplished through the use of the consent exception to the Fourth Amendment."); Daniel J. Steinbock, *The Wrong Line Between Freedom and Restraint: The Unreality, Obscurity, and Incivility of the Fourth Amendment Consensual Encounter Doctrine*, 38 SAN DIEGO L. REV. 507, 537 (2001) (observing that officer discretion is "essentially unfettered" in consent-search encounters).

race (column 2). This suggests that, despite the discretionary context, a consent search is about as efficacious as the average search.

While the original KTP paper did not divide searches by level of discretion, my analysis offers a more complete picture of hit-rate discrepancies. Since commentators cite officer discretion as a factor in racial profiling,[83] one might hypothesize a finding of relatively equal hit rates in non-discretionary searches, but disparate hit rates in discretionary search contexts. Surprisingly, at least one early traffic-stop study made the counterintuitive finding that, while minorities "were more likely to be searched when stopped by the police . . . these searches were more likely to be nondiscretionary in nature."[84]

The Illinois data confirms neither of the obvious hypotheses. On one hand, the hit-rate ratio between Whites and Hispanics for all searches is 2.3 (Whites have over twice the hit rate as Hispanics), while the ratio for non-discretionary searches is 1.74 (Whites have a bit less than twice the hit rate as Hispanics in non-discretionary searches). This suggests that Illinois police discriminate against Hispanics (slightly) more in discretionary contexts. On the other hand, the ratios between Whites and Blacks are very similar across all search types: Whites have about 1.25 times the hit rate of Blacks in both non-discretionary searches and all searches. It is difficult to reach a definitive conclusion as to whether there is more discrimination in discretionary versus non-discretionary searches. This may suggest that broad, department-level law enforcement policies are just as much a cause of discriminatory racial profiling as officer discretion.[85]

C. *Are Police Trying to Maximize the Quantity of Drugs Seized?*

One extension of the KTP model asks whether police are maximizing the value of drugs seized or the number of large drug seizures, rather than maximizing just the number of successful searches. Using Maryland traffic-stop data similar to the data used by KTP, two other researchers tested this hypothesis. Gross and Barnes found that racial profiling may increase the probability of large drug finds. The Maryland data showed that substantial quatities of drugs were more likely to be found among Black and Hispanic drivers.[86] Whites

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83 *See, e.g.*, Samuel R. Gross & Katherine Y. Barnes, *Road Work: Racial Profiling and Drug Interdiction on the Highway*, 101 MICH. L. REV. 651, 655 (2002) ("Racial profiling depends on police discretion in choosing suspects.").

84 Schafer et al., *supra* note 51, at 200 ("This observation runs counter to traditional thoughts about racial profiling. African American and Hispanic drivers were less likely to be subjected to completely discretionary searches.").

85 *See* Korobkin, *supra* note 24, at 5 (discussing how racial profiling can either occur at the discretionary level or result from department-wide policies).

86 Gross & Barnes, *supra* note 83, at 660.

appeared to carry smaller quantities of drugs compared to minority groups.[87]

Becker notes that while "Whites had almost uniformly the highest [hit] rates" (suggesting discrimination against minority drivers according to the traditional KTP test), this simple finding obscures the more complex picture of quantity-based drug finds.[88] Whites had higher hit rates in marijuana finds, but the odds of finding large quantities of drugs were highest when searching minorities, especially Blacks. After imputing street values for the drug types and quantities, the paper found that the average value of a drug find "varied widely by race of the driver with an average of $50 for Whites, $1,700 for African Americans, and $4,400 for Hispanics."[89] Summarizing the results, Becker writes that an inefficient amount of profiling occurred only if the goal was to maximize the number of drug finds.[90] If, on the other hand, police were trying to maximize the amount of hard drugs or maximize the expected street value of the hits, then officers should have targeted minorities *more*.[91]

I tested this theory with the Illinois data and asked whether the drug-quantity hit rates were equal across racial groups. The application was imperfect because the Illinois data did not specify the drug type seized or the exact quantity; nonetheless, the data did provide a range.[92] Table 2 and Table 3, shown in the Appendix, offer basic tabulations of the drug quantities found during stops and the race of the driver during the busts. The simple story is that smaller finds were much more common than large busts.

Table 4 below shows the hit rates for each racial group at different drug-quantity ranges. Percentages in the table reflect the hit rates: given that an officer decided to search someone of a particular race, whether the search would produce drugs of either the specified quantity *or greater*.

The tests performed in Table 4 follow the same basic procedures as the calculations from Table 1[93]: For each level of drug quantity and each race, the question is whether there is a statistically significant difference between the race's hit rate and the hit rate of the control

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87 *Id.*

88 Becker, *supra* note 54, at 186.

89 *Id.* at 190.

90 *Id.* at 191.

91 *Id.*

92 There is evidence that the drug-quantity field on the traffic-stop form had a less than 100% completion rate. I found fewer than 46,000 instances where the drug-quantity-seized field had a non-empty response, which is less than the number of entries in the drug hit section. For more information, see text accompanying Table 4, *infra*.

