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The North Carolina Highway Traffic Study

Final Report to the
National Institute of Justice
U.S. Department of Justice

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Executive Summary

The North Carolina Highway Traffic Study NIJ Grant Award Number 1999-MU-CX-0022

Project Overview

The North Carolina Highway Traffic Study is a multi-method investigation of the phenomenon popularly referred to as “driving while black,” or more generically as “racial profiling” and “racial targeting.” There is widespread belief that African Americans and other minorities are at increased risk of police stops compared to white drivers. A 1999 Gallup Poll, for example, found that 56 percent of whites and 77 percent of African Americans believed that racial profiling exists (Newport 1999). In our own survey of adult, North Carolina-licensed drivers, 30 percent of whites and 80 percent of African Americans reported that they believed that African Americans were more likely than other drivers to be pulled over by the police. “Racial profiling” and “racial targeting” refer to a fairly specific police practice of using race as an explicit criterion for deciding which cars to stop or search. “Driving while black” is a less focused term, but summarizes a widespread belief in minority communities that they are singled out for harsher treatment than are white drivers.

In designing this project we were faced with three associated issues. First, the actual degree and spatial/organizational distribution of racial disparity in stops are not known and current methodologies are inadequate for establishing scientifically reasonable estimates of disparity. This project develops and evaluates a series of alternate methodologies for

establishing the degree of racial disparity in stops. Second, the political attention to this complex phenomenon needs to be clarified with theoretical understandings of the various mechanisms which plausibly could produce racial disparity in police stops. While racial profiling, the explicit use of race by police as an indicator of potential criminal status, might be one such mechanism, there is no reason to believe it is the only or even most general mechanism. For policy, ending explicit racial profiling might do very little to reduce racial disparity in police stops if other mechanisms produce racial disparity in stops and post-stop outcomes. Indeed, it is necessary to examine mechanisms that, on their face, are not racially biased, but may in fact work to produce observed racial disparity in traffic stops. For example, one might expect some level of observed disparity in stops if police deployment, perhaps in response to calls for service or accident rates, increases police presence in areas that are disproportionately minority. Here, minority vehicle stops may be a function of increased patrols and resultant citizen contact. Third, regardless of the level or dispersion of ethnic disparity in stops, the perception that “driving while black” places some community members at special risk represents a widespread threat to the legitimacy of law enforcement.

This project combines demographic analyses, highway observations, surveys of citizens, focus groups with drivers, and focus groups with North Carolina State Highway Patrol troopers to develop methodologies to estimate racial disparity in traffic stops, identify plausible mechanisms producing those disparities, and learn more about the consequences of perceptions of racial disparity in policing for trust in the police.

This project began in 1999 when the North Carolina State Legislature mandated that the North Carolina State Highway Patrol (NCSHP), and all state law enforcement agencies, begin to assemble data on the racial distribution of all vehicular stops initiated by officers. The NCSHP

agreed to cooperate with us in a more thorough study of traffic stops and outcomes. The cooperation of the NCSHP allowed us access to a great deal of demographic data on stops as well as cooperation in organizing focus groups with troopers and collecting observational data on North Carolina highways. Our project is a joint effort of faculty researchers and graduate research assistants at North Carolina State University and North Carolina Central University.

The research is intended to answer four basic questions: 1) Do NCSHP troopers stop minorities, particularly African Americans, on the road at higher rates than they do whites? 2) Once stopped, do African American citizens and white citizens experience different rates for citations, written warnings, and searches? 3) What factors might account for highway stops? and 4) How do African Americans and other ethnic minorities experience and respond to traffic stops? Our goal is to produce informed answers to these questions that can help to shape public policy, police training, and citizen outreach.

These specific research questions result from the way that we conceptualize the “driving while black” phenomenon. That is, we view “driving while black” as the result of at least three processes: police stops of motorists, the decisions that motivate police stops, including racial and ethnic bias as well as drivers’ behavior, and the interpretations of police stops by the minority communities.

We have taken substantial care to produce appropriate baseline comparisons (denominators) for highway racial homogeneity or heterogeneity and driving behaviors. Baseline denominators have been produced from available records (such as licensed drivers within counties), calculations to create estimates of “drivers driving” within NCSHP districts (in other words, it is necessary to estimate the number of drivers driving in a particular district [or area] who do not reside in the district of observation), direct observation of motor vehicles and

drivers on the highway, and traffic accident rates. These various baseline denominators allow us to more completely examine the racial/ethnic differences in stop rates and outcomes in the contexts of racial composition on the highway, driving behavior, and trooper activity (individual and organizational).

Official Records (Citations, Written Warnings, Searches, Stops)

Official record information provided by the NCSHP includes data bases on vehicular stops, citations, written warnings, and searches. Our ability to verify that there were stop records to match all associated written warning or citation records is limited by the fact that there was no identifying number linking the different data bases. The lack of such a linkage has implications for our ability to assess whether, for example, every non-accident related citation has an associated stop record. Using a restricted list of specific measures across data bases, we were unable to identify many stop records as matching the citation or written warning records. Indeed, we found that approximately one in three incidents that could have had a corresponding stop record did not – in part because stops at checkpoints do not require that a stop form be completed, calling into question the completeness of the stop record data and raising questions about why some stop records would not be filed.

As a consequence, we focused much of our analytic attention on the citation, written warning, and search records. As for the general relationship between citations, written warnings, and the race of the driver, we found that there was considerable variation in the racial distribution of these interventions across the types of behaviors that were likely to have resulted in the citation or written warning incident (such as speeding or unsafe vehicular movement). Our initial hypothesis was that the more subjectively measured behaviors, such as “driving too

close” or “failure to yield,” would show greater disparity than the more objectively measured behaviors, such as vehicular speeding (virtually always measured by radar guns when citations are issued), having a revoked or expired license, or failure to wear a seat belt. The opposite is the case: the more objectively measured indicators of violating behaviors in citations are more often involving African American drivers. This suggests the possible importance of variations in driver’s behavior as a primary determinant of whether or not someone is cited.

Racial disparity in official written warnings was generally found to be more pronounced than in citations. However, such disparity is inherently ambiguous. If African Americans are more likely than whites to receive a written warning for “unsafe movement,” it may be evidence of more lenient treatment of African Americans (who are receiving a written warning rather than a citation) or it may be evidence of so-called pre-textual bias: troopers are stopping vehicles and giving warnings as a pretext to looking over the vehicle for signs of contraband. Since searches by the NCSHP troopers are very rare, the latter interpretation is doubtful.

The question of the extent of racial disparity in citations and written warnings issued by the NCSHP is much more complicated than looking at the racial distribution of the stated reasons for these interventions. Ideally, the statewide incidence of citations for a type of vehicular behavior needs to be evaluated in terms of what are the likely mechanisms by which statewide patterns of disparity are generated. The understanding of when and where troopers are deployed may go a long way to account for racial disparity. For example, if there are relatively many African Americans living in urban areas of North Carolina, and the NCSHP over-patrol highways in and near urban areas, the proportion of citations issued to African Americans statewide may be enlarged.

Somewhat to our surprise, we found empirical evidence to the effect that there is also racial variation by time of day in the distribution of drivers on the highways of North Carolina. African Americans are more likely to be driving in the evening and early morning hours relative to their distribution in the licensed driver population. To the extent that the NCSHP happens to patrol more at night in some areas could also enhance the racial disparity estimate.

The process of evaluating whether the NCSHP stops and cites African Americans excessively is made easier if objective measures of drivers' behaviors are available against which to compare with citation rates. For example, all else equal, we would expect that the proportion of African Americans stopped for speeding or other infractions would mirror the corresponding rate at which these infractions occurred (if 20 percent of the drivers speeding are African American then 20 percent of the drivers cited for speeding would also be African American). Toward the goal of establishing a method to measure objectively drivers speeding, we conducted an observational baseline study at fourteen sites. Each site consisted of between 10 and 15 miles of highway (both directions). We sent a research team to each site for a week to measure the speeds of motorists passing our research vehicle, which was driving at the speed limit while traveling these stretches of highway. We estimated the speed of the passing motorists by using stop watches to measure the time it took the passing vehicle to pass the rear and front bumper of the researcher's van. These estimates were found to be within a few miles per hour of the actual speed (as validated in road tests that we conducted).

We operationalized speeding as driving at or above the speed at which drivers were generally found to be cited for speeding and found that there were differences in the driving behaviors by race. Specifically, African Americans were over-represented among those driving above the "speeding threshold" at which drivers tend to be cited. However, the African

American over-representation declined above approximately 8 mph over the threshold speed, indicating that the relationship between vehicular speeding and race is more complicated than we initially thought. The racial speeding differences that were observed, however, do not necessarily generalize to all highways in North Carolina, nor should they be used to generalize to other states. As argued in the report, the relationship between speeding and race is potentially quite complex, having to do with the types of roads and reasons for driving, among other things.

The observational baseline study was particularly useful in alerting us to the fact that there is considerable variation in the proportion of drivers who are African American across even very proximate locales. Even the same highway a few miles apart or two highways intersecting may have a substantially different racial composition. Subtle differences in the volume of patrolling could interact with the variations in where African American drivers drive to generate higher or lower percentages of African American drivers stopped and cited. That is, the percent of those stopped and cited who are African American may reflect variation in the deployment of troopers to one highway over another. Unfortunately, we do not have measures at the very micro-level of where troopers patrol. We only have records of troopers having written a citation or written warning in what we call county highway areas (segments of highway within about a third of a county). The unmeasured variation in patrolling, coupled with unmeasured racial composition of specific segments of highway (smaller than a county highway area), is referred to as the “spatial heterogeneity” problem. This reduces our ability to make strong claims about the degree of racial disparity and about the likelihood that racial bias accounts for such disparity.

So far we have been speaking about what is unavailable to us: multiple and objective measures of driving behavior and of where (precisely) troopers spend their time patrolling. On a more positive methodological note, we do have measures that can partially substitute for the lack

of direct measures of the behavior of drivers, and we can make some reasonable assumptions about patrolling so as to lessen the likelihood of spatial heterogeneity problems. Specifically, we have measures of the resident driving population living in an area such as a county (100 counties in North Carolina) or patrol district (fifty-three such districts). We were able to utilize data on the residency of drivers cited outside of their county of residency to create an estimate of “drivers driving” in an area (in a nutshell, we estimate the proportion of drivers driving in a district based on the assumption that their driving outside of their district of residency is racially proportional to the composition of licensed drivers within their district—see discussion in the report for details). A third source of data to be used as a baseline against which to compare citation and written warning rates is the racial composition of drivers in accidents. This third source of information is particularly useful in that it can be measured at relatively small units of analysis (the county highway areas alluded to above). We found that we could measure the proportion of those drivers involved in accidents who are African American at these relatively small units of analysis. While not an ideal unit of analysis, the county highway areas represent some degree of control for spatial heterogeneity problems.

Having discussed some of the primary methodological issues, we turn our attention to the results of the analysis. The analysis is broken down into two parts. The first assesses whether or not there are any districts which have a high rate of citations of African Americans (end of Chapter 2). The second describes some models for assessing whether specific troopers have unduly high numbers of citations of African Americans (Chapter 3).

