Are blacks egregious speeding violators at extraordinary rates in New Jersey?†

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In 1996, a New Jersey Court found that the New Jersey State Police engaged in targeting black motorists on the New Jersey Turnpike (NJT), intensifying the debate around racial profiling. Two recent articles have claimed that the standard of comparison for determining racial profiling was incorrect because either the measurements utilized to make that determination or the standard used in Soto were wrong. The present article concludes that the measures used in the Soto case were valid and reliable. It presents two experiments that show that the suggestion that blacks are stopped at about the correct rate on the NJT because they egregiously violate speed laws much more frequently than do whites is erroneous. The data are consistent with the use of racially informed traffic stops as a pretext for drug searches on the southern end of NJT.

Keywords: racial disparity; differential enforcement.

1. Introduction

For black residents of New Jersey and the nation, the case of New Jersey v. Pedro Soto, et al.1 was a seminal event that condemned the practice of targeting blacks on the New Jersey Turnpike (NJT). Judge Robert E. Francis summed up his findings in the following manner:

Here, defendants have proven at least a de facto policy on the part of the State Police out of the Moorestown Station of targeting blacks for investigation and arrest between April 1988 and May 1991 both south of Exit 1 and between Exits 1 and 7A of the Turnpike.

Following that ruling there has been a large amount of litigation in New Jersey that resulted in the dismissal of numerous prosecutions of black motorists whose initial traffic stops had been the result of improper use of race. A number of civil cases were filed against the New Jersey State Police (NJSP) which has resulted in the State paying out over 19 million dollars.2 In 1999, the

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State Attorney General issued a report that not only admitted racial profiling by the NJSP\(^3\) but also commissioned a study to determine if black motorists were stopped more often than their presence in the population of motorists on the NJT violating traffic laws would suggest because they violated the speeding law more egregiously than did non-black motorists. Further, at about the same time the State of New Jersey entered into a consent decree with the United States Department of Justice (DOJ) which did not formally admit racial targeting but did mandate monitoring of the NJSP by a court-appointed monitor. Most recently, the Governor of New Jersey appointed an Advisory Committee on Police Standards which was, among other things, tasked with advising the Governor about whether the monitoring of the NJSP should be discontinued.

The Soto decision was followed by a number of articles, commentaries, polemics and even diatribes about the decision.\(^4\) Two of these are particularly substantive, so we concentrate on them in this article. The first, Driving While Black: A Skeptical Response, by Stephan Michelson,\(^5\) excoriated, to a greater or lesser degree, all three experts who testified at the hearing, the Judge who conducted the hearing and his opinion and a reviewer for Jurimetrics, among others. Since Michelson argues that the kind of data available to Judge Francis were insufficient to reach the decision that he did, it seems important to discuss a proper standard for a Court to use in reaching a decision when scientific data are at the heart of the argument (Section 2). It is also important to correct a misimpression left by Michelson about the effect of the supposed inability of police officers to determine the race of the motorist prior to the stop. While that old (and for the police, self-serving) argument is often repeated, there is little evidence to support it and evidence produced in the Soto case to dispute it.\(^6\) In addition, counterintuitively, the extent to which the police sometimes cannot identify the race of the driver before a stop increases the estimate of the extent of racially disproportionate stops when they can identify the driver’s race (Section 4).

The second article we address here approached the issue from another angle, collecting data on motorists who were egregious speeders and suggesting that the Soto decision rested on assumptions that were erroneous. That is, concluded Lange \textit{et al.}:\(^7\)

Specifically the proportion of speeding drivers who were identified as Black mirrored the proportion of Black drivers who were stopped by the police.

Interestingly, both articles conclude that the Soto opinion was incorrect because the comparison or benchmark population to which the proportion of black motorists stopped was compared was faulty. They both argue that the correct standard is egregious violation, specified by Lange \textit{et al.} as

\(^3\) http://www.state.nj.us/lps/itm419.pdf


\(^6\) His (States expert) premise that the State Police generally cannot recognize race at night, however, is belied by the evidence. On 16 July 1994, between 9:40 p.m. and 11:00 p.m. Ahmad S. Corbitt, now an assistant deputy public defender, together with Investigator Minor of the Office of the Public Defender drove on the Turnpike at 55 mph for a while and parked perpendicular to the Turnpike at a rest stop for a while to see if they could make out the races of occupants of the vehicles they observed. Mr Corbitt testified that the two could identify blacks versus whites about 80% of the time in the moving mode and close to 100% of the time in the stationary mode. Opinion in \textit{State v. Soto} 324 N.J.Super. 66, 734 A.2d 350 (L.Div. 1996).

driving at least 15 mph over the speed limit. Had such a measure been used, both argue, there would have been no claim of differential enforcement. This paper points out limitations in Michelson’s analysis and reports on a pair of studies that were undertaken in an attempt to shed more light on the subject of the race of egregious speeders (Section 3).