93 *See supra* note 70 and accompanying text.

TABLE 4:

Race:	Hit Rate for <2 Grams	Hit Rate for 2–10 Grams	Hit Rate for 11–50 Grams	Hit Rate for 51–100 Grams	Hit Rate for >100 Grams
Whites	10.4%***	4.3%***	1.1%*	0.2%	0.1%*
Blacks	11.4%***	5.7%***	1.7%***	0.4%***	0.2%***
Hispanics	4.1%***	1.9%***	0.5%***	0.1%***	<0.1%***
Other	6.0%***	3.1%***	0.6%***	0.1%***	0.1%

***Represents that the difference in mean is statistically significant from the control group (all stops not of the given race) at level $p < .001$

*Represents that the difference in mean is statistically significant from the comparison group (all stops not of the given race) at level $p < .05$

group. The control group is the subset of all searches that did not include members of the race being tested.[94] Once again, most comparisons reported statistically significant results at the stringent .001 level but, given that the sample becomes smaller as the domain narrows to only large busts, some results reported in Table 4 meet only the traditional (and more liberal) .05 standard.

The quantity-adjusted hit rates in Table 4 are a stark contrast to the basic hit-rate data presented in Table 1; they question the finding of discriminatory racial profiling. Recall that Table 1 showed a statistically significant disparity in the hit rates for minority motorists: Searches of minorities were less successful than searches of Whites, meaning that the policing practices failed the KTP bright-line test and showed discriminatory racial profiling against minority motorists. In contrast, the quantity-adjusted hit rate table suggests that Black drivers, when searched, are more likely to be found carrying a large amount of drugs than White drivers. Black motorists have a greater hit rate than non-Black motorists at every quantity threshold, and the results are statistically significant. Following the KTP tradition, some might view this as quantitative justification of Illinois's police work: Illinois officers search minorities at disparate rates, but minorities are more likely to carry large quantities of drugs.

But the complete story about the drug-quantity hit rates is more complicated than that simple narrative, which obscures the extent of discriminatory racial profiling against Hispanics. While racial profiling is often seen as a problem concerning Whites and Blacks, the quantity-adjusted table provides further quantitative evidence that police

[94] For a discussion of the control group in the difference of means test, see *supra* note 72.

departments discriminate against Hispanics more than any other ethnic or racial group. Not only did Hispanics have the lowest hit rate,[95] they had a lower hit rate at every quantity threshold. This Note's most emphatic empirical finding is that Illinois law enforcement searches Hispanics in a way that fails the KTP bright-line test and is highly suggestive of racial discrimination against the group.

It is also worth asking whether the differences among some of the groups, while statistically significant, are great enough disparities to be considered practically significant. For example, only at the two-to-ten gram quantity do Blacks have a hit rate that is one percent greater than the corresponding White hit rate, and at no level do Blacks have hit rates two percent greater than the rates of corresponding Whites. While tests of statistical significance are convenient in part because of the resulting definitive conclusions, evaluating whether differences are large enough to be troubling is a murkier task. One could claim from Table 4 that Blacks were twice as likely as Whites, when searched, to be found carrying a large amount of drugs (in excess of 50 grams), and that this justifies the general disparity in hit rates from Table 1. One could also use the two tables to argue that Whites had the greatest overall hit rate, and that large drug finds—rare and unpredictable events—were relatively equal among the races (except for Hispanics, who were subjected to inefficient racial profiling at all levels). Both are accurate descriptions of the data, which is malleable enough to fit either narrative.[96]

III
The State of the Law and Potential Opportunities for Racial Profiling Plaintiffs

A. *Equal Protection Precedent Through* Chavez

By presenting empirical findings before reaching legal conclusions, this Note proceeded in the reverse order of the *Chavez* case and the events that followed. There, the Seventh Circuit's finding that Chavez's empirical evidence was insufficient to sustain an equal protection claim led, in part, to the state law that mandated the collection

95 *See supra* Table 1.

96 The drug-quantity data in Table 4 seems suspiciously inconsistent with the hit-rate data in Table 1. Why did Whites have a greater overall hit rate when Blacks had higher hit rates at every drug-quantity level? One possible explanation is that hits were recorded when searches produced non-drug items like weapons, alcohol, or stolen property. Another explanation is that the traffic-stop data was imperfectly recorded. For over 8000 stops, police reported that they had found drugs but did not list a drug quantity; however, brief analysis did not show a disproportionate number of Whites in the stops that were missing data.

of traffic-stop data.[97] Before addressing whether the new data presented in Part II would remedy the evidentiary weaknesses in the *Chavez* litigation, however, it is helpful to learn more about the Supreme Court's discrimination jurisprudence in the Constitutional setting, as elaborated in *McCleskey v. Kemp*[98] and *United States v. Armstrong*.[99] Those precedents, briefly discussed below, and the Seventh Circuit's application of the doctrine in *Chavez* undoubtedly limit the ability of a litigant to rely solely on statistical evidence when bringing an equal protection claim.