For the aggregate analysis, in which our goal is to determine whether or not there are any districts (of the fifty-three NCSHP districts) with unduly high citation rates of African Americans, we show that there are several such districts (varying by night and day times) that

qualify as “positive outliers”—the districts “lie” outside a range of measurement that is likely due to measurement error, and as such, qualifies them for further scrutiny as to possible racial disparity. At the same time, it must be mentioned that there are even more districts that are “negative outliers”—in other words, they have fewer citations of African Americans relative to the prevalence of African Americans in accidents. Thus, some observers might attribute the pattern of results to measurement error or to factors that were unspecified in the analysis (uncontrolled factors). It should also be mentioned that the specific districts defined as positive or negative outliers will vary somewhat depending on which baseline (resident drivers, “drivers driving,” or accidents) is used. Thus, there is ambiguity as to whether any particular area is suspect as having unduly high levels of racial disparity.

Such a finding of ambiguity may be disquieting to those who would want to know what might seem to be a straight answer to a simple question: is a troop racially biased or not? We interpret our evidence to mean that we can safely rule out widespread and large degrees of racial disparity in the behaviors of the NCSHP across districts. However, we cannot rule out with certainty the presence of small degrees of racial disparity. That is, in some districts there may be some disparity that cannot be accounted for by the deployment patterns of troopers, but our measures and methods are not adequate to tell conclusively. Some will argue that our models are not sufficiently fine-tuned to rule out non-bias interpretations. We agree, but we do not have measures of what those factors might be. If there is racial bias operating, it is most likely of a “cognitive” sort (defined as bias that does not stem from conscious or overt racial antagonism).

As for the presence of racial bias in the behaviors of individual troopers, we are dealing with a somewhat different policy issue since evidence of racial disparity may be grounds for “personnel action”—at least of an investigative sort. That is, if a trooper has a tendency to cite

more African Americans than he or she “should”—relative to some baseline— then there may be grounds for looking further as to whether or not the officer is a “positive outlier.” This investigation would presumably weigh further evidence so as to determine if there are reasons other than bias for the high number of citations of African Americans. Thus, our research here has as its goal to show methods that could be used to identify the “outlier” troopers, and not to claim that any specific trooper is disparate or biased in his or her citation behavior.

We present some regression models at the individual trooper level in which the dependent variable is the number of African Americans cited in the year 2000. We find that a control variable for the volume of citations (specifically, the number of whites cited) is a strong determinant of citing of African Americans. So too is any of a number of contextual measures (the percent of citations issued by other troopers to African Americans or the percent of drivers in accidents who are African American). Time of day effects (such as late evening or early morning) and type of highway (interstate or rural highways) are also found. Approximately 60 to 70 percent of the variance in the citation of African Americans is explained by these factors.

When we try to identify troopers who represent positive and negative outliers relative to the ordinary least squares model, we find that approximately eighteen are “positive” outliers (have more citations of African Americans than our model indicates should be “expected”) and slightly more are “negative” outliers (fewer citations of African Americans). More sophisticated statistical models indicate only partial overlap in who is a positive or negative outlier. Thus, some of the “positive outliers” from the ordinary least squares model may be “false positives,” as some of the negatives may be “false” also. Such findings are unfortunate from an efficiency point of view, since there are multiple classifications of who has high (or low) levels of citations of African Americans. However, recall that our purpose is to show that the statistical models can

be used as part of a more general strategy to assess whether or not racial disparity or bias is present in the actions of individual troopers rather than to claim that any officer is disparate or biased. We think we have accomplished that purpose.

One final set of findings based on official records includes the study of the consent and probable cause searches conducted by the NCSHP. We distinguish between the behavior of the regular road trooper (who rarely searches a vehicle) from that of the Criminal Interdiction Team (CIT—whose job it is to stop vehicles, question drivers, and search for contraband such as drugs or guns). For the regular trooper, almost all of them seem to avoid proactive work toward the goal of conducting a search. The small volume of consent searches (searches justified based on some suspicion, yet requiring the permission of the driver to conduct the search) indicates that the regular trooper is not proactively looking to conduct searches. Among the small number of troopers who make up the CIT (about thirteen in calendar year 2000), searches are more common place (although the number dropped off in the late 1990s to only a little more than one vehicular search per day by the year 2000). As for the racial composition of the searches, the CIT troopers are more likely to search the vehicle of an African American stopped than that of a white. They did so somewhat inefficiently as late as 1997, but by 2000 the “hit rate” (successfully finding drugs or other contraband) was higher for an African American driven vehicle than a white driven vehicle (however, we do not mean to suggest that the high rate of finding contraband justifies the high search rate of African Americans, as that is a more complex question, involving utility and civil libertarian concerns beyond to the scope of our project).

As for the mechanism that could account for the disparity in the search rates of African Americans, it seems to us, based on discussions with CIT troopers, that their use of the “conversational method” is one that could easily lead to the manifestation of disparity or bias. In

this method, they question drivers routinely and evaluate such factors as the consistency of the driver's answers and the degree of nervousness of the driver (and other vehicle occupants). Based on what they see and hear, they may decide to ask permission to search the vehicle. We do not know whether African American drivers are more likely to draw the suspicions of the troopers because the troopers are "looking harder" for the proper "signs" due to the race of the driver. There are alternative explanations about which we simply lack sufficient information. For example, African Americans' perceptions that the troopers are "looking harder" for some violation when they are stopped may result in a higher prevalence of "nervous behavior," independent of culpability during their stops.

The plausibility that "cognitive bias" may account for the higher search rate of African American driven vehicles is enhanced by the CIT troopers' recognition that they use certain generalizations in their everyday interaction with the public. Decisions must be made on a daily basis as to whether or not the citizen in front of them poses a threat to them or not. Style of dress, hair or verbal expressions will all be drawn upon by the trooper in making the decisions. Some typing is probably necessary for some decisions, such as whether or not to exercise extreme caution. While we do not question the practical need for generalizing behaviors and situations, we merely point out that "typing" people may lead to decisions that have racially biased implications.

The Survey of North Carolina Drivers

The North Carolina Driver Survey was designed to compliment the official statistics analysis described above. Official law enforcement statistics are accounts of citizen-trooper encounters provided by the individual trooper and organization, as complete and accurate as they

may be. Further, official data contain little information about driver behaviors which may provide an opportunity to examine whether drivers who are stopped actually drive differently than those who are not stopped. The survey data we collected allows us to collect information on reported typical driving behaviors that may influence the probability of being stopped and to capture information about stops conducted by law enforcement agencies across the state. In the survey we not only asked North Carolina drivers whether or not they were stopped, but also why they were stopped, the outcome of the stop, and how they were treated. Overall, the survey was intended to 1) help develop more inclusive baseline estimates of African American and white motorists' differences in driving patterns and driving behavior; and 2) measure African American and white motorists' differences in traffic stop experiences and their respective interpretations of the events.

We conducted a telephone survey for a stratified random sample of current North Carolina licensed drivers (African American=1,368; white=1,487). The sample was stratified by race in order to have sufficient sample sizes to compare the experiences of white and African American drivers. The sampling frame included white and African American drivers who had applied for or renewed their licenses in the previous six months. Data were collected between June 22, 2000, and March 20, 2001.

A comparison of our final sample to the actual race-gender-age distribution of licensed drivers in North Carolina shows that our final sample is quite a good match to the state distributions. Still, in all four gender-race groups, young adults age 30–39 are under represented. In most statistical analyses we weight the data to correspond to the known gender and age distributions of licensed drivers within the two race strata.

African American drivers are significantly more likely than their white counterparts to report a traffic stop in North Carolina. The odds of a stop by local police may be twice as high for African American as they are for white drivers even after controlling for other demographic statuses and reported driving behavior. Local police are also significantly more likely to stop African American males relative to African American females, while among whites there is no gender disparity in stops after controlling for driving behavior.

The estimated racial disparity in stops by the NCSHP is much smaller, but still statistically significant after controls for driver characteristics and reported driving behavior. The NCSHP does not stop African American males at higher rates than African American females net of driving behavior. Among the NCSHP troopers, race is linked to other attributes in the stop decision. Older whites and whites driving late model cars are less likely to be stopped than are other whites. African Americans who report more risky driving behaviors are more likely to be stopped. This suggests that the NCSHP troopers are not simply reacting to the race of the driver, but perhaps to the combination of race and other status attributes for whites and race and driving behavior for African Americans.

After the stop, differences in white and African American reported experiences are less dramatic. African Americans are slightly more likely to have been informed that the stop was for a more discretionary reason. African Americans are also slightly more likely to report that they were treated disrespectfully after the stop. There are no racial differences in the distribution of self-reported citations, written warnings, and verbal warnings. Racial differences in experiences after the stop are small.

Our telephone survey also addressed citizen trust. We found that distrust of law enforcement is produced by a combination of negative personal experiences with the police,

negative experiences of family and friends, belief in police profiling on both racial and non-racial grounds, general distrust of government institutions, and being a minority.

The related problems of racial profiling and trust in the police are not simple ones. African Americans distrust the police because of their personal experiences and more general community orientations. Disrespectful interactions are particularly powerful sources of both distrust in the police and belief in racial profiling. This is not, however, simply a perception produced by direct experience. On the contrary, negative encounters with the police by family and friends generate distrust and increase belief in racial profiling. In fact, among African Americans, disrespectful police treatment or stories of disrespectful police treatment can even undermine trust in government institutions in general. Belief in racial profiling undermines trust in the police even among whites.

African Americans are more forgiving of the NCSHP than are whites. African Americans are more likely to translate negative experiences into distrust of local police forces than the NCSHP. This may reflect their observations of lower bias or more professional carriage by NCSHP troopers. Whites, on the other hand, are less discriminating. Any perception of disrespect or profiling undermines white trust in all types of police. Whites are particularly influenced by perceptions of non-racial profiling (for example a young driver playing loud music), perhaps because these are the types of profiling for which they or family members are thought to be most at risk. Thus while African Americans are more distrustful of the police in general than are white citizens, whites' trust in the police seems more vulnerable to recent experiences and media portrayals.

Citizen trust in police is also influenced by more general dispositions toward trust in government. This is true for white and African American citizens and for all types of police

examined. This suggests that the legitimacy of the police in general, and of specific police forces, is a nested problem. Police legitimacy is undermined by perceived disrespectful treatment (especially among whites), belief in racial profiling (especially among African Americans), and belief in other forms of profiling (especially among whites). Where racial disparity in treatment is lower, as in the NCSHP versus local police, African Americans do not translate negative experience into reduced trust. Police legitimacy is more vulnerable among whites. African Americans, however, have a lower level of trust in the police of all types stemming from their past experiences in, and cultural understanding of American society. Some of this can be seen in African Americans' lower trust in government institutions in general, but most seems to be focused on a specific fear of the police. Distrust of the police among whites is more strongly tied to distrust of government institutions in general.

Citizen Focus Groups

Citizen focus groups enable us to examine reported experiences of drivers and better understand the range of feelings about racially motivated traffic enforcement among both African Americans and whites. Specifically, we used these discussions with citizens to explore reported and perceived reasons for police stops, the perceived treatment of citizens by NCSHP troopers as reported by the respondents, their experience with other law enforcement encounters (local, county, state), how the police-citizen encounter began and unfolded (did the stop result from a stationary radar unit, passing on a two-lane or four-lane road, or driving side by side), and what knowledge citizens can report about police-citizen encounters by other community members, friends and relatives. We were very interested in learning something about the themes and patterns of their police-citizen encounters that might directly inform both policy and practice.

Our focus groups with African American drivers revealed a generally positive evaluation of the job that the police do. Participants were quick to say that the police had an important and difficult job and that they were grateful for the good work they do. At the very same time many of the African American drivers had very little trust in individual police officers. They felt at risk for harassment and bias based on race and made considered analytic distinctions for each and every time they were stopped. Some stops were judged fair, typically when the law was broken and they were treated with respect. In general, law enforcement as an institution was described as legitimate and reasonable, individual police were suspect, and racial bias was attributed to “bad apples.” There was, however, some disagreement among African Americans as to how common the bad apples are.