2. What standard should courts use in deciding cases alleging differential enforcement?

A court has to decide cases on the basis of the evidence provided to it. If the standards for proof of differential enforcement are too high, it may be impossible, as a practical matter, for minorities to enforce their constitutional and statutory rights. On the other hand, if they are too low, non-meritorious claims may be upheld, embarrassing the police and possibly contributing to an atmosphere of lawlessness. Balancing these type I and type II errors are what courts are entrusted to do.

In the Soto case, the violator survey indicated that about 15% of the cars that were driving at least 4 mph above the speed limit on the southern end of the NJT had a black occupant, while 46.2% of the race-identified stops were of blacks. The large disparity between these two numbers was a central feature of Judge Francis’ decision in favour of the defense. The state’s expert witness, Dr Leonard Cuppingood, raised the possibility that blacks drove faster than non-blacks, but he had no data to support this possibility. Indeed, four State Troopers testified to the contrary, as Michelson notes (his footnote 23). But Michelson sneers at this evidence (‘Politically correct; yes; but true?’). Does he propose that all four lied about this? Does he propose that Judge Francis should have ruled, with no evidence to support the proposition and four presumably knowledgeable witnesses to the contrary, that because it is possible that blacks drive faster than do whites, that therefore the judge should condone the 15 to 46.2% disparity in the numbers?8

There was other evidence, not remarked upon by Michelson, that corroborated differential enforcement. There were two different State Police units giving tickets on the southern end of the NJT: a radar unit and a patrol unit. Generally, the radar unit has less discretion in whom to stop. However, 19.4% of the tickets issued by the radar unit were to blacks, while 43.8% of the tickets issued by the patrol unit were to blacks. If we take the radar unit’s figure as an estimate of the black proportion of the fastest cars on the road, 19.4% is close to the 15% found in the violator study and very far from the 40+% blacks issued by the State Police patrol unit. These data confirmed the impression that the State Police were enforcing the laws differentially by race. We return to the topic of the appropriate benchmark in Section 3.2 below.

It is also worth noting that after the Soto decision was handed down, the New Jersey Attorney General announced that he would appeal the ruling. Three years later, a week before that appeal was to be heard, the Governor and the Attorney General announced that they would withdraw the appeal because they had done their own study which corroborated the findings of defense’s experts and the court.9

Judge Francis did not have data on the speeds of cars on the NJT differentiated by race. We now have such information, as discussed in Section 3.

8 Lest there be any confusion, the percentages noted here are for race-identified stops. There are slightly different percentages in the Soto opinion with regard to citations, discussed in the next paragraph.

3. Do blacks drive faster than whites?

Recently, there have been claims that blacks are stopped more on highways than are whites because they are much more likely to be in that small percentage of drivers that egregiously violate traffic laws. Lange et al. report that black motorists are almost twice as likely as white motorists to be among those going 15 mph or more over the speed limit. To test this assertion, a study was conducted on the southern end of the NJT and the results were compared to the stops by the NJSP that were available to us.

3.1 Methods

Two traffic surveys of the NJT were carried out during August and September 2005, both were of those motorists violating traffic laws and thus subject to being stopped. The first (vehicle) survey of violators was modelled after the violator survey that was relied upon by the Court in New Jersey v. Soto et al. The survey vehicle drove at a constant 4 mph above the speed limit. For approximately the first 33 miles of the NJT from Exit 1 to Exit 4, the highway is two lanes in each direction and the vehicle stayed in the right-hand lane unless a slow moving vehicle forced a move to the left lane to pass that vehicle. The next 27 miles to Exit 7A is three lanes in each direction and the survey vehicle drove in the centre of the three lanes for this section of the roadway. The surveyors were instructed to drive 4 mph above any reduced speed limit, if possible, for the length of the section of roadway for which the limit was reduced for reasons of construction, traffic congestion and weather. Cruise control was used to maintain the speed of the survey vehicle and before each run the cruise control was calibrated by driving for 1 mile on the highway and timing a measured mile. Observers noted any vehicle that passed them as a violator and any vehicle that they passed as a non-violator, unless they saw another violation in the few moments that the vehicle was in view. If the survey vehicle had to reduce its speed under the specified limits for any reason, observation and recording of vehicles were suspended until it was possible to drive at the prescribed speed. Further, they were instructed to include automobiles, pick-up trucks, sport/utility vehicles, mini-vans, vans and recreational vehicles.