1. McCleskey *and* Armstrong

McCleskey involved an equal protection challenge to the death penalty in Georgia, based on a study that purported to show racial discrimination in the application of capital punishment.[100] In addition to a disparate racial impact claim,[101] McCleskey argued an equal protection violation by both the prosecutors and the jury in his original trial and sentencing. As the Court admitted, it had "accepted statistical disparities as proof of an equal protection violation"[102] in the past in the context of jury venire selection.[103] But the Court distinguished claims dealing with juries and employment[104] from the death penalty setting, holding that the death penalty required a much greater level of discretion for the decisionmakers involved.[105] Given the amount of discretion granted to juries and prosecutors when deciding whether to impose the death penalty, the Court said it would

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97 *See supra* note 57.

98 481 U.S. 279 (1987).

99 517 U.S. 456 (1996).

100 *McCleskey*, 481 U.S. at 286–87.

101 The Supreme Court summarily dismissed a suggestion that Georgia violated the Equal Protection Clause by simply allowing a capital punishment regime to "remain in force despite its allegedly discriminatory application." *Id.* at 297–98. McCleskey would have needed to show that the Georgia legislature enacted the capital punishment policy for the purpose of its racial disparities, rather than "merely 'in spite of'" the discriminatory effects. *Id.* at 298. Analogizing to the racial profiling context, it would be impossible to sustain an equal protection claim against legislatures or law enforcement agencies on the ground that they tacitly endorsed a discriminatory traffic-stop regime. Instead, one would need to show that the policymakers chose a traffic-stop regime specifically "to further a racially discriminatory purpose." *Id.*

102 *Id.* at 293.

103 *Id.* at 293–94 (citing Arlington Heights v. Metro. Hous. Dev. Corp., 429 U.S. 252 (1977)); *see also* Castaneda v. Partida, 430 U.S. 482, 495–96 (1977) (holding that a statistical disparity in jury selection constituted a prima facie case of an equal protection violation, and that the burden shifted to the State to refute the statistical evidence).

104 Plaintiff-employees have long been able to show Title VII violations using statistics and regression analysis. *See McCleskey*, 481 U.S. at 294 ("[T]his Court has accepted statistics . . . to prove statutory violations under Title VII of the Civil Rights Act of 1964.").

105 *Id.* at 297.

"demand exceptionally clear proof" before inferring a discriminatory purpose from statistics alone.[106]

Armstrong was an equal protection case arising out of a raced-based prosecution allegation.[107] In *Armstrong,* the Supreme Court denied a discovery request based on an affidavit stating that all twenty-four defendants prosecuted by the Federal prosecution office in the previous year were Black.[108] The Court unequivocally placed the burden on the defendant to show "clear evidence"[109] of a violation by demonstrating both discriminatory effect and a discriminatory purpose.[110] For an equal protection claim alleging prosecutorial misconduct on the basis of race, this standard requires that "the claimant must show that similarly situated individuals of a different race were not prosecuted;"[111] the affidavit and study submitted by Armstrong's attorney failed to do so.[112] Thus, in the selective prosecution context, at least, the similarly-situated persons test is the key to showing discriminatory effect.[113] *Armstrong* may thus require that a successful racial profiling claim show more than discriminatory impact on minorities: It must also identify specific instances of disparate treatment amongst "similarly situated individuals."[114]

2. Chavez

As previously noted,[115] *Chavez v. Illinois State Police* had an unusual origin: After the police stopped and searched a White motorist, his defense attorney "suspected that state troopers were stopping motorists based on skin tone or travel patterns."[116] The defense attorney hired Peso Chavez to recreate the circumstances of the stop.

[106] *Id. McCleskey* is seen as sharply limiting the use of statistical evidence to infer discriminatory purpose. *See* Melissa Whitney, Note, *The Statistical Evidence of Racial Profiling in Traffic Stops and Searches: Rethinking the Use of Statistics to Prove Discriminatory Intent*, 49 B.C. L. REV. 263, 282, 284 (2008) (citing *McCleskey* as an example of the "nearly insurmountable discriminatory intent requirement . . . [that makes] equal protection claims due to racial profiling virtually illusory").

[107] United States v. Armstrong, 517 U.S. 456, 458 (1996).

[108] *Id.* at 459, 470.

[109] *Id.* at 464 (quoting United States v. Chem. Found., 272 U.S. 1, 14–15 (1926)).

[110] *Id.* at 465.

[111] *Id.*

[112] *Id.* at 470.

[113] For an example of the application of this standard, see *Chavez v. Ill. State Police*, 251 F.3d 612, 636 (7th Cir. 2001), which states: "To prove discriminatory effect, the plaintiffs are required to show that they are members of a protected class, that they are otherwise similarly situated to members of the unprotected class, and that plaintiffs were treated differently from members of the unprotected class."