Stops that were not tied to serious illegal driving behavior—the most common of which was the “rolling stop sign” pull-over—were considered to be likely instances of racial bias. In many of these cases African Americans assumed race was the cause of the stop, because they did not recognize any other legitimate reason. In some cases this assumption was confirmed by the officer making the stop, such as when reporting that the African American citizen was stopped for being in the “wrong”—that is to say “white”— neighborhood (and thus out of place). One young man spoke of the time he was stopped (with his brother), removed from the car, tackled, and had guns drawn on him for driving in a neighborhood where another African American man on foot was being pursued by the police. Here, apparently, “young, black male on foot” was interpreted as “African American male anywhere.”

Lack of respect by the police during legitimate stops were also evaluated by some African American drivers as likely evidence of racial bias. Lack of respect in the interaction was interpreted as an indicator of racial bias, and encouraged the suspicion that the pull-over was

racially motivated as well. Troopers of the NCSHP, in contrast to officers attached to various local police forces, were singled out as treating drivers with respect and professionally. It was a clear pattern in the focus groups that African American drivers had less suspicion of the NCSHP than they did of other police officers. While this evaluation mirrors our findings in the citizen survey that racial disparity in police stops is lower among the NCSHP than among other law enforcement agencies in North Carolina, the focus group participants used respectful treatment, rather than the rate of stops, as the basis for arguing that the NCSHP was better.

In general, African Americans were more likely to perceive racial bias in a stop if the officer interacted with them in a disrespectful manner or if they were stopped without what they believed to be legitimate driving infractions. They seemed to be more than willing to acknowledge their personal responsibility for a “real” violation. What were seen as minor violations were another story. Here they saw race as the predictor of the stop, not the violation. This is in contrast with white drivers who tended to see all stops, “real” or otherwise, as discretionary and idiosyncratic. White drivers talked about “driving while blond” or “driving while a musician” or “he should have cut me a break.” In many ways white drivers evaluated the police more harshly than African American drivers did, and were also more likely to generalize “unnecessary” enforcement across agencies. African American drivers saw many stops as legitimate and some as potentially racially biased. White drivers saw most stops as illegitimate, but idiosyncratic.

We also found stark contrasts between African American and white drivers in evaluations of the “driving while black” phenomenon. African Americans tended to see it as just another example of general and continuing racial bias. Racial bias in the policing of drivers was seen as a form of discrimination similar to the other forms of discrimination faced each day. Its existence

was confirmed by some combination of their own experiences, stories they had heard from friends and family, media reports on questionable police behavior (Rodney King was often mentioned), and the existence of general levels of prejudice and discrimination in the society at large.

White descriptions were considerably simpler and more disturbing. The white focus groups tended to accept that police targeted African American drivers, but described racial targeting as at least understandable if not fair and justifiable. Since African Americans were stereotypically assumed to be more dangerous and thus more culpable, white citizens typically saw police stops on the basis of race as reasonable. Whites tended to use stereotypes and statistical discrimination arguments similar to those sometimes used by police to justify racial targeting. It seemed very easy for the white subjects to collectively justify discrimination in policing, even though they were quite resistant to taking personal responsibility for their own police stops. As such, disgruntled white drivers are not natural allies of African American drivers who fear they are being harassed because of their race.

NCSHP Focus Groups

The purpose of the focus groups with NCSHP troopers was to inquire into the following domains: What circumstances are considered before, during, and after a stop? What are the training issues pertinent to highway traffic stops and how are they interpreted by NCSHP troopers? Is there a perceived reward structure that might influence the behavior of NCSHP troopers? And, what are the troopers' interpretations of racial differences in stops made by the NCSHP?

Six focus groups were conducted in early June, 2001. Due to the racially sensitive nature of the topic, four focus groups were race-specific (two African American and two white). This was deemed appropriate in order to best provide a forum where respondents would feel less restricted, although each group noted that they would feel comfortable speaking in the presence of fellow troopers. The management and command groups were racially diverse. Our random selection process did not capture any women troopers (there are few women in the NCSHP relative to men). Focus groups numbered from six to nine troopers and each lasted approximately 2 hours. The sessions were facilitated by two members of the research team. Three other members of the research team observed the sessions shielded by a one-way glass window.

The central issue, of course, was whether or not extra-legal factors (especially race) serve as the basis for the disproportionate number of stops involving African American and Latino drivers. It appears that troopers, for the most part, engage in enforcement patterns they believe would yield the greatest number of enforcement opportunities. A major determinant in the decision to make a stop appears to be the “behavior” of the vehicle. The focus on such behavior seems to vary situationally. The interstate, it is believed, is more likely to yield speeding violations rather than seatbelt violations. Participants state that it is not possible to know the race of the driver on the interstate or at night. Rather, they report that their focus is the behavior of the vehicle. Still, and significant given our interests here, not all troopers were unwilling to attribute likely traffic violations to specific segments of the community.

Troopers also generally acknowledged that it is easier, because of reasons out of their control, to do their jobs in some places and not others. Simply, some citizens are more likely to resist the legality of the troopers’ actions, complain to supervisors, and challenge the citation.

Each of these situations is something that most troopers would like to avoid. Interestingly, but not surprisingly, it was reported that the NCSHP receives more complaints from whites as compared to African Americans and Latinos. We pick up the same theme in our citizen focus groups and our telephone survey. White focus group citizens tended to see any stop as an unnecessary intrusion and an unproductive use of police time. African American citizens tended to acknowledge and accept responsibility for stops resulting from clear violations of traffic laws.

This raised an auxiliary issue that compounds the problem of racial profiling: how does the coupling of expected resistance from a white person—who may also be from a more affluent segment of society that possibly harbors stereotypes of the minority community (such as perceived levels of law violations)—impact the level and degree of disparity in stop outcomes? This coupling, intentionally or unintentionally, may produce deployment or locales of enforcement that will serve only to increase disparity in traffic stops. It appears that while some deployment decisions are based on traffic demands (for example, a road with a history of accidents or fatalities), others are based on areas with a significant “opportunity” factor. That is, opportunities associated with density of bars or other environmental factors (“where the fishing is good”). Such decisions are more likely to target low-income people (thus disproportionately people of color) than their high-income counterparts. This is manifested with the presumption that they are less likely to challenge the action in court and that the higher income areas are involved in less overall criminality and disorder.

While there was acknowledgment of the possibility of racial profiling in the NCSHP, it is generally believed to be an infrequent occurrence today but perhaps was a more frequent occurrence in recent years past. They attributed the reduction in complaints to the dismantling

the statewide drug interdiction units and reducing the competitive nature surrounding the quantity of drugs seized.

Chapter 1 “Driving While Black” and the North Carolina Highway Study

This project is a multi-method investigation of the phenomenon popularly referred to as “driving while black.” Cognate terms that refer to police behavior include “racial profiling” and “racial targeting.” The general social problem is the widespread perception that African Americans and other minorities are at increased risk of police stops compared to white drivers. A 1999 Gallup Poll found that 56 percent of whites and 77 percent of African Americans believed that racial profiling is widespread (Gallup 1999). In our own survey of North Carolina drivers, 30 percent of whites and 80 percent of African Americans reported that they believed that African Americans were more likely to be pulled over by the police than other drivers. “Racial profiling” and “racial targeting” refer to fairly specific police practices of using race as an explicit criterion for deciding which cars to stop or search. “Driving while black” is a less focused term, but summarizes a widespread belief in minority communities that they are singled out for harsher treatment than are white drivers.

In designing this project we were faced with three associated issues. First, the actual degree and spatial/organizational distribution of racial disparity in stops are not known and current methodologies are inadequate for establishing scientifically reasonable estimates of disparity. This project develops and evaluates a series of alternate methodologies for establishing the degree of racial disparity in stops. Second, the political attention to this complex phenomenon needs to be clarified with theoretical understandings of the various mechanisms which plausibly could produce racial disparity in police stops. While “racial profiling,” the explicit use by police of race as an indicator of potential criminal status, might be one such mechanism, there is no reason to believe it is the only or even most general mechanism. For

policy, ending explicit racial profiling might do very little to reduce racial bias in police stops if other racially biased mechanisms produce racial disparity in stops. It is also possible that other, not racially biased, mechanisms produce the observed racial disparity in stops. For example, if police deployment in response to accident rates or calls for service increase police presence in minority neighborhoods this might lead to higher minority automobile stops as a function of increased patrols and contact. Third, regardless of the level or dispersion of ethnic disparity in stops, the perception that “driving while black” places some community members at special risk represents a widespread threat to the legitimacy of law enforcement.

This project combines demographic analyses, highway observations, surveys of citizens, focus groups with drivers, and focus groups with troopers to develop methodologies to estimate racial disparity in police stops, identify plausible mechanisms producing those disparities, and learn about the consequences of perceptions of racial bias in policing for trust in the police. This project began in 1999 when the North Carolina State Legislature mandated that the North Carolina State Highway Patrol (NCSHP), and all state law enforcement agencies, collect data on the racial distribution of all vehicular stops initiated by officers. The NCSHP agreed to cooperate with us in a more thorough study of traffic stops and outcomes. The National Institute of Justice funded this research. The cooperation of the NCSHP allowed us access to a great deal of demographic data on stops as well as cooperation in organizing focus groups with troopers and collecting observational data on North Carolina highways.

In this introductory chapter we first present a brief review of current approaches to the problem of racial bias in traffic stops and suggest that simple assumptions about the universality of police bias or lack thereof are unlikely to capture the reality of the situation. Instead we advocate that if bias occurs, it occurs in the context of the work different police forces do, their

organizational practices, the immediate locality where individual officers patrol, and in interaction with citizens. This is followed by a theoretical elaboration of several mechanisms that could plausibly produce racial differences in police stops. For example, one mechanism could be average racial differences in driving behavior. Simple differences in racial composition of stops do not demonstrate the presence or absence of police bias. Rather, we should suspect racial bias only after accounting for racial/ethnic differences in driving behavior. On a practical level, what is most important in this regard is the racial distribution of drivers on the road. Who is driving and where they drive will vary dramatically from place to place as a function of racial differences in residence, employment, and driving destinations. We argue that, for policy purposes, estimates of racial disparity in police stops adjusted for driving behavior are preferable to simple counts of the racial distribution of police stops. A variety of approaches to establishing racial disparity are briefly discussed in this chapter. Finally, the chapter introduces the issue of when and how minorities perceive police bias and the consequences of such perceptions for trust in law enforcement as an institution.

The Work of the North Carolina State Highway Patrol

Since much of this report focuses on the stop and search decisions of the NCSHP we begin with a description of the work done by the NCSHP. We wish to emphasize at the outset that this research did not arise out of a specific lawsuit or other accusation of gross racial bias by the NCSHP. Instead, this research was developed with the cooperation of the NCSHP who was willing to take the public risk of external research on the topic of racial bias in policing in order to both facilitate the research and improve police practice.

Public attention is often focused on the NCSHP when they make a large drug bust or when a trooper loses her or his life in the performance of duty, but the day-to-day reality of patrolling for most of the approximately 1,400 NCSHP troopers is one of responding to accidents and stopping vehicles that are speeding or are otherwise in violation of safety laws, and then writing citations and warnings. This is occasionally dangerous but seldom glamorous work. NCSHP troopers generally carry themselves with a great deal of dignity; they keep their uniforms crisp and clean, wear their hats when approaching vehicles, and endeavor to treat citizens politely but firmly. Making the highways safe is the primary and predominant function of the NCSHP in North Carolina. In addition, a small special force of approximately twelve troopers is assigned primary responsibility for drug interdiction and identification of other types of contraband. We will discuss this unit, the “Criminal Interdiction Team” in Chapter 4. For the most part we will be focusing on racial disparity and possible racial bias in the routine day-to-day activities of the NCSHP.