The survey vehicle made 25 trips on the southern end of the NJT between Exits 1 and 7A. Each trip, which took approximately 3 h, consisted of a complete coverage of all turnpike areas between those two exits, including the west spur to the Pennsylvania Turnpike at Exit 6. The days and times of day that the surveyors were on the road were randomly selected, as were the exits from which each survey trip began and the direction in which the survey vehicle started the trip. All days of the week and times of day were included in the possibilities for random selection because (a) the NJSP patrols round the clock and (b) troopers have more time for discretionary traffic control during the times that the roadway is not heavily trafficked. The survey was conducted between 16th August and 28th August 2005. During each of the 25 trips, one person drove and one person noted and
recorded the race/ethnicity of the driver of each vehicle observed, whether the vehicle was a violator or not and also noted each exit on the data sheet as it was passed. The data collectors, who were hired for this project, were not informed about the hypothesis of the study. It is highly improbable that data collectors, even if they knew that there were allegations that the NJSP overstopped black motorists, would have any idea of how many black motorists were expected to be on the southern portion of the NJT.

Since the moving survey methodology was introduced in the Soto case, there have been several researchers who have utilized and/or adopted it for several reasons. Observing the race/ethnicity of a motorist from a vehicle is easier to do than from a stationary point on the roadway because the observer is closer to the observed individual (approximately 10–12 ft) and has more time to make the observation. Observers are trained to first view oncoming vehicles in their rear view inside mirror followed by viewing the vehicle through the exterior rear view mirror and then to view them through the drivers side window if they do not get the race/ethnicity from the mirrors. These observations take less than a second to make. Depending upon the speed of a vehicle approaching the survey vehicle from the rear, the observer has from approximately 3–15 s to make this observation. During this time, the observer has at least three opportunities to observe the race/ethnicity of the driver (from the interior rear view mirror, from the exterior rear view mirror and through the driver’s side window).

There is a question of whether the visual assessment of drivers made from a moving vehicle was reliable. The idea informing this work is an attempt to recreate the conditions under which a police officer decides to stop a vehicle. If troopers are targeting blacks to stop, then they are doing it on the basis of their perception of the driver’s race, from either a moving vehicle or a stationary one by the side of the road. Even if they first saw the vehicle from the side of the road, they have an opportunity to view the driver from their cruiser after catching up with the target vehicle. Our methodology is designed to replicate as well as we can the information available to a trooper making a decision to stop a car. In short, the trooper has the opportunity to determine the race of the driver of the vehicle from a moving vehicle if he/she decides to.

There is information about the concordance of these perceptions with a self-report measure of race. In 1999, we conducted a traffic survey of the entire NJT for another case that ultimately settled. We know what the racial make-up of the southern portion of the NJT was from that research. Utilizing the same methodology used in the vehicle survey described here, the data indicated that there were 15.1% black motorists on the roadway between Exits 1 and 7A. In May 2000, a contract from the State of New Jersey was let to determine the race of the motorists on the NJT. As reported by Lange et al., the study randomly selected vehicles exiting from the NJT and paid the motorists for a short interview which included a self-report of the motorist’s race. This study found that 15.1% of the motorists on the southern portion of the NJT were self described as black. As the standard error for the results of Lange et al. was 2.6% and the standard error for our data was 2%, these data suggest a strong concordance between self-reported race and visually observed race from a moving vehicle.

Prior to the start of the observations, the observers were paired and asked to observe and racially categorize a series of drivers of vehicles while in a car moving at 69 mph on a multilane highway. This interrater reliability test was done to assure that pairs of individuals categorized drivers reliably, for drivers on the southern end of the NJT among these studies that occurred from 1993 to 2000 indicate comparability for these studies.

15 See n. 5.
even though they would be recording observations separately. This assured us that there was general agreement among observers.