[114] *Id.*

[115] *See supra* notes 1–4 and accompanying text.

[116] *Chavez*, 251 F.3d at 623.

Chavez drove infraction-free through Illinois but quickly drew the attention of police, who, after a half-hour of surveillance, pulled him over.[117] The police asked for consent to search his vehicle (which Chavez declined to give) and detained him for a canine search (which was also non-consensual).[118] Chavez then brought a claim under the Equal Protection Clause of the Fourteenth Amendment.[119]

The *Chavez* court tackled the equal protection claim by adopting the traditional bifurcated analysis of the Supreme Court in *Armstrong*,[120] requiring plaintiffs to prove both discriminatory effect and discriminatory intent.[121] Citing both *Armstrong*[122] and *McCleskey*,[123] the court held that the plaintiffs' traffic-stop statistics failed to prove either discriminatory effect or discriminatory intent, and affirmed summary judgment against the plaintiff.[124] But the court distinguished the relevance of statistical evidence for a racial profiling claim from the Supreme Court's dismissal of such evidence in *Armstrong*:

> *Armstrong* . . . require[s] a criminal defendant in a selective prosecution case to provide the precise name of a similarly situated defendant who was not prosecuted . . . [but] the rationale behind such a requirement does not apply with equal force in the context of a civil racial profiling claim. . . . [where] the similarly situated

117 *Id.*

118 *Id.* at 624.

119 *Id.* at 621. Given that Chavez's claim arose from a highway traffic search, one might expect that the claim would be litigated under the Fourth Amendment's prohibition against unreasonable searches and seizures. However, the Supreme Court indicated in *Whren v. United States* that "the constitutional basis for objecting to intentionally discriminatory application of laws is the Equal Protection Clause, not the Fourth Amendment." 517 U.S. 806, 813 (1996). Thus, individuals seeking to bring litigation based on racially-motivated policing practices must proceed by alleging a violation of the Equal Protection Clause. *See, e.g.*, David Rudovsky, *Litigating Civil Rights Cases to Reform Racially Biased Criminal Justice Practices*, 39 COLUM. HUM. RTS. L. REV. 97, 108 (2007) ("[T]he Court [in *Whren*] stated that the Equal Protection Clause . . . would prohibit any intentional race discrimination in a car stop. However, this perfunctory statement did not address the means by which an intentional race discrimination claim could be proven . . . ."); Whitney, *supra* note 106, at 280–82 (noting that *Whren* relegated discriminatory policing claims to the Equal Protection Clause where "[t]he nearly insurmountable discriminatory intent requirement . . . mak[es] equal protection claims due to racial profiling virtually illusory"); Nicola Persico, Rational Choice Foundations of Equal Protection in Selective Enforcement: Theory and Evidence 16 (July 2006) (unpublished research paper, University of Pennsylvania) (on file with the *New York University Law Review*) (observing that "*Whren* significantly closed the door on racial profiling suits" by pointing to the Fourteenth rather than the Fourth Amendment).

120 United States v. Armstrong, 517 U.S. 456 (1996).

121 *Chavez*, 251 F.3d at 635–36.

122 *Id.* at 640.

123 *Id.* at 645.

124 *Id.* at 656.

> requirement might be impossible to prove. . . . A second distinction between this case and *Armstrong* is the factual context. . . . The analysis is narrowly focused on the constitutional implications of interfering with the prosecutorial function, a factor . . . not directly at issue in a plaintiff's civil claim of racial profiling.[125]

Thus, one reading of *McCleskey* and *Armstrong* views the Supreme Court's reluctance to use statistical data as motivated in large part by context, including the high degree of discretion that the Court grants prosecutors and juries.[126] While the Supreme Court has not explicitly set the statistical standard for a racial profiling claim, *Chavez* set a workable legal standard in the Seventh Circuit: Statistical evidence, by itself, can prove discriminatory effect in a racial profiling case.[127]

Indeed, the *Chavez* court's problem with the plaintiff's equal protection claim was not the use of statistics itself,[128] but rather the particular statistics that Chavez used: "[T]hough our reasons differ slightly from those of the magistrate judge, it is clear that these statistics cannot satisfy the discriminatory effect element of the plaintiffs' prima facie case—they are simply insufficient as a matter of law."[129]

The court noted several weak aspects of the plaintiffs' statistical evidence. Chavez and other plaintiffs took a random sample from about three hundred field reports and claimed that the sample showed a disproportionate number of field reports for Blacks and Hispanics.[130] The court thought that this sample was neither large enough nor sufficiently random to sustain the allegation.

> There is no indication of the total number of stops this [the sample] is being compared to, thus it is impossible to tell if this sample size is sufficiently large to be reliable . . . . Further, the field reports . . . . [are a] type of non-random sample [that] might undermine the reliability of the statistics.[131]

An additional problem with the statistical evidence was the lack of racial data of the stopped motorists in the larger database of citations.[132] The court also took issue with the population benchmarks—

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[125] *Id.* at 639–40.