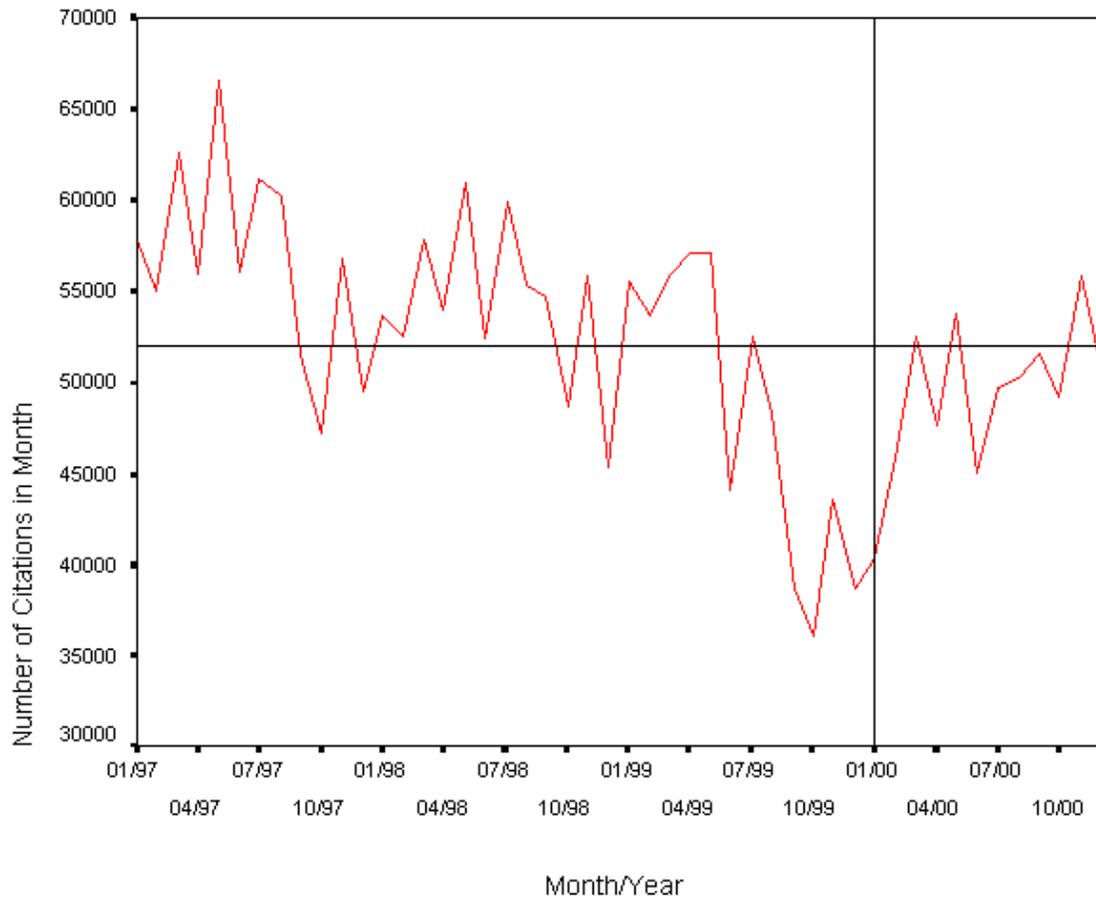
Because the activity of the NCSHP is primarily oriented toward vehicle-safety-related stops, about half of which are for speeding, the opportunities and motives for racial bias in police stops are probably small relative to other police forces that have both broader jurisdiction and enforcement goals. While troopers have some discretion in the decision to write a ticket, the information that generates speeding stops is mostly guided by race-blind radar devices. Racial stereotypes that associate minority status with criminality or drugs might be expected to encourage racial bias in policing, but most NCSHP troopers do little or no criminal or drug investigation work. Still, traffic law enforcement is a combination of proactive and reactive activities. NCSHP troopers receive few 911 or other citizen initiated calls, relative to local law enforcement, for example. This situation would seem to clearly increase the time available to

them to choose which citizens they encounter. But it is important to recognize that both citizen initiated calls for service and citizen initiated driving violations result in a reactive response from police. It is precisely the duality of traffic enforcement activities that is at the heart of the racial profiling controversy: Do police stop only those whom they see or only that which they see?

The major work of the NCSHP officer is stopping cars for traffic law violations. In Figure 1.1, the numbers of “citation events” handled by the NCSHP are presented by month for the years 1997 through 2000. A citation event is an occasion in which a citation or multiple citations are written. Several citations can be written at a stop or accident scene (an event). Since it is important for us to know about the composition of the drivers who are stopped, warned, cited, and so forth, in this report we will often be examining data on citation events. Here, and throughout most of the discussion in this and the next two chapters, we will be focusing on citation events when we refer to “citations.”

In Figure 1.1 we see that the four-year average of citation events is approximately 52,000 per month (horizontal reference line), or approximately thirty-seven per month for each of 1,400 troopers. Each year the NCSHP cites more than .5 million drivers in North Carolina (roughly 500,000 to 650,000, varying by year). The citation rates were generally above the four-year

Figure 1.1 Trend in Citations, 1997–2000



average (1997–2000) prior to the summer of 1999, and below it afterwards, although for some months in 2000, the rate does rise to or above the average. At least a portion of the decline after 1997 in the number of citations is probably due to the implementation of the NCSHP’s implementation of a Total Quality Management (TQM) plan intended to shift troopers’ activities to focus on their core responsibility—reducing accidents—rather than the volume of citations. We will discuss TQM more below in our account of the focus groups with NCSHP troopers.

The second, vertical, reference line highlights January, 2000, when Senate Bill 76, requiring NCSHP troopers to record all traffic stops, went into effect. Senate Bill 76 was specifically concerned with the possibility of racial bias in police stops among state law enforcement agencies, particularly in stops that did not result in a citation or written warning. Across 1999, there is a steep drop in citation activity. This is a period when the NCSHP patrolling was the lowest in several years (NCSHP Statistics, 2001) (The NCSHP Web site shows almost one-hundred-thousand fewer patrolling hours in 1999 than in 1998).¹ It was also the period of political discussions about racial bias and the drafting of legislation to require the NCSHP to collect new stop data and investigate the potential for racially biased policing. After January, 2000, the frequency of citations seems to become stable around the four-year mean. It appears that neither Senate Bill 76, nor the political and media attention to “driving while black,” influenced aggregate citation activity. It should be noted that the number of citation events will vary with the number of troopers working the highways, and we will have more to say about

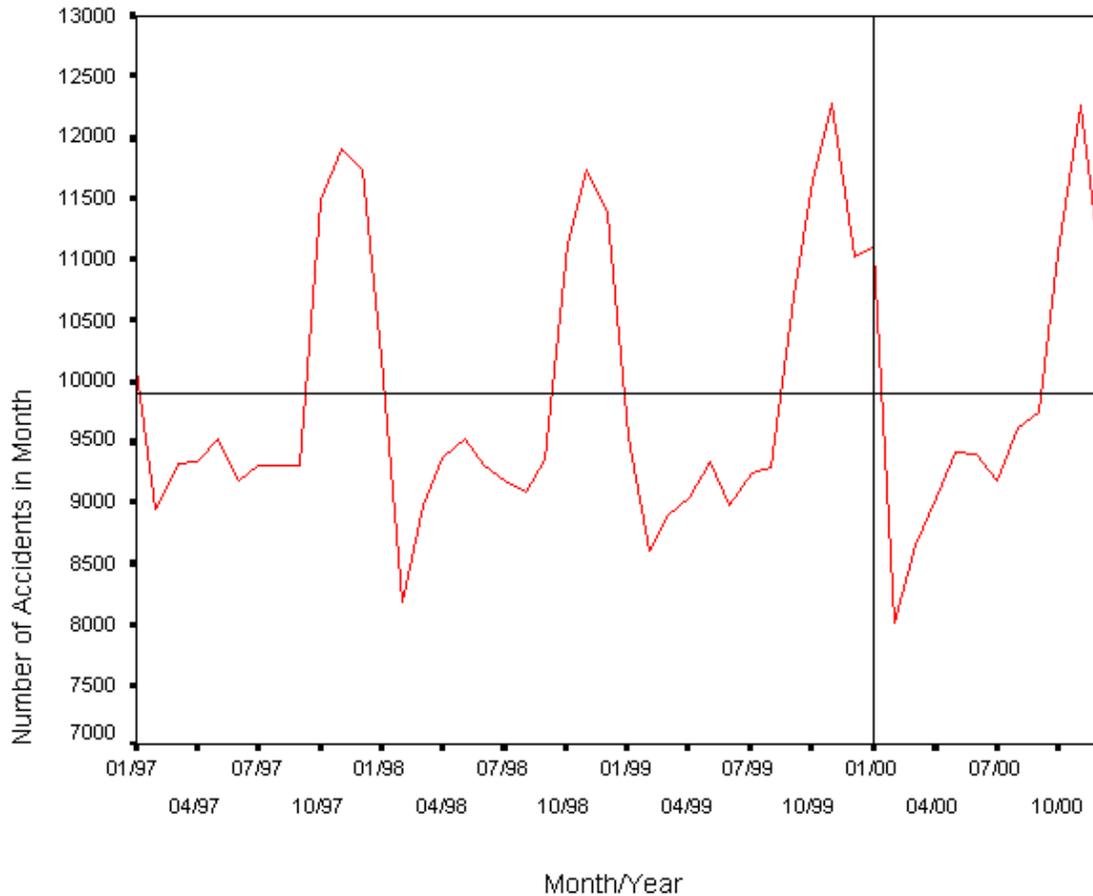
¹ In 1998, the NCSHP Statistics Web page shows that there were 1,056,049 “preventive patrolling” hours, compared to only 960,297 hours in 1999. In 2000, there were 1,064,283 preventive patrolling hours. The number of hours investigating collisions in 1999 was also up from 1998 (276,190 versus 264,972 in 1998). Thus only a relatively small percentage of the reduction in the number of hours on patrol could be attributed to increased hours investigating collisions.

that in subsequent chapters. Another interesting pattern we see from the graph is the seasonal effect. Here, the numbers of citation events decline during the fall, probably due to an increase in the number of accidents during that time (see Figure 1.2). Accidents take up a considerable proportion of the troopers' time, leaving less shift time for general traffic enforcement that would result in citations and warnings (written and verbal).

The number of accidents that the NCSHP responded to and noted in accident reports for 1997–2000 is presented in Figure 1.2. There are numerous accidents, and reporting them constitutes a significant proportion of the workdays of the NCSHP. The four-year average is approximately 9,900 accidents per month (horizontal reference line) or seven per officer per month. Looking at this reference line, we see that the accident rates seem to have the same seasonal cycle over the four years, peaking every fall. As with the graph on citations, the second reference line (vertical reference line) highlights January, 2000, when Senate Bill 76 was initiated. Unlike this reference line on Figure 1.1, this line does not mark any change in the pattern of accident rates. The main effects we see from the graph are seasonal effects.