The second survey (radar) was conducted to determine the speed of each vehicle seen on the roadway. The observer drove a Toyota Tundra with a Genesis II Select Directional radar from Decatur Electronics mounted in the vehicle. The observer was a former police officer who was certified by the manufacturer on the radar unit. The unit was calibrated in test runs on the NJT in both northerly and southerly directions and on a daily basis prior to the beginning of survey runs. The survey vehicle which proceeded for one complete loop of the Turnpike between Exits 1 and 7A was driven at the posted speed limit. That is, if the speed limit was 65, and normally for this section of the Turnpike it was 65, that was the speed the vehicle was driven. However, if the speed limit dropped below 65 for construction, weather or congestion, the survey vehicle adhered to the reduced speed limit.

There were 40 sessions between 27th August and 23rd September 2005, with each one conducted on randomly selected days and time of day. Each session started and ended at a randomly selected exit and proceeded in a randomly selected direction. The surveyor had the radar unit mounted in the survey vehicle and recorded the speed, race of driver and state of registration for each vehicle encountered.\(^\text{16}\)

Interrater reliability tests were conducted between pairs of observers or between an observer and one of the authors. These tests were conducted to determine what the level of agreement was between observers as to the race/ethnicity of drivers of vehicles. The tests were conducted while driving/riding in a vehicle on a limited access highway at the speed limit. The overall mean of the agreement between raters was 96\%. The goal of the observations is, as closely as possible, to approximate what a police officer sees while patrolling a roadway.

### 3.2 Results

In the vehicle survey, the proportion of vehicles driven by blacks that passed the survey vehicle or were observed to be committing some other violation was 19.0\%.\(^\text{17}\) The speed of violators was not measured in the vehicle survey.

In the radar survey that captured the speed of each vehicle, 18.5\% of the cars that passed the survey vehicle were driven by blacks.\(^\text{18}\) However, neither figure reflects speed bias in reporting.\(^\text{19}\) Speed bias can be adjusted only for the radar survey because we know the speed of the violator only in that case. We recommend that the weighted figures found below be utilized for analysis of the radar study.

As previously noted, the instructions to the observer in the radar study were to drive at the speed limit. Other cars going at the speed limit would not be observed, since they would not pass the observer’s car. Cars going only slightly above the speed limit would be less likely to pass the observer than would cars going substantially above the speed limit. Thus, the probability of being

\(^{16}\) A comparison of data collected in this way with data collected by speed cameras would be interesting. However, no such speed cameras were, or are, installed on the NJT.

\(^{17}\) In the vehicle study, 3028 vehicles were observed, with 96.5\% of them being classified with respect to the race/ethnicity of the driver.

\(^{18}\) In the radar study, 6929 vehicles were observed with 97.8\% of them being classified with respect to the race/ethnicity of the driver.

observed depends on the speed of the violators, which is what is meant by speed bias. (This kind of bias shows up in other contexts and is sometimes known as size-biased sampling. For example, in estimating the extent of undiscovered oil, it is reasonable to suppose that larger fields have a higher probability of already being discovered). The appropriate weighting is derived in Appendix A and is \( T/(S - T) \), where \( T \) is the speed of the observer (which varies with the speed limit) and \( S \) is the measured speed of the violator. Thus, cars for which \( S - T \) is large (i.e. cars driving much faster than the speed limit) are down-weighted relative to slower cars driving over the speed limit. This down weighting of the fastest cars appropriately reflects their greater probability of being observed.\(^{20}\)

These two surveys were undertaken in an attempt to determine whether the NJSP was continuing to stop black motorists at a rate higher than would be expected among violating motorists. To determine the stops of blacks by the NJSP in the area patrolled by the Moorestown station, we consulted the first 11 ‘Semiannual Reports of Aggregate Data Submitted Pursuant to the Consent Decree Entered Into By the United States of America and the State of New Jersey’.\(^{21}\) These reports cover stops by the NJSP for the time period 1 January 2000 to 30 April 2005. From 2000 to 2005 the mean percentage of blacks stopped by the NJSP between Exits 1 and 7A was 30.8 and the latest reporting period was also 30.8, so we accept that proportion as the aggregated estimate.