[126] *See* Rudovsky, *supra* note 119, at 110–12 (distinguishing potential racial profiling claims directed at policing practices from those arising from prosecutorial behavior in *McCleskey* and *Armstrong*).

[127] *Chavez*, 251 F.3d at 640.

[128] *Id.* at 638 ("While few opinions directly acknowledge that statistics may be used to prove discriminatory effect, the Court has repeatedly relied on statistics to do just that.").

[129] *Id.* at 641.

[130] *Id.* at 642–43.

[131] *Id.* at 643.

[132] *Id.*

an estimate of minority drivers as a percentage of all highway drivers—used by the plaintiffs. The Census data that the plaintiffs used told the court "very little about the numbers of Hispanics and African-Americans driving on Illinois interstate highways,"[133] while a DOT study included estimates that were ill-suited for local and state racial estimates.[134]

The court, especially troubled that the data set from the plaintiffs was small and not representative, suggested the ideal type of statistical evidence for such claims:

> [T]here is no database that tracks every stop, the race of the parties involved, and whether a search took place. This is ultimately the type of information that would be useful in a suit such as this, as it would clearly indicate what percentages of African-American and Hispanic motorists were being stopped and searched on Illinois highways.[135]

By describing what the data-set in *Chavez* was lacking, the court implied some willingness to use statistics when these criteria are met.

B. Evaluating the Hit-Rate Data in Light of Equal Protection Precedent

In many respects, the data from Part II addresses the problems the Seventh Circuit identified with Chavez's statistical evidence, and thus might be used to show discriminatory effect to that court's satisfaction. First, the data presented in Part II—drawn from the state law mandating data collection from all traffic stops—meets and surpasses the court's hypothetical evidentiary standard. At over nine million stops, the data is certainly "sufficiently large to be reliable."[136] It represents four consecutive years' worth of data but could also be broken into subsets if the plaintiffs or a court wanted to focus on more specific time periods, locations, or demographics. Most importantly, the data represents all of the stops performed in the state of Illinois rather than just a selected sample of certain types of stops.[137]

The new Illinois data in Part II also resolves another thorny issue from *Chavez*[138]: The matter of a suitable population benchmark, which is essentially an estimate of the number of each racial group that exists in the general population or which "should" be stopped in a

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133 *Id.* at 644.
134 *Id.*
135 *Id.* at 642.
136 *Id.* at 643.
137 For a description of the data collected in this project, see *supra* note 58–63 and accompanying text.
138 *See supra* note 125 and accompanying text.

colorblind world. First, by focusing on adverse stop outcomes other than stops themselves (such as searches), one avoids the benchmarking problem altogether. Whether Blacks make up 15% or 30% of the motorists on Illinois highways makes a significant difference in trying to determine how many traffic stops of Blacks would indicate discriminatory racial profiling. But the difference is meaningless if we look at the percent of stops that then result in searches, because there is no reason that the underlying population percentages would affect the decision to search. In essence, the use of the KTP hit-rate test makes the question of population benchmarks irrelevant. The hit-rate hypothesis says that the *rate* (or percent) of successful searches should be the same for each race, but does not make any prediction about the actual number of searches.

In summary, the data presented in Part II of this Note would likely be sufficient to show discriminatory effect in light of *Chavez*. First, the Seventh Circuit explicitly stated that statistics *could* be used to show discriminatory effect. Second, this improved data source responds to all statistical concerns expressed by the *Chavez* court. Third, the data reflects hit-rate disparities that show discrimination at a statistically significant level.

While the data from Part II may enable a racial profiling plaintiff to establish the discriminatory effect portion of an equal protection claim, a plaintiff must still show that the state acted with the intent to discriminate. The Seventh Circuit suggests that "only in the Title VII or jury venire context" may statistics alone state an equal protection violation by showing both discriminatory effect and discriminatory intent.[139] Legal commentators have made much of the same point, concluding that, "[i]n general, courts reject the use of aggregate population statistics to prove discriminatory intent towards a particular plaintiff."[140] And the Seventh Circuit itself seemingly announced a clear rule that "[i]n this [racial profiling] context, statistics may not be the sole proof of a constitutional violation."[141] As a result, although a litigant may have some success in convincing a court to consider statistical data, these litigants may still need to overcome a significant hurdle to succeed in a racial profiling claim.

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[139] *Id.* at 640.

[140] Whitney, *supra* note 106, at 283; *see also* Korobkin, *supra* note 24, at 51 ("[A] study . . . showing that minorities are statistically more likely to be stopped and searched than Whites might be one way to establish a disparate *impact* claim, but it does not prove discriminatory intent.").

[141] *Chavez*, 251 F.3d at 648.