Many of the stops by the NCSHP do not result in citations, but in written warnings. In Figure 1.3 we see that the mean four-year average is approximately 24,500 warning events per month (horizontal reference line). Looking at this reference line, we see that the warning rates were generally above the four-year average prior to the summer of 1999, the time period following the implementation of the NCSHP's TQM program, wherein quality, not quantity of

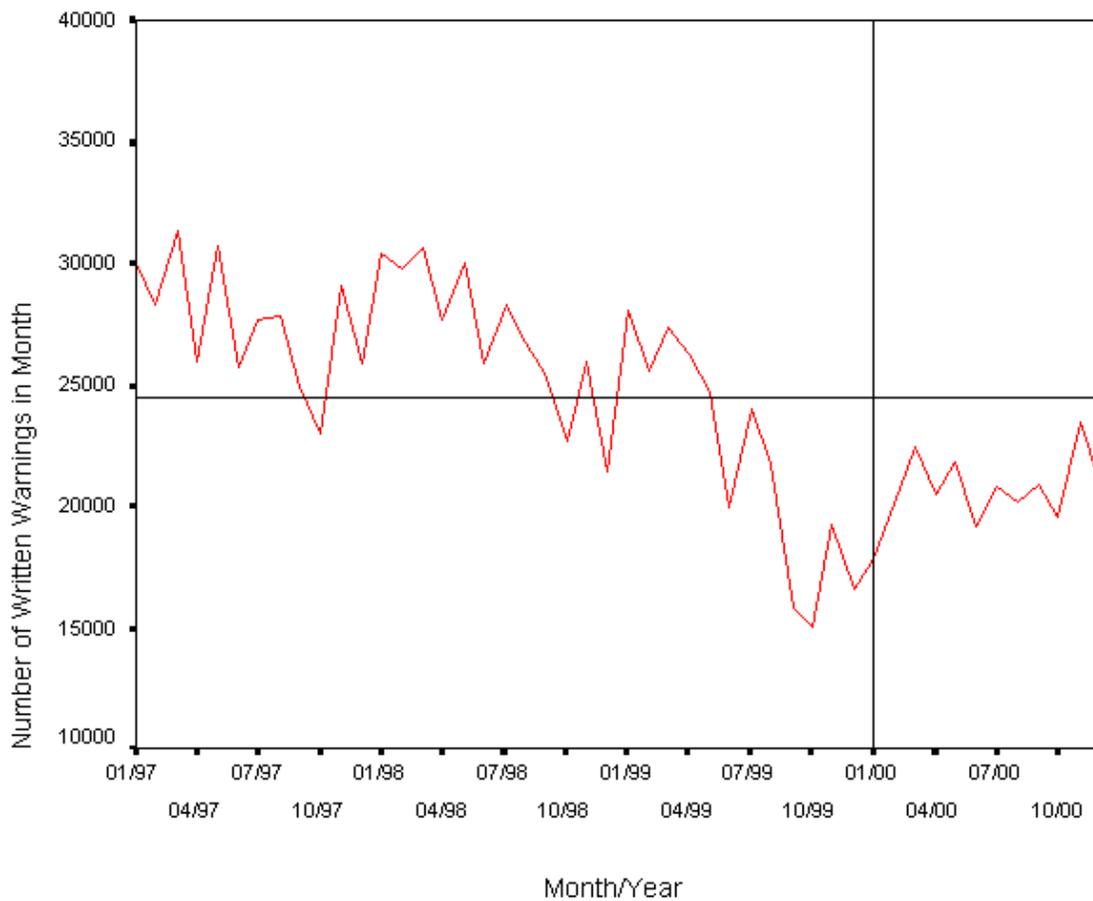
Figure 1.2 Trends in Accidents, 1997-2000



stops became a focus. Other than a much lower mean, this graph is almost identical to Figure 1.1 for citation events. At least some of the decline after 1997 in the number of warnings is due to TQM. The second reference line (vertical reference line) highlights January, 2000, when Senate Bill 76, requiring NCSHP troopers to record their traffic stops, went into effect. This line immediately follows the lowest point in the number of warning events.

Other than a much lower mean for written warnings, Figure 1.3 is almost identical to Figure 1.1 (of citation events). Thus, there is no need to repeat the discussion here of the trend. Note that there are fewer written warnings issued per month than citations. In Figure 1.3, we see that the mean four-year average is approximately 24,500 warning events per month (horizontal reference line).

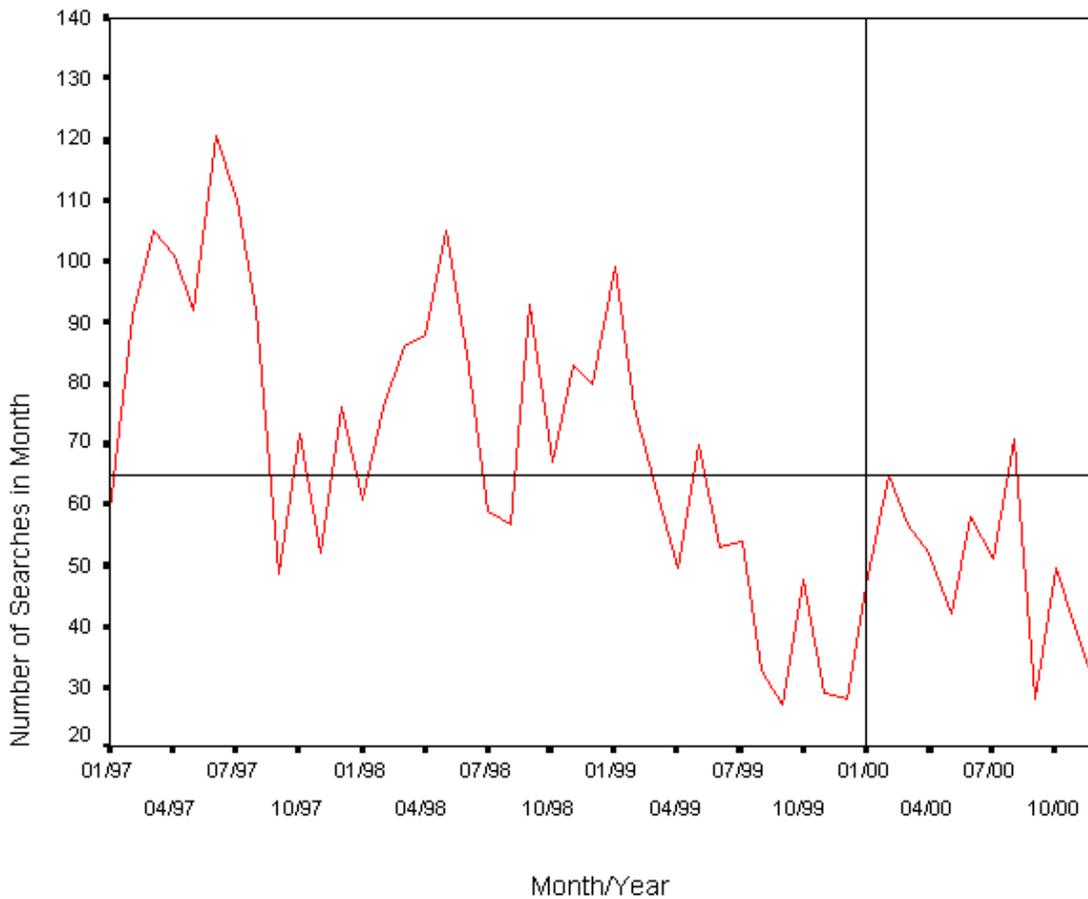
Figure 1.3 Trends in Warnings, 1997–2000



In Figure 1.4, the number of “consent searches” (which here include probable cause searches as well as consent searches) per month is presented for the years 1997–2000.

There are far fewer searches conducted per month (sixty-five), of course, than citations and written warnings. Consequently, the pattern of the trend line is less stable. The volume of searches seems to have remained low since 1999, unlike the pattern observed for citations and written warnings. Thus, one could say that there has been a decline in the number of consent and probable cause searches since the spring of 1999. Another interesting effect we see from the

Figure 1.4 Trends in Searches, 1997–2000



graph is the seasonal effect, but somewhat different from those observed earlier. Here, the number of search events increases during the summer months, perhaps reflective of increases in the volume of traffic on the interstate highways (where many of the searches take place).

In Chapter 5, we analyze survey data which allows us to compare in a preliminary way the degree of racial disparity and potential racial bias in vehicle stops by the NCSHP and the many town, city, and county police officers also operating in North Carolina. The work of local police forces is typically different from the NCSHP. They respond to 911 calls for service more often, investigate criminal activity more often, typically have more knowledge of residential areas and individuals in the community, and operate their patrol vehicles at far slower speeds. With the exception of 911 calls, all of these aspects of local police work increase the potential for discretionary stops relative to the NCSHP. In addition, previous research has shown that larger police forces (Gardiner 1969) and state police forces (Mastrofski et al. 1987) tend to have more professional training. Some reports (see for example, Adams 2000; Parker 2001; Gaines 2002) have tended to show large stop disparities by city and suburban police. Allegations of racial bias by state police and highway patrol troopers have often focused on vehicle searches, where discretion is presumably higher and where stereotypes about minority behavior may encourage searches for contraband. We report in Chapter 5, that even after accounting for racial differences in self-reported driving behavior, racial disparity in police vehicle stops is reported to be much higher among stops made by local officers than by members of the NCSHP.

The vast majority of this report is about the NCSHP. We include a contrast with local police forces in part to emphasize that the analysis of racial bias in policing needs to take into account the local context and content of actual police work. We develop in this chapter theoretical tools for thinking about racial bias in police stops that may be more useful in

explaining bias processes in other settings. Similarly, we develop across this report a series of methodologies for examining bias processes. We expect that the methodologies will be usefully generalized to other contexts. We do not think it is reasonable to generalize any of our findings (except perhaps the findings regarding trust in the police) to other police forces or geographic contexts. Our main contributions to the study of the “driving while black” phenomenon are theoretical and methodological. Our substantive conclusions are mostly restricted to the activity of the NCSHP in the most recent years.

The Importance of Process and Context

During the course of this research we have been struck by the deterministic flavor of discussions of racial bias in policing. On the one hand, stakeholders who wish to raise awareness of racial bias in policing and the consequent distrust of police in the minority community, tend to level blanket charges of police bias with little attention to the source of the bias or to alternative sources of disparity in stops. All bias is the same and the assumption is if you look hard enough you will find it everywhere. We call this the lay theory of a racist society. In our focus groups with African American citizens, we discovered that this lay theory of pervasive societal racism was generally endorsed. Conversely, police officers and other stakeholders tend to dismiss the charge of pervasive racism, claiming they don’t know any (or few) racists and that there are other good explanations for racial disparity in stops, such as not properly accounting for racial differences in who is actually on the road and breaking the law. White citizens we talked to also endorsed a more extreme lay theory in which racism was not the point: minority behavior required more police attention and intervention. Proponents of both lay theories, when pressed,

often will point to “bad apples”—that is, racist individual officers—as the source of the problem, although they will disagree as to the pervasiveness of the amount of rot.

From the viewpoint of social science, both lay theories seem quite misdirected. To argue that racial bias and racism are a unitary, pervasive phenomenon flies in the face of what we know about racial bias and its consequences. To argue that there is no racial bias anywhere in a very large organization or across police forces is equally improbable. As we will outline below there are a number of racially biased mechanisms that may produce racial disparity in traffic stops. Self-conscious, mean-spirited racism, while culturally the most recognizable, is by no means the most likely. It is also well known by social scientists who study organizational bias that the degree of decision making discretion and policies that encourage and discourage bias vary across organizational contexts (Bielby 2000; Reskin 2000). It is simply not plausible to expect that racial bias will be produced in the same way or with the same intensity everywhere. Nor is it plausible to expect that it does not exist somewhere. Our discussion of the difference between the NCSHP and more locally-oriented police forces raised some of these issues. We will also investigate variation across the fifty-three troop districts within the NCSHP in the degree of racial disparity and potential racial bias. We think that one of the most important contributions this project may make is to clarify the variety of bias producing mechanisms we might encounter and the importance of organizational context and policies for encouraging or discouraging racial bias. Racial bias in policing is not an all-or-nothing phenomenon. Instead, it is a series of questions about how and where bias is a problem.