We compare the 30.8\% number for the Moorestown station with two numbers derived from the radar study. The first looks at the (weighted) proportion of blacks among all violators. In the analysis of all violators, the (weighted) proportion of blacks is 0.177 with a standard deviation of 0.0055. An analysis based on this criterion has been criticized\(^{22}\) because it does not distinguish egregious violators from those driving only slightly above the speed limit. To address this concern we did a separate analysis of egregious violators, those driving at least 15 mph above the speed limit. In the analysis of egregious violators, the proportion of blacks is 0.192 with a standard deviation of 0.014 (the standard deviation is higher for egregious violators because there are fewer of them and hence more uncertainty).

Our findings are summarized in Table 1.

Note that these numbers are roughly consistent. The largest difference in \% black is between the unweighted egregious violators and the weighted all traffic violators and is 1.8\%. To appreciate these numbers, the weighted analyses are relevant to the distribution observed by a stationary observer, while the unweighted ones are those observed by an observer moving at the rate of the observer in the radar study. It is not unreasonable to suppose that traffic stops are undertaken by police officers, some of whom observed while being stationary, while others observed while moving.

\(^{20}\) A referee asked whether the data speak to the argument that blacks may be likely to speed for a longer period of time on the road, and therefore the high proportion of black stops may be an unbiased reflection of ‘opportunities’ that the police had to catch speeders. If blacks speed for longer periods of time, they are more likely to be observed speeding by us and by the police. Thus, if the police are responding to ‘opportunities’ without regard to race, our observed proportions and their ticket proportions should coincide by race.

\(^{21}\) These data are published by the State of New Jersey Department of Law and Public Safety Office of the Attorney General, at website http://www.nj.gov/lps/aggregate-reports.htm. The semi-annual numbers are 29.0, 31.9, 29.7, 32.3, 30.4, 30.5, 29.8, 31.7, 30.1, 32.1 and 30.8.

\(^{22}\) This criticism was made by Dr Leonard Cuppingood in the Soto case. In addition, Lange et al. suggest that black are so heavily overrepresented among egregious speeders that egregious speeding explains the overrepresentation of stops of black motorists along the southern portion of the NJT. Michelson enthusiastically endorses the concept of egregious speeders as constituting the proper benchmark for police stops.
Since all the black proportions in Table 1 are substantially below 30.8% of blacks among stops, we see immediately that there is a potential problem. It appears that although blacks are slightly more numerous among egregious violators than among all violators, the extent to which they are in no way explains the discrepancy between both numbers and the rate of blacks among those stopped.

To understand what these proportions mean, we convert them to a relative risk, as detailed in Appendix B. The relative risk is

\[
\frac{P\{\text{Stop|Black and Violator}\}}{P\{\text{Stop|Not Black and Violator}\}}.
\]

In a system in which race played no role in who was stopped among violators, the ratio would be 1. To the extent that it is greater than 1, the system disadvantages blacks.

Appendix B shows that for all violators the estimated relative risk is 2.07, which is $1.07/0.078 = 13.7$ standard deviations above 1, while for egregious violators, the estimated relative risk is 1.87, which is $0.87/0.169 = 5.15$ standard deviations from 1. Thus, in both cases there is very substantial evidence that blacks have roughly twice the odds of being stopped as do others.

Those results indicate:

1. That blacks are disproportionately stopped at the southern end of turnpike.
2. Among egregious violators, blacks are disproportionately stopped at the southern end of turnpike.

There is no need to restrict the analysis to a single definition of egregious speeding. Figure 1 displays the weighted speeds of motorists going faster than the speed limit, separating blacks from all other drivers. There are three differences between this figure and Fig. 2 of Michelson (2004), one minor difference and two major ones. The minor difference is that our figure excludes drivers who were not speeding. We know that nearly everyone speeds on the turnpike, but we failed to record the speeds of cars going less than the speed limit, and therefore cannot include them in a weighted analysis. The first major difference is that our figure is based on data, while Michelson’s is hypothetical. He made up his figure to illustrate a possible world, while we record the (weighted) frequency of speeds observed. The final difference between the figures is that his figure hypothesizes a quite large difference between the speeds of black drivers and others, while our data show a very small difference. It does appear that blacks drive slightly faster than others, but only slightly. Thus, this figure confirms the rest of our analysis, which shows that the extent to which blacks drive faster than others is not nearly enough to explain the disparity in stops between blacks and others. Figure 1 can also be compared to Fig. 1 of Lange et al. However, their plot is the racial proportions of drivers at various speeds. Consequently at high speeds, they are taking proportions of very few drivers.
As a result, the standard error of the numbers in their Fig. 1 at high speeds will be very high, i.e. the curves they report have large uncertainty at those high speeds.