C. Potential Uses of the Hit-Rate Data for Future Litigants

Not everyone agrees that hit-rate disparities will fail to satisfy the discriminatory intent prong: Persico, one of the KTP authors, argues that hit-rate data alone could establish both elements of an equal protection claim.[142] In fact, KTP are relatively optimistic about how courts would receive their bright-line test and argue that their test is consistent with the equal protection framework. Persico elaborates on this legal reasoning in a recent working paper.[143] Persico uses Judge Easterbrook's decision in *Anderson v. Cornejo*, a Seventh Circuit case involving allegations of racial profiling during security screening at Chicago O'Hare airport,[144] as evidence of analysis which "is consistent with our [the KTP] model:" "First, Judge Easterbrook declined to use search rates to infer intent to discriminate. Second, he deduced the absence of disparate treatment between different groups from roughly equal hit rates."[145]

Persico also suggests that the *Chavez* opinion might provide support for the hit-rate test, noting that the plaintiffs presented stop-and-search rates (as opposed to KTP-style hit rates), and that the court rejected this type of statistical evidence as failing to prove disparate impact.[146] Persico believes the *Anderson* opinion supports the idea that courts elsewhere would be likely to use (or perhaps already have employed) the KTP test when evaluating allegations of racial profiling.[147]

Unfortunately, Persico's predictions about using the hit-rate data to show discriminatory intent are overly optimistic. His argument overstates the importance of a disparate hit-rate finding and exaggerates the ease of bringing a claim in light of the discriminatory intent

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142 Persico, *supra* note 119, at 7.

143 *Id.* at 16–25.

144 355 F.3d 1021, 1022 (7th Cir. 2004).

145 Persico, *supra* note 119, at 24.

146 *Id.* at 22. Persico also argues that *Chavez* is consistent with the KTP, *Racial Bias* model.

> [T]he court [referred] to statistics that are more closely analogous to search rates rather than hit rates, which we argue below are not informative about intent to discriminate. Thus, the court in Chavez correctly (in our view) rejected an attempt to prove intentional discrimination by means of statistics that were probative of disparate impact alone.

Id. at 22. *But see* KTP, *Racial Bias*, *supra* note 13, at 207–08 (recognizing that the law on racial profiling claims is "not clear-cut," and only devoting a cursory one page to legal analysis while failing to mention the Equal Protection Clause, suggesting that the focus of KTP's work was the economic rather than legal implication).

147 Persico, *supra* note 119, at 7 ("[O]ur analysis provides a bright-line test that faithfully interprets the spirit of the current application of the McCleskey standard and dovetails with the most recent judicial approach (Anderson).").

requirement. The chief problem with the theories advanced by both Persico alone and by KTP as a unit is that they seem to believe that the hit-rate test provides statistical evidence well-suited for showing discriminatory intent[148] but fail to fully elaborate on why this is. A showing that a jurisdiction failed the KTP hit-rate test, like the one made in Part II of this Note, only shows that the police acted irrationally at large; it says nothing about the individual plaintiff in a given case. The KTP test does not solve the general problem of using macro-level aggregate data to infer discriminatory intent for each plaintiff's particular micro-level interaction with law enforcement.

Though litigants will probably not be able to prevail on an equal protection claim using traffic-stop data alone, the data could still be useful to plaintiffs in several ways. First, the data could be used to show discriminatory effect, and plaintiffs could then supplement the data with non-statistical evidence like the racial profiling anecdotes that courts have historically looked to for evidence of discriminatory intent.[149] Second, the probative value of the statistical evidence may convince a court to impose a lower burden of proof on the plaintiff with regard to discriminatory intent, at least in the context of a civil racial profiling claim. Finally, the statistical disparities reflected in the data may be significant enough to create a presumption of discriminatory intent.

Plaintiffs could rely on the hit-rate data to prove discriminatory effect and then offer anecdotal evidence to satisfy the discriminatory intent prong. As discussed in Part III.A, *Chavez* implicitly endorses the use of separate evidence to show discriminatory effect and discriminatory intent. Even assuming that courts require plaintiffs to produce individualized evidence of discriminatory intent, many plaintiffs will probably be able to meet that burden. With nearly three million traffic stops in Illinois annually, it is likely that many minority drivers experience discrimination every day and that law enforcement officers reveal discriminatory intent in at least some of these interactions. For example, while the *Chavez* court played down a comment by the officers that "one can never tell with 'you people,'"[150] it also stated that "that the use of racially derogatory language is [not]

[148] *Id.* at 25.

[149] The court in *Chavez* discussed a racially insensitive remark that one of the officers purportedly made towards Chavez during the stop. *Chavez*, 251 F.3d at 646. While the court found that Chavez's specific evidence was insufficient, the court's discussion suggests that it would have been willing to accept statistical evidence as proof of discriminatory effect had it been accompanied by stronger anecdotal evidence of discriminatory intent.