Non-Bias Mechanisms that Could Produce Racial Disparity in Stops

The simple observation of a racial disparity in police stops or searches is not sufficient evidence to support accusations of racial bias in policing. Conversely, a finding that minority drivers are stopped or searched by police in numbers roughly proportional to their incidence in the population cannot be used to rule out the possibility of biased police stops. We define bias in police stops as disproportionately stopping, citing, or searching minority drivers given their incidence in the population of offending drivers encountered by police. This definition, assumes that it is police discretion as to who is stopped (cited, searched) which potentially generates racial or ethnic bias in the distribution of stops. Since the bias is an interaction between officers' discretion and the drivers available to stop, a suspicion of racial bias requires us to first develop estimates of who is at risk to be stopped and where the officers actually patrol before considering explanations of racially biased policing.

In most places we expect to encounter relatively large average racial and ethnic differences in driving behavior. The most important reason for these racial differences in driving behavior is locational. That is, there are very large African American-white, and Hispanic-white differences in residence patterns, reflecting historical patterns of residential segregation (Massey and Denton 1993). Although, not much is known about its spatial distribution, there are also well known patterns of racial/ethnic employment segregation (Tomaskovic-Devey 1993). Since we know that in most regions of the country the average white and minority citizens live in segregated neighborhoods and work in different organizations we can be fairly sure that in most places the racial composition of drivers in different places (neighborhoods, highway segments, main versus local streets) will be highly variable. Since we have no reason to believe that police patrol streets randomly and proportional to population, a city or state level ethnic disparity in

police stops may simply reflect the racial composition of roads that police patrol. This cuts both ways, observing that African Americans are stopped less than their proportion in the population does not necessarily imply an absence of racial bias. Rather, African Americans simply may be driving less in those places where police patrol more.

Where people drive is obviously related to both the locations of where they live and their likely destinations (such as where they work or where they travel to for shopping). How much people drive is obviously related to how far they must go to get places they need to be, but also to other factors which may be related to race. Outside of the South, the United States African American and Hispanic populations tend to live in more urban areas on average relative to whites. This simple residential difference will have implications for the degree of reliance on automobiles and mass transit. In urban areas especially, simple comparisons of those stopped to minority and majority population proportions may be quite different from the actual racial/ethnic distribution of drivers on the road.

There may also be racial/ethnic differences in the driving behaviors that are likely to place one most at risk of a traffic stop. Some recent evidence in New Jersey suggests that on some highways at some times of day, African Americans are more likely to break the speed limit at higher speeds than white drivers (Lange, Blackman and Johnson 2001). Our own research at fourteen sites indicates that African Americans speed disproportionately.² We know of no other research at this time that suggests that this is the case.

These preliminary observations must be cautiously examined. The minority population is slightly younger on average than the white population in many places. Since it is well known

² Note however that our observational study is of a non-random sample of observed drivers on four-lane highways, and one should not generalize the findings to all types of locations.

that young drivers engage in more risky driving behaviors, racial differences in the age structure might lead to small average racial differences in risky driving behavior. On the other hand, because of widespread fear of bias by the police in the minority community, we might expect minorities to drive more carefully than whites. Since so much of social life is organized or associated with race, it is not unreasonable to assume that, at least in some localities, there may be average racial/ethnic differences not only in where and how much people drive, but also how they drive. There is no reason, however, to assume, a priori, that it is minority drivers who tend to be worse drivers. It could quite reasonably be white drivers, secure in their privilege to drive as they wish, who are more frequently the risky drivers.

It is our expectation that racial differences in where people drive and how much they drive (because of their strong links to residence and income) will be much greater than racial differences in the use of seatbelts, turn signals, or excessive speed. One of the primary contributions of this project is to develop a series of methodologies for examining the spatial distribution, density, and driving behaviors of drivers at risk to be stopped. Another contribution is that discussions of racial disparity in policing require a good faith effort to account for non-discriminatory sources of racial disparity in stops associated with driving behavior before reaching a conclusion that a particular police force is guilty of racial profiling in traffic stops.

Deployment and Patrol Patterns

Police deployment may produce absolutely no racial disparity in police stop decisions, but a large racial disparity in stops. Deployment refers to where police patrol and concentrate their activities. Deployment for crime control, for example, tends to be greatest in higher crime neighborhoods. If higher crime neighborhoods tend to have larger minority communities, minorities might be stopped for traffic offenses at higher rates, simply because as a group they have a higher probability of encountering an officer. A similar pattern might hold if state troopers were deployed and concentrated their efforts in areas for reasons unrelated to race (for example, areas with higher accident rates) but those areas tended to have more African Americans routinely traveling these specific areas. This latter example is less plausible on its face, but the general point is that who gets stopped reflects, in many ways, where the police are deployed. It is possible for deployment to generate more minority (or majority) stops than their incidence in the population. Because residential segregation and employment segregation tend to be powerful forces in most places, we strongly believe that studies of racial bias in policing must be able to adjust for deployment patterns.

If police are deployed specifically to harass African American drivers, deployment could be influenced by a bias process at the organizational level. We suspect that it is more likely that police deployment is intended to reflect public safety goals such as crime prevention or highway safety. In this project we find that the racial composition of accidents is a useful tool with which to identify the racial composition of drivers at risk for stops, but the figures must be adjusted for police deployment by location and time of day to be most useful.

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Chapter Two: The North Carolina State Highway Patrol Data Bases and Evidence of Racial Disparity at the Aggregate Level

Racial Disparity in Geographic Areas

In this chapter we address the question of whether there is an excessive number of stops and citations of African Americans across the North Carolina State Highway Patrol (NCSHP) districts. This seemingly simple question is not easily answered. As researchers, our goal is to uncover indications of possible racial bias, yet we have no data on the motives of individual troopers who may or may not be biased in their behavior. Overt racism is seldom admitted to, and thus, evidence of possible racial bias must be gleaned from a statistical analysis of the behavior of troopers. As we will see, this evidence, while allowing us to rule out some claims about the prevalence of bias, is equivocal about others.

We separate two forms of NCSHP behavior relevant to the questions of bias: the deployment of troopers to a district (are some districts with relatively many African Americans “over” patrolled?) and the behavior of individual troopers toward citizens (Are the behaviors of individual troopers race neutral?). We take up the first question in this chapter and the second in the following chapter.

The analysis of race-specific rates of stops/citations across areas that we address in this chapter is important in two ways. First, these areal or aggregate rates can be used to study patterns of stop/citation disparity.³ If an NCSHP district, for example, has an unusually high rate of citations or stops of African Americans, it may indicate organizational practices in that district

³ By “aggregate” rates we simply mean that the number of stops/citations are summed for an entire geographic area such as a district or a county.

which need to be corrected. These practices may range from a seemingly benign decision to patrol one highway more than another because of tradition (“we always patrol there because there are a lot of speeders there”— and more African Americans happen to drive there, driving up the rate of African American stops in a district), to policy that possibly involves targeting highways because of the racial composition (troopers are deployed because a highway is known to have many African American drivers). In such a case, disparity may be reduced or eliminated by simply changing deployment patterns, or making deployment patterns more rationally based (patrol where preventable accidents occur) rather than based on tradition.

A second importance of areal analysis is that, as researchers, we cannot evaluate how troopers behave individually without knowing about the contexts in which they work. For example, a trooper working in the predominantly white counties of western North Carolina would not be expected to have the same rate of stops of African Americans as a trooper in the counties in eastern North Carolina, where relatively many African Americans live. If we can ascertain that a district does not have an excess number of African Americans stopped/cited, then we have a baseline against which to compare the behavior of individual troopers.

In the organization of this report, this chapter explores whether there are any patterns of excessive stops/citations of African Americans at the area level. Where there are no differences in stops/citations, or where the range of disparity is small (or within the range of measurement error), we have, as a result of this comparison, established a baseline against which to judge the behavior of individual troopers (this latter task we leave for the next chapter). For example, if we know that 23 percent of the drivers speeding on the highways in an area are African American, and that 23 percent of the drivers cited for speeding on the highways of an area are African American, then we have one base rate (later we will discuss others) against which to

evaluate individual troopers. That is, we can reasonably take the next step to see if there are specific troopers who have an excessively high percentage of their citations for speeding issued to African Americans. If an entire geographic area has an excessively high rate of citations of African Americans, however, then we will need to adjust our estimates to account for this fact when evaluating individual trooper behavior. (We will address this issue in greater detail in the next chapter).

Theories of Racial Disparity

Previous research has generally been focused on individual officer bias. As discussed in the first chapter, individual officers may make decisions (to stop, to issue a citation) that are racially motivated (active, racial animus) or make decisions that represent cognitive bias (officers are unaware that they give the benefit of the doubt to whites more than African Americans). What is called organizational bias or “institutional racism” (alternatively, “institutionalized racial bias”) also can be at work. The term has several different meanings, and can include any organizational policy that has an adverse effect on African Americans, irrespective of the basis or rationale for the policy. We find it helpful to differentiate policies (organizational decisions) that may have racial bias as its motivational origin from those that do not have such a motivational source, but which nevertheless have adverse consequences for African Americans. We argue that there can be three types of such “unconscious” bias at the organizational level: 1) cognitive bias, 2) irrational incidental bias, and 3) rational incidental bias (some may prefer the term “disparity” to “bias” here).

As an example of cognitive bias at the organizational level, suppose it were found that troopers over-patrolled a given stretch of highway because the local sergeant believes that there

are “numerous bad drivers” on that stretch of highway. But he or she (and presumably others in the Troop) may have formed that view based in part on the observation that there are disproportionately high numbers of African Americans usually found driving that specific highway who contribute to the high number of “bad drivers” (white and African American). In other words, unconscious bias against African Americans could lead to a labeling process (labeling an area or stretch of highway) resulting in over-patrolling of that stretch of highway. “Organizational cognitive bias” is distinguished from individual cognitive bias as the latter refers to possible bias of an individual trooper.

As an example of irrational incidental bias, suppose it were found that troopers tend to patrol a given stretch of highway because “it is easier to catch speeders there,” but that same stretch of highway is where, coincidentally, there are a disproportionate number of African American drivers (we say “coincidentally” because we are not implying that troopers are aware of there being a disproportionate number of African American drivers speeding). Again, stops and citations of African Americans for the area may be excessively high due to the over-patrolling of this stretch of highway. Reducing the patrolling of this highway would result in fewer African Americans stopped/cited and could reduce/eliminate racial disparity in the area. The “over-” stopping or citing of African Americans, in this case, is not related to any racial bias of a cognitive sort (nor motivational source, for that matter), and it is easily corrected by limiting the patrolling of that stretch of highway.

The third type of organizational decision that can generate racial disparity is one that is not racially motivated, and which nevertheless affects African Americans in an adverse manner (we assume that more citations and stops are “adverse”), but which is justified on the basis of rational principles of organizational action. For example, if it were the case that Highway A,

with disproportionately many African Americans, had substantially more accidents than Highway B, thereby justifying the allocation of a higher proportion of patrols to Highway A, then the consequence for African Americans would appear as a form of racial disparity, but one incidental to the rational allocation of troopers to that highway. We refer to this as “rational incidental disparity.”