There are, of course, striking differences in the results of the study by Lange et al. and the present research. The key area of disagreement is the weighted percentage of black motorists who are driving 15 or more mph over the limit. The weighted percentage in the Lange et al. study for black motorists is 26%, while it is 19.2% in the present research. It is instructive first to review methodological differences between the two studies that might impact the results.

3.3 Missing data

The study by Lange used photographs of motorists and found that a great deal of the data were unusable. This is not difficult to understand given the nature of photography on high-speed roadways. Still cameras take a picture and capture what is visible to the lens of the camera (as well as the naked eye) at the split second that the shutter opens and closes. If there were a human observer stationed at the camera position, there would be a longer period of time that the windshield of the car and possibly the occupants would be visible. Glare on the windshield from the sun or from the strobe light might well obscure a photograph for a split second while a human observer could successfully see the race/ethnicity of the occupant as the car moved out of the glare. In a rolling survey of the type utilized in the present research, the observer is much closer to the motorist and has a longer period of time to make the observation than a stationary observer. Thus, there are far fewer missed observations. In the radar study, there were 2.2% missed racial/ethnic determinations made vs. 38% missing data in the study by Lange et al. which covered the entire NJT. On the southern section of the NJT, 51% of the pictures of speeders (15 mph and above the speed limit) were either unreliable
or unusable. At the heart of the results of Lange et al. is the assumption that the missing data are distributed comparably to the observed data. We do know that particularly in the southern section of the turnpike more photographs from the left lane (presumably the high-speed lane) were designated unusable than were photographs from the lanes closer to the camera. Hence, the missing data are not ignorable.

Michelson asserts that the DOJ attempted to suppress the results of the study by Lange et al. The correspondence available to us indicates that DOJ asked for several clarifications and were particularly concerned with the amount of unusable/unreliable data. They asked for a calculation of the outer bounds of the potential impact of the unusable/unreliable data on the survey results by first assuming that all such drivers were white and then assuming all were black. While the State (New Jersey) did not report that the analysis was undertaken, it is possible to present those results here. They indicate that the outer bounds of weighted black egregious speeders on the southern section of the NJT is between 12.8 and 63.5%, based on the Lange data.

3.4 The appropriate benchmark

What then is the appropriate benchmark against which to compare the stops of the NJSP or any other police department?

For New Jersey, the Kennedy Court held that the appropriate benchmark was ‘...the racial composition of the group of persons who violate the traffic laws on roadways patrolled by the State Police. This is the pool of persons from which State Troopers must select violators’ (p.33). Kennedy was the law in New Jersey in the mid 1990s when Soto was heard and continues to be the law today.

Lange et al. suggest that NJSP stops are consistent with their benchmark of egregious speeders. Michelson generally agrees with this argument and goes a bit further. ‘If cars are being legitimately stopped for speeding, they should be going at the fastest speeds we generally find at that time of day. If we are going to apply a selection statistic there should be data on what kinds of people drive at those speeds on that stretch of highway at those times’ (p. 165).

Thus, both Lange et al. and Michelson are arguing that the Kennedy Court’s benchmark should be overturned. Even if we accept the benchmark of Lange et al. of egregious violators (15 mph above the speed limit), the data above still show that blacks are disproportionately stopped. There are several sources of information showing that it is unlikely that the State Police are in fact stopping egregious violators and only egregious violators.

First in the Soto case, NJSP officers testified that blacks and whites drive indistinguishably from each other. More compelling is the evidence that was presented in the case that showed that 63% of stops on the southern end of the NJT were not cited. It is hard to accept that officers are stopping the most egregious violators and then sending them on their way without a citation.