[150] *Chavez*, 251 F.3d at 646.

without legal significance" and suggested that such language "is strong evidence of racial animus."[151]

Plaintiffs may also be able to use the hit-rate data to lower their burden of proof on the discriminatory intent prong by making a showing of significant discriminatory effect. The Seventh Circuit explicitly stated that statistical data is more probative in a civil racial profiling claim than in criminal cases or claims that allege prosecutorial misconduct.[152] While this statement appeared in the court's discussion of discriminatory effect, a reasonable reading could indicate a willingness by courts to lower the discriminatory intent bar as well. Even if evidence of discriminatory intent is minimal, a court might accord such evidence more weight if the plaintiff's statistical evidence of discriminatory effect is stronger than it was in *Chavez*. In this scenario, a litigant's use of the macro-level data from Part II to make a convincing showing of discriminatory effect could encourage the court to scrutinize more closely anecdotes from individual citizen-police encounters that are suggestive of discriminatory intent. Thus, the possibility remains that a litigant advancing a civil racial profiling claim who was exposed to a minimal level of derogatory language may be able to use the data from Part II to show discriminatory effect and obtain a relatively lower burden of proof on discriminatory intent.

Finally, a limited body of case law supports the proposition that sufficiently large statistical disparities may create a presumption of discriminatory intent. In *McCleskey*, the Supreme Court conceded that "stark" patterns may serve "as the sole proof of discriminatory intent."[153] More recently, a California district court, echoing much of *Chavez*'s language that distinguished judicial treatment of police officer behavior from the deference afforded to prosecutors in *Armstrong*, observed that statistical evidence of profiling on highways could "support an inference of discriminatory intent."[154] For a plaintiff relying on these cases, the problem is the lack of a clear standard for which statistical disparities, by themselves, are sufficient to serve as prima facie evidence of discriminatory intent: When employing the hit-rate test, is the twofold disparity between Hispanics and Whites enough?[155] What about a threefold difference?

151 *Id.*

152 *Id.* at 639 ("[T]he rationale [in *Armstrong*] behind [the strict similarly-situated person requirement] does not apply with equal force in the context of a civil racial profiling claim.").

153 McCleskey v. Kemp, 481 U.S. 279, 293 (1987).

154 Rodriguez v. Cal. Highway Patrol, 89 F. Supp. 2d 1131, 1141 (2000).

155 *See* Table 1, *supra*.

D. Alternative Strategies for Victims of Racial Profiling

Although victims of racial profiling may be able to rely on the hit-rate data to satisfy part of their burden on an equal protection claim, the hit-rate data alone will almost certainly be insufficient to establish both discriminatory effect and intent. As a result, victims and advocates may wish to pursue alternative strategies in addition to or in place of litigation.

One option is to seek relief from the state legislature, which has the power to pass a data collection act to ease a plaintiff's burden. The Illinois state legislature has amended the Traffic Stop Collection Act three times since 2003,[156] suggesting that the state legislature is responsive to the concerns of victims of racial profiling. Additionally, public disclosure of information like the Illinois traffic data may lead to public shaming followed by reform through political channels. It is interesting to note that the release of annual reports[157] in Illinois often leads to newspaper articles discussing racial profiling.[158] And in New York, the publication of data on stop-and-frisk practices led to significant media attention and calls for reform from political figures.[159] Of course, given the infancy of public data on stopping and searching practices,[160] it remains to be seen whether mere publication of disparities will lead to substantial political change.

Nor is it immediately clear which specific, additional amendments advocates should encourage their representatives to pass. The obvious choice is an amendment mandating equal hit rates across races. But officers, who would probably know if their hit rates were lower for minorities, could easily game such a system: If a search of a minority was unsuccessful, the officer could simply abstain from recording the search. Thus, the denominator for minority searches would decrease on paper even though the discrimination would remain in practice. As noted in Part II.A, data collection already yields less than 100% accu-

[156] *See supra* note 57 (noting that the Act was amended in 2007, 2010, and 2012).

[157] For an example of a publically available report of Illinois traffic-stop data, see WEISS & ROSENBAUM, *supra* note 65.

[158] *See, e.g.*, Art Golab, *Minorities More Likely to Get Tickets*, CHI. SUN-TIMES, July 14, 2011, at 3.

[159] John Eligon, *Taking On Police Tactic, Critics Hit Racial Divide*, N.Y. TIMES, Mar. 23, 2012, at A1 ("Black and Latino lawmakers, fed up over the frequency with which New York City police officers are stopping and frisking minority men, are battling what they say is a racial divide as they push legislation to rein in the practice.").

[160] *See supra* note 20 and accompanying text.

racy,[161] and it seems unlikely that motorists would follow up to ensure that searches of their vehicles were accurately reported.

I instead advocate for an arguably simpler remedy: Ask executive agencies to incentivize success. Consider two officers, one who profiles in a racially discriminatory manner, and one who does not. The beauty of the KTP theory is that we know the officer who profiles in a racially discriminatory way will perform more searches of minorities with less cause and, therefore, have a lower overall hit rate. The use of performance-based incentives, such as promotions based on officers' hit rates and efficacy, could eliminate discrimination by taking advantage of the same mechanism that discourages racial discrimination in the corporate context: the desire to remain competitive.[162] Because the collection forms record the length of a traffic stop, this variable could be used as a check on cheating or lying by officers.[163] If an officer has abnormally long stops that do not result in a search being recorded, that officer could be flagged and the stops reviewed to ensure that the officer is not lying about whether searches were completed in order to boost his hit rates.