Since we have no data on the motives of organizational actors (organizational decision makers), and differences in the patterns of NCSHP troopers’ behaviors are often small, we will most times not be in a position to interpret the data as being more consistent with one explanation than another. Is disparity due to cognitive bias, irrational incidental bias, or rational incidental bias? Sometimes, however, we will be able to weigh the evidence that is available to see whether for specific findings one explanation is more consistent with those findings than another, or perhaps only more plausible.

As mentioned, we do not have direct measures of the motivations of individual troopers or of Troop captains or sergeants. As such, as stated above, it will generally be the case that evidence of racial disparity cannot be interpreted unequivocally as bias. Nevertheless, despite possible ambiguity, organizations need to respond to the possibility that disparity and possible discrimination may exist. For example, it is possible that everyday deployment decisions by the NCSHP, even if made without any conscious implication of race, could have profound impacts on the racial composition of drivers who are stopped and cited. For example, suppose there are only two highways in a county and they have equal traffic volume. Because of patterns of activity between residence, work, and recreation, 30 percent of Highway A’s drivers are African American, while only 20 percent of Highway B’s drivers are African American. The average for the county would be 25 percent (the average of 30 and 20). If 25 percent of those cited in the

county were African American, one could conclude that there is no evidence of racial disparity. But what if, say, 28.5 percent of those cited were African American? That presumably would be defined as a racial disparity worthy of further investigation (in other words, 3.5 percent higher African American citation rate than the base rate).

The evidence of a 3.5 percent difference could be interpreted as follows: at the individual level, NCSHP troopers (acting alone) make either or both biased decisions to cite African Americans (racial animus) or racially unconscious decisions to let whites go with unreported warnings (cognitive bias). But there is another plausible interpretation: deployment can be unequal. That is, the difference in allocation could be accounted for statistically by deployment of patrols to highways where African Americans are known to drive, or the deployment of patrols at times of the day when African Americans are more likely to be driving. If this were the case, the various forms of organizational bias discussed above might be the appropriate interpretation. If the excessive deployment of troopers to Highway A is justified by a perception of the highway as a “problem highway” then organizational cognitive bias may be at work. If it were found that troopers patrol a highway because “the fishing is better there” (violators are easier to find), then what we call “irrational incidental bias” may be operative. If there has been a rash of accidents on Highway A, triggering a deployment decision that 70 percent of the patrol time is devoted to Highway A, while only 30 percent is spent on Highway B, then perhaps rational incidental disparity is operating. For the county in question, a citation rate of 28.5 percent for African Americans could be in part a consequence of the deployment decision (which had been made for non-race-related reasons). Without knowledge of the fact that Highways A and B differ both in the amount of patrolling and in the proportions of their drivers who are African Americans, a researcher might mistakenly interpret the 28.5 percent as evidence of

individual troopers' racial bias, instead of a consequence of a non-racially motivated deployment decision.

Suppose the situation were different and the sergeant deployed 70 percent of the patrolling to Highway A because his or her perception was that there were a lot of African Americans speeding on Highway A and "those people" needed to be stopped and cited. It would be unlikely that we, as researchers, would have access to information of such explicit bias of this sort (here, overt racial animus). Except for some very specific instances that have gained national attention because of the litigation process, there is seldom "smoking gun" evidence in racial bias research. Yet, the deployment decision in this hypothetical example was in fact a racially motivated and biased decision with arguably negative consequences for African Americans (excessive number of stops and citations compared to what would have been the case if a racially neutral decision had been made). For these reasons, we have to be cautious in interpreting the patterns of data on stops and citations. Any evidence of disparity requires further analysis and scrutiny. At the same time, it is difficult to determine when disparity should be interpreted as bias.

The real life situations of patrolling and citizen driving are far more complicated than described above. Troopers of the NCSHP typically have a great deal of discretion concerning where they patrol in a given hour of a given day, so that even if there was a directive of the first sergeant to focus patrols on Highway A, there is no guarantee that such a decision would be followed. There are several reasons for the discretion. So called "micro-managing troopers" has not been a tradition in highway patrolling, presumably because it would be time consuming to administer. Someone would essentially have to dispatch troopers and verify that they have not left a given highway. Patrol cars are generally mobile. From our conversations with NCSHP

troopers, they perceive themselves as more effective if their vehicles are mobile because they are more likely to come upon unsuspecting motorists violating the law. Factors such as the “good fishing” principle may be operating: troopers cite drivers in locations where they have historically found speeders and other violators to be plentiful.

From the focus group data collected for this project, we learned that troopers go where violations are plentiful not because they have quotas to fill, but because they have citations and written warnings to write as evidence of their productivity. Presumably they also go to where accidents are plentiful. But the two are not always highly correlated. For example, a very busy interstate highway may have relatively few accidents per vehicular miles driven, yet many speeders. Since speeding (frequent event) is presumably only moderately correlated with accidents (rare events), there must be locations where speeding occurs but few accidents (for example, a straight-away), and other areas where there are many accidents but little speeding (sharp curve or steep incline). Furthermore, the stopping and citing of drivers may be due to the fact that an area is within a short range of a major intersection such that drivers speed up as they drive away from that intersection, violating the speed limit by an excessive amount as they settle in on a comfortable speed for that highway. Data we have examined as to the location of stops suggests that troopers have a tendency to stop drivers within a mile or two of major intersections (of interstate highways). Whether it is because drivers are driving faster there or the troopers choose these areas for other reasons (an intersection is a convenient turn-around place) is not known, but we suspect that both citizen behavior and the “fishing principle” are operating.

As stated above, the general problem that we face as researchers is that data on possible racial motivation is generally not available, only data on stops, citations and accidents. Some inference need be drawn on the possibly biased nature of organizational responses from the

patterns of these data. One of the specific problems a patrol organization faces is that decisions by lieutenants and sergeants that affect deployment of patrol cars are likely to not always be based on accident data, but rather on partially subjective evaluations of accident patterns. Some subjectivity is probably necessary, since the type of accidents, their presumed causes and so forth ought to be taken into consideration. A second problem they face is the fact that the individual trooper is generally free (and, to some extent, required given staff shortages) to roam a rather large area while patrolling and the specific locations are chosen at his or her discretion. Thus, patrolling is unlikely to ever be fully rational, as the trooper's beliefs and ideas of what are "good fishing" areas will almost inevitably come into play. Thirdly, when troopers in patrol cars stop drivers in heavy traffic, they can be inadvertent causes of accidents. Thus, some of the most heavily trafficked areas may be "under-patrolled" for fear of causing more of a problem than is necessary. For all these reasons, the correlation of accidents and the volume of patrolling is likely to be attenuated (less than perfect).

Methodological Issues

Keeping in mind our two goals of studying aggregate units of analysis to inform our subsequent individual-level analysis and identifying areas where racial disparity is most pronounced, we next turn our attention to several methodological issues. First, we as researchers do not have direct measures of routine citizen-driver behavior. As a consequence, we must examine patterns among variables that stand as "proxies" (approximate measures) for citizen driver behavior. Some measurement error pertains to these proxy measures. These proxy measures include the number of resident licensed drivers in a district, an estimate of the number

of what we call “drivers driving” in a district,⁴ and the number of individuals in accidents (we will look at accidents reported by the NCSHP and by other agencies). We also have collected information at fourteen sites on speeding behavior of drivers. The data from these sites can help us validate the proxy variables, that is, help us determine how much measurement error there is across the proxy measures. Appendix A discusses the speeding study of the fourteen sites.

Another methodological problem we have to address is how substantial a statistical difference is necessary before we consider an area to have an excessive number of African American stops/citations. When dealing with stops/citations, there are hundreds of thousands of these events involving the NCSHP in North Carolina each year. It is not a given that a *statistically significant* difference is a *meaningful* difference. Also, differences can be attributed to measurement error. For example, if 23 percent of the drivers speeding in an area are African American and 22 percent of the drivers cited for speeding are African American, we must ask if that difference can be attributed to measurement error (in this case most likely in our estimate of African Americans speeding) or not (is there actually “reverse discrimination”?). Secondly, even if the difference is not attributed to measurement error, we must decide whether the difference is “actionable.” That is, should a policy decision reasonably be based on the observed differences that we find? These are complex issues, and we offer suggestions as to how to proceed toward resolving them.

⁴ More specifically, “drivers driving” refers to a statistical estimate of the racial composition of the drivers in an area, based on the aggregate racial composition of the area in which a driver who has been cited resides. This is discussed in greater detail elsewhere in this report.

The Importance of Context

As we have indicated above, to assess the presence and extent of racial disparity in an organization such as a highway patrol, it is useful to distinguish methodologically between individual trooper behavior and the context or environment in which the trooper works. We claim that without understanding the context of the trooper's work, one cannot evaluate statistical evidence that measures the individual trooper's behavior. In the example discussed above, a trooper working in the predominantly white, western counties of North Carolina would be expected, all else equal, to have a lower proportion of African Americans among his or her citations than a trooper patrolling in the predominantly urban counties, which in North Carolina have relatively large African American populations. But variation in context occurs within relatively small areas, such as a county. For example, a patrol of a highway near the predominantly white suburb of Cary in Wake County would be expected to yield a relatively low percentage of African Americans issued citations, compared to a highway on the east side of Wake County, outside of a predominantly African American residential section of the city of Raleigh.

Very "local" factors may also be relevant to the racial make up of a troopers' citations. The presence of a textile plant with predominantly African American employees would presumably have an influence on the percentage of citations issued to African Americans on a highway near the plant. Similarly, the presence of a bar where African Americans recreate and consume alcohol would affect the "driving under the influence" rate for African Americans on the highway near the location of the bar. Organizational factors, such as a sergeant's decision to "crack down" on DUI driving on a highway where the "African American bar" is located, also can affect the rate of DUI citations (and arrests), generally, and the African American rates,

specifically, for the district. Thus, the volume and often type of behavior that police observe or come into contact with will vary, depending on where the patrolling occurs.

Another set of factors can influence the rates of stops/citations of African Americans. Our analysis below reveals that the representativeness of African Americans on the highways is different at different times of the day, and different from that of whites. Our initial evidence of this was surprising to us, and so we looked for confirming evidence in the literature and found that the 1995 National Transportation Survey confirms our findings that African Americans are disproportionately to be found on the highway in the evening and nighttime hours (relative to white drivers). Also, we discovered that the NCSHP does not work the highways evenly across the hours of the day, and that the number of patrols vary across the state. Essentially, there is more patrolling at night in the more urban or heavily driven areas (especially where the interstates pass through) and less patrolling in the rural areas. The possibility of a mismatch between when drivers are on the highway and when troopers are on the highway needs to be addressed. The issues are complicated because it is conceivable that some decisions about deployment are themselves not racially neutral. For example, if there is an excessive number of African Americans issued citations because there is an “excessive amount” of patrolling at night, a reasonable question is “why?” The answer may or may not be racially neutral. We will discuss this possibility again further below.