23 Personal communication from Mark B. Johnson. The authors wish to thank Dr Johnson for making data from the study by Lange et al. available to us.
24 Lange et al., p. 206.
25 To quote a letter from Mark Posner of DOJ: ‘Finally, we note that, were the State to proceed with its original plan to publish the survey in its current form, we would insist that all references to our reviewing a draft of the survey be deleted. Those references could be read as indicating that, to the extent the Department had concerns about the survey, those concerns have been answered, which is not accurate. Moreover, the report’s first page should clarify that this study, unlike the previous Turnpike driver’s survey, was conducted at the behest of the State and not pursuant to the Consent Decree’. Letter dated 8 January 2002 from Mark Posner to Anthony Cowell, Deputy Attorney General, State of New Jersey.
Michelson continues his argument as follows:

Nowhere does Lamberth suggest that his New Jersey clients did not possess drugs, nor that they were not speeding. If they were among the fastest cars on the road, they should have no cause to complain about being stopped. They never asserted otherwise. (p.174)

Why would defendants assert otherwise? As explained above New Jersey law permits a stop for any violation of a traffic law. It was and is irrelevant to the lawfulness of their stop, that they were or were not among the fastest cars on the road. Michelson’s standard of egregious violation has no basis in law.

However, we can determine if the defendants in Soto were among the fastest cars on the road. There were 17 defendants in the case, resulting from 11 stops on the NJT. Data on one stop is unavailable to us (the discovery is missing from the client’s long closed file). Of the remaining 10 stops, three were for what would be termed by Lange et al. egregious speeding (15 or more mph above the limit). The speeds over the limit were 23, 20 and 17 mph. Two of the other stops were for speeding, both 10 mph over the limit. The other 5 stops were for (1) crossing over the centre (dividing line between two southbound lanes) and continuing to weave within the lane, (2) vehicle drifted over the right shoulder line and back again abruptly, (3) travelling in the left-hand lane for approximately 3 miles, (4) vehicle slowed to 40 mph and began to drift into the fast lane and 5) driving with high beams on, drifted across centre lane (between two lanes going in the same direction) and passed the police car. Clearly these 10 stops were not primarily because the cars were the fastest on the road.

Cuppingood, Lange et al. and Michelson all have asserted that officers stop primarily or exclusively the most egregious speeders on the roadway and that this group should be the appropriate benchmark. That these speculations are erroneous is attested to by the data that are available with respect to the proportion of cited stops, the testimony of police officers and the proportion of drivers that were stopped among the Soto defendants for other than egregious speeding.

Knowles et al.27 have another take on what the police do or ought to do on the road. Their view is that police are in a game situation against drug transporters. Their argument is that if the marginal rate of successful searches is the same for both racial groups, this would be (non-discriminatory) evidence of maximization of the probability of search success. Not having evidence of marginal rates, they use average rates. In the Soto case, the State Police and the Attorney General vehemently denied use of a drug courier profile including racial variables. In 1999, the Chief of the State Police was fired for publicly defending the use of such a profile.28 The low rate of the issuance of tickets after stops (about 40%),29 together with the fact that the rate of blacks among stops (46.2%) is greater than the rate of blacks among tickets issued by the patrol unit (43.8%) and the radar unit (19.4%), is consistent with the use of racially informed traffic stops as a pretext for drug searches.30 Insight into what was going on during this period inside the New Jersey state government can be gained from the report of the Judiciary Committee of the State Senate (2001).31

29 See Soto decision, n. 6.
31 http://www.njleg.state.nj.us/RacialProfiling/sjufinal.pdf
4. What if police cannot always determine the race of the driver?

Michelson also raises the question of the extent to which police can determine the race of a driver they are about to stop, writing ‘We need to know how well a police officer can make the racial identification that analysts assume he can make’ (p.168). Certainly it is unreasonable to suppose that they can always make such a determination, but equally unreasonable to suppose that they never can. However, as will be seen below, this argument cuts in an unexpected direction.

For the sake of the argument, suppose that 20% of the egregious violators are black, 30% of the stops are of blacks, and that the police can tell the race of the driver half the time. Suppose when the police can tell the race of the driver, \( x \% \) of their stops are of blacks. These assumptions allow us to determine \( x \). Essentially, the assumptions say that half the time, the police draw the race of the stopped driver from the urn that is 20% black and half the time from an urn that is \( x \% \) black. The result is an average of 30% black. Thus,

\[
\frac{1}{2}(20) + \frac{1}{2}(x) = 30
\]

from which \( x = 40 \).

This result is general. Whenever the proportion of blacks among the egregious violators is less than the proportion among those stopped, and some proportion of the stops are race blind, the proportion of blacks among those stopped is a lower bound for the proportion of blacks the police intend to stop, when they have race information. Thus, the proportion of blacks stopped in the Soto case, 46.2%, is less, by an unknown amount depending on how well the police can determine the race of the driver, than the extent to which their practices lead to racially disproportionate stops.