Looking to the agencies themselves as the ultimate remedy for discriminatory racial profiling highlights both the advantages and the pitfalls of the KTP methodology. With its efficiency-based rationale, KTP may misunderstand the true discomfort that many legal scholars and practitioners have with racial profiling. But by providing a baseline in the form of a results-based test, KTP may also be capable of aligning the incentives of law enforcement managers and advocates. The adversarial approach to eliminating discriminatory racial profiling, as epitomized by the trying of an equal protection claim in court, remains a difficult battle to win. Yet a collaborative approach that encourages advocates and law enforcement agencies to agree on hit-rate targets and relative equality across races might prove more successful.

Conclusion

At first, this Note may seem to present a pessimistic view of prospects for attaining racial equality in the law enforcement context. The

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161 *See supra* notes 4 (describing officers misreporting a motorist's race), 63, 92 (describing instances where data had to be discarded due, in part, to incomplete data forms).

162 *See, e.g.*, Gary S. Becker, The Economics of Discrimination 43–44 (2d ed., 1971) (suggesting that firms in competitive markets that do not racially discriminate will be more profitable than those that do).

163 Because other evidence (i.e., work logs, schedules, and dispatcher communications) also suggests typical stop lengths, it would follow that it would be harder (though not impossible) to lie about the stop length than about whether a search took place.

statistical evidence shows that, even utilizing a minimalist test of racial profiling that permits profiling as long as it is efficient, Illinois traffic-stop data indicates discriminatory racial profiling. Each test showed evidence of discriminatory racial profiling generally, and evidence of more profiling against Hispanics in particular—a group sometimes excluded from the Black-White racial profiling narrative—than against any other race including Blacks.[164] The legal analysis that followed doubted that this statistical evidence alone could sustain a successful equal protection claim given Supreme Court and Seventh Circuit precedents.

But advocates of racial equality have reasons to remain optimistic. The Illinois traffic-stop data will likely allow litigants to satisfy the discriminatory effect prong of an equal protection claim, giving them the opportunity to put forward anecdotal evidence of discrimination to establish discriminatory intent. Some courts may find the traffic-stop data sufficiently convincing with regard to discriminatory effect such that they will be willing to lower the plaintiffs' burden of proof on discriminatory intent. Additionally, at some level of statistical significance, courts might even step outside the normal requirements of discriminatory intent and either give plaintiffs the benefit of a presumption of discriminatory intent or infer the existence of such intent from the statistical evidence. Perhaps the data presented in Part II—showing disparate hit rates for Blacks but arguably at efficient levels of drug-quantity maximization[165]—is not the ideal data advocates would hope for, yet the Illinois experience presents reasons for optimism. The Illinois legislature appears uniquely attuned to the Traffic Stop Collection Act,[166] and the Illinois ACLU appears dedicated to challenging highway inequality.[167] Most significantly, the KTP test provides a potentially consensus-building, baseline test for racial profiling that can be used in other jurisdictions as more traffic-stop data collections become available.

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164 Korobkin, *supra* note 24, at 3 (noting that public attention is drawn to claims of profiling against "young black men").

165 Part II.C.

166 *See supra* note 57 (noting the three amendments to the Act since it was first introduced).

167 *See, e.g.*, Press Release, ACLU, ACLU of Illinois Files Complaint with U.S. Department of Justice Requesting Investigation of Persistent Racial Bias in Illinois State Police Consent Searches (June 7, 2011), *available at* http://www.aclu.org/racial-justice/aclu-illinois-files-complaint-us-department-justice-requesting-investigation (requesting that the Illinois State Police troopers be barred from consent searches due to the possible coercive nature of obtaining consent).

Appendix

Table 2:

Drug Quantity Found:	Number of Stops:	Percent of All Stops (n=9,382,371)
No drugs found	9,336,621	99.5%
Less than 2 grams	25,072	0.27%
2–10 grams	15,131	0.16%
11–50 grams	4,278	0.05%
51–100 grams	515	0.005%
More than 100 grams	754	0.008%

Table 3:

Drug Quantity Found	African American	Hispanic	White	Other	Total Number:
Less than 2 grams	8874 (35.4%)	2796 (11.2%)	13206 (52.7%)	196 (0.8%)	25,072
2–10 grams	6295 (41.6%)	1744 (11.5%)	6918 (45.7%)	174 (1.1%)	15,131
11–50 grams	2014 (47.1%)	441 (10.3%)	1789 (41.8%)	34 (0.8%)	4,278
51–100 grams	235 (45.6%)	57 (11%)	223 (43.3%)	0 (0%)	515
More than 100 grams	330 (43.8%)	120 (15.9%)	295 (39.1%)	9 (1.2%)	754