5. Conclusions

Judge Francis wrote in his Soto decision, ‘Once defendants expose a prima facie case of selective enforcement, the State generally cannot rebut it by merely calling attention to possible flaws or unmeasured variables in defendants’ statistics. Rather, the State must introduce specific evidence showing that either there actually are defects which bias the results or the missing factors, when properly organized and accounted for, eliminate or explain the disparity’.

As a referee points out, there may be other factors than race leading the police to stop a car. For example, the police might believe that cars with certain characteristics might be more likely to be involved in illegal activities, and such cars might be more likely to be driven by blacks. However, as Judge Francis pointed out, the establishment of data to support such an argument is the responsibility of the State.

A court cannot anticipate what future research and studies may show. However, it is interesting to address how a similar case with the current data might fare. Our own data (see Table 1) show our best estimate of the proportion of black drivers on the southern end of the NJT to be 17.7%, not far from the 13.5 and 15% figures found in Soto. The proportion of blacks among those stopped on the turnpike has dropped from 35.6 to 30.8%. While this is a moderate drop, there is still more than a 10% disparity.

Does the hypothesis that blacks are more frequently ‘egregious speeders’ than whites explain this disparity? Our best estimate of the proportion of blacks among egregious speeders (those going more than 15 mph above the speed limit) is 19.2%. Thus, while our data do support the conclusion that blacks are somewhat more likely to be egregious speeders than are others, the extent to which this
is true does little to explain the disparity between the proportion of black drivers and the proportion of blacks among those stopped. By contrast with our data, Lange et al. (2005) found that 26% of the egregious violators were black. We believe that the discrepancy is due to the large amount of unusable/unreliable racial identification data in their study.

Our conclusion, then, is that the NJSP since Soto have reduced, but not eliminated, the extent of their differential enforcement of the traffic laws against blacks at the southern end of the NJT. There is more work to be done.

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Appendix A
Imagine a circular track of length $M$ and the observer car is going at speed $T$. Suppose a speeding car going at speed $S > T$ is placed at random on the track. What is the probability that the speeder will overtake the observer? Suppose that the observer starts at the same point $P$ and drives the entire track, which takes time $M/T$. If the speeder starts at distance $X$ behind the observer, he will get to the point $P$ for the second time (and thus catch the observer car, just barely), if $(X + M)/S = M/T$.

So the relevant $X$’s are those less than or equal to $U$, where $U$ satisfies $(U + M)/S = M/T$. Thus, $(U + M)T = MS$, or $UT = M(S - T)$, or $U/M = S/T$. Thus, the probability of the speeder overtaking the observer, i.e. being observed, is $S/T$.

Using the principle that observations should be weighted by the reciprocal of the probability of observation, the weight of a speeder observed going at speed $S$ when the observer is going $T$ is

$$\frac{T}{S-T} = \frac{1}{(S/T)-1}.$$
\[
\frac{0.308}{0.692} = \frac{P(B/V)P(V)}{P(\overline{B}/V)P(V)} = \frac{(0.308)}{(0.692)} \left( \frac{1 - p}{p} \right),
\]
where \( p = P(B/V) \). Hence, when \( p = 0.177 \), the estimated relative risk is 2.07. When \( p = 0.192 \) the estimated relative risk is 1.87.

In the first case, \( (\hat{p}) = 0.177 \) and the standard error of \( (\hat{p}) \) is 0.0055. In the second case, \( (\hat{p}) = 0.192 \) and a standard error of \( (\hat{p}) \) is 0.014.

To convert these to standard errors of the relative risk, we use the delta\textsuperscript{32} method:

\[
\text{SD} \left[ \frac{0.308}{0.692} \left( \frac{1 - \hat{p}}{\hat{p}} \right) \right] = \left( \frac{0.308}{0.692} \right) \text{SD} \left( \frac{1 - \hat{p}}{\hat{p}} \right) = \left( \frac{0.308}{0.692} \right) \text{SD} \left( \frac{1}{\hat{p}} - 1 \right) = \frac{0.308}{0.692} \text{SD} \left( \frac{1}{\hat{p}} \right) = \frac{0.308}{0.692} \cdot \frac{1}{\hat{p}^2} \text{SD}(\hat{p})
\]

When \( \hat{p} = 0.177 \), SD(relative risk) = 0.078

When \( \hat{p} = 0.192 \), SD(relative risk) = 0.169