Citation Zones

One further issue will be developed which we did not anticipate when we began this research and which seems to have a profound impact on the rate at which drivers are stopped. It is essentially similar to the “the speed trap.” We prefer to use the term citation zone as we

imply by that term something more than a trooper hiding his or her patrol vehicle behind a bush or in a ditch to catch speeders, but rather use the term generically to refer to an area of a highway where many citations are issued. Also, we imply by the term “citation zone” that other behaviors are looked for besides speeding, such as failures to stop, yield, weaving or other unsafe driving, or perhaps simply expired automobile tags. By using the term citation zone, we want to capture the idea that patrols tend to work certain stretches of a highway (a specific mile or several miles of highway), presumably because “the fishing is good there” — in other words, there are many speeders or other violators, and the highway design may be conducive to making stops (for example, the highway has broad shoulders to safely pull over vehicles). If these citation zones account statistically for a large proportion of the stops/citations, then very local conditions and circumstances are crucial to understanding the stop/citation rate of an area. For example, we notice that there tends to be a high rate of citation activity near major intersections, indicative of heavy patrolling of those areas. Why the high rate? The most probable explanation is that as cars accelerate coming from an intersection they often exceed the speed limit. One might even say it is a natural tendency to speed up as one enters onto a highway from another highway. Also, as one leaves a city or town where one has been driving more slowly — in accordance with slower speed limits — one may tend to accelerate to a speed well above the limit (the vast majority of drivers on major highways speed at some level). Perhaps the “liberating” effect of the “open road” represents a form of psychological “release.” Troopers may have learned that these are good locations to pick up speeders (and collaterally, other violators) because “the fishing is good” and the landscape is suitable for a stop. The racial distribution of stops in a district can be influenced by the choice of where to look for drivers. If the citation zone is near a residential area that is mostly African American, or it is near a factory where African Americans

make up a high percentage of the labor force, the stop rate of African Americans will be high for that district or county.

Our point about what we call the citation zone is that they can unduly “tip” the statistics in the larger geographic area in which they are embedded. That is, an area which statistically may not show evidence of racial disparity, may do so based on one “citation zone” that happens to be located where African Americans frequently drive (alternatively, the speed trap is located there for racially motivated reasons). In either case, the location of the speed trap is an important part of understanding the explanatory mechanisms by which racial disparity evidence is generated.

Spatial Heterogeneity

At a more abstract level, one of the predominant issues that we have to address in this chapter is that of “spatial heterogeneity” (also known as geographic heterogeneity) – a fancy term for the simple mismatch between when and where patrolling occurs and when/where driving occurs as these phenomena are measured across geographic areas. If troopers patrol more frequently at night in one district than troopers in another district, and African Americans drive more at night in both districts, the former will have a higher percentage of African American stops/citations than the latter. If troopers choose citation zones near areas with relatively high numbers of African American drivers, the percent of stops involving African Americans for the whole district may be affected. In both instances, race may or may not have something to do with the deployment decision, and we must consider that possibility in the analysis and interpretation of our data.

Proxy Measures of Citizen Behavior

One of the key issues to the study of racial disparity is that there is seldom any direct — much less error-free — measure of the behavior of the citizens driving on the highways where the troopers patrol. If one wants to know if the troopers issue an excessive number of citations to African Americans in an area, enforcement must be gaged relative to the prevalence of violating behavior of African Americans *where* the troopers are patrolling. Depending on the county, area of the county, and local features on a highway, the presence of African Americans, as well as their violating behaviors, may vary. The most difficult issue faced by researchers is to assess the adequacy of measures to approximate the behavior of citizens so as to have a basis for comparison. For example, if one knows that 25 percent of the “threshold speeders” on a highway are African American, then we would expect that 25 percent of the citations for speeding would be written to African Americans. (A “threshold speeder” could be defined as a speeder who is traveling at a speed that usually triggers a stop and citation by the NCSHP on that specific highway.) Lacking a measure of the threshold speeding behavior of drivers, a researcher must look for alternative measures. Such measures could seem rather crude, such as estimates of the proportion of drivers in an area who are African American or white. For example, census data, or data from the state Division of Motor Vehicles on the addresses of licensed drivers could be used to estimate what percent of the eligible drivers in an area are African American. Under the assumption that whites and African Americans violate traffic laws to the same extent and degree, and do so where the NCSHP troopers can see them, such measures would constitute adequate proxy measures of behavior (further assumptions could be made about out of state drivers or drivers from other areas within the state). If, however, there is variation in the

distribution of traffic violating behavior across races and across the locations where the patrols occur, or variation in the levels of travel from other areas of the state or out of state, then these resident-based proxy measures might prove to be inadequate.

Command Policy and Procedures

In addition to examining our data at the aggregate level to measure characteristics of the contexts in which troopers work, we also study at the aggregate level to gain information on the nature of the local command policies and procedures as they impact the racial composition of drivers stopped, cited or warned. Some sergeants may promote the issuing of citations, while others promote the idea of deterring through presence on the highway (particularly highways where accidents in general or specific types of accidents occur). The implications for race are not obvious, but if a push for a high volume of citations happens to occur on highways used disproportionately by African Americans, then that district may appear to have an excessively high rate of such citations. Yet, the statistical disparity is but an artifact of a possibly arbitrary decision as to where to patrol intensively. Of course it is also possible that the decision to patrol more intensively is itself racially motivated. We will also discuss that possibility below.

Other command policies or practices in place may have the effect of lowering the rate of citations of African Americans.⁵ For example, troopers have reported to us that citations are reviewed each week to monitor the racial distribution of that activity. Presumably the intent is to identify some number that represents “too many” and a trooper who is found to cite consistently a high percentage of African Americans would be called upon to account for this. If this

⁵ It would be naive to assume that it has been “business as usual” for the NCSHP since implementation of the special stop data collection. From our conversations with troopers, we know that they are well aware of the public and legal scrutiny to which their behavior is subject.

managerial decision has a real impact, it might lead troopers to be cautious in their personal citation rates of African Americans. We can only imagine what strategies a trooper would devise to avoid any possible reprimand. For example, a trooper may “look the other way” for some African American drivers so as not to come close to the acceptable proportion of African American citations. Or he or she may avoid citations of African Americans early in the week so as to have the option open for him or her to cite African Americans in the latter part of the week, should a particularly serious offense be observed. We do not know about these possible individual adaptations. However, it should be mentioned that we are unlikely to find any trooper with extremely high rates of citations of African Americans due to these weekly accounting procedures (however, we suspect that these are unevenly applied and are not systematically administered).

Aggregate Units of Analysis

To evaluate the behavior of the NCSHP at the aggregate level, it is necessary to discuss levels of aggregation (county, district, or alternative areal measures) that warrant study. There are only five aggregate units of analysis that seem relevant to our study. Starting with the largest, there are the eight regional Patrol Troop Areas (each covering about one eighth of the state). The second largest is the Patrol District Area (there are 53 NCSHP districts). A patrol district is usually made up of one to two counties. The third largest unit is the county (100 counties state wide). Fourth is the county area level (roughly one third of a county), an area that the NCSHP uses to locate citations in a subarea of a county. Last is what we call a highway area, defined as a stretch of highway between the borders of a county area. Although there is virtually no limit to how areas could be defined, (for example, a researcher might define an area

as one side of a highway between intersections), there is little practical reason for looking at citations or citizen behavioral proxy measures for areas that are extremely small because there are too few observations of trooper intervention to study statistically over a short time span of a year or even three or four years. Figure 2.1 shows how the units of aggregation differ. For analysis purposes we will largely stick to two or three levels of analysis, using the highway area for some of the analysis, the 53 patrol district areas (PDA) for other analysis, and – for presentation purposes – the eight regional patrol troop areas (PTA).

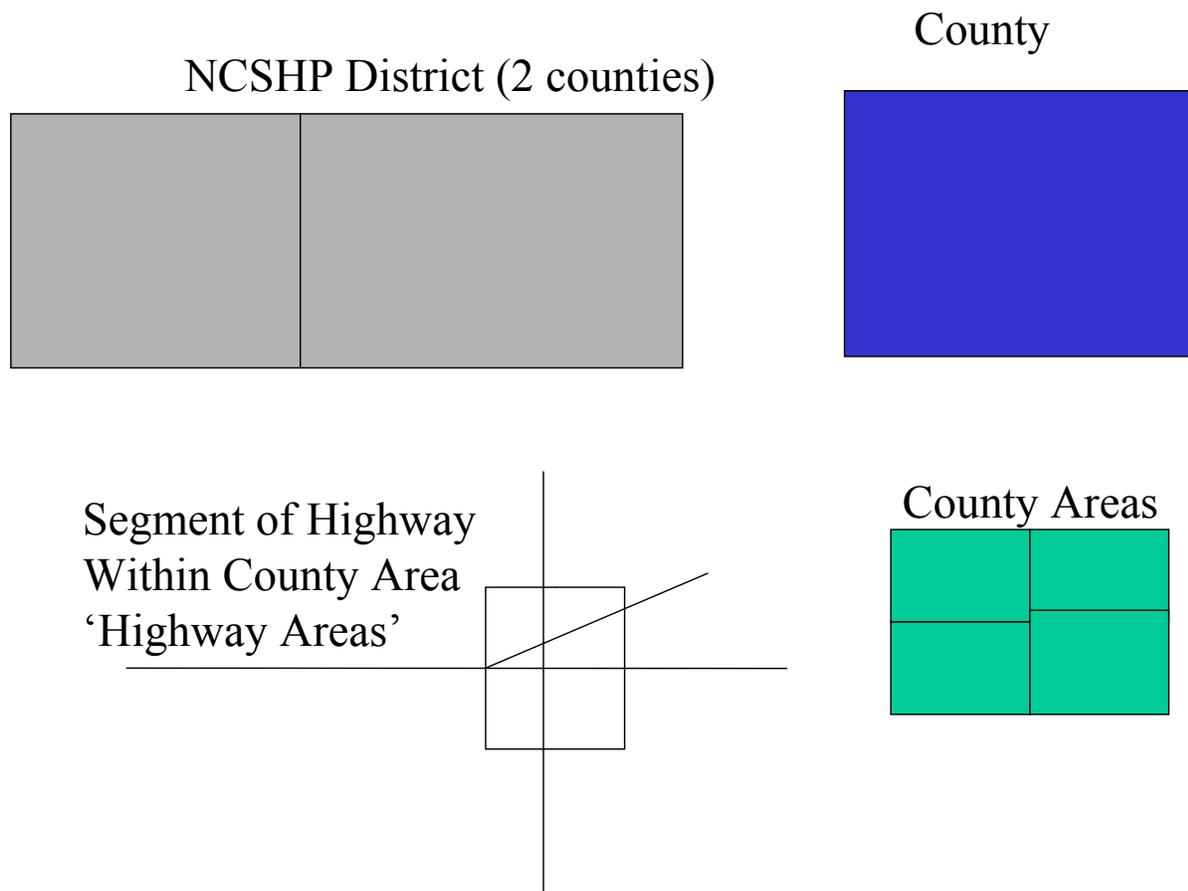
There are tradeoffs with the choice of a unit of aggregation. At the most micro level studied here (highway area) we argue that there is less measurement error introduced due to what we call “spatial heterogeneity” (essentially the mismatch between where troopers patrol and where drivers drive within an area). At the same time there is more measurement error of other types at the highway area level. Because highway areas are quite small, there will be measurement error attributable to relatively small numbers of observations (for example, few accidents may occur and few citations may be awarded in that particular stretch of highway). There are many reasons for measurement error that we can only imagine, and they probably vary with the type of data collected. For accident data, which we propose as a measure of a baseline against which to compare the citation rates of African Americans,⁶ troopers may have the option of letting another law enforcement agency (county sheriff or local municipal police) write up and record the accident report. The data we have on the number of accidents reported per county indicate that it is primarily the NCSHP who is responsible for handling the write up of accidents in counties (and not, for example, the county sheriff’s office), except in those counties with large

⁶ That is, we propose comparing the proportion of accident involved drivers who are African American with the proportion of those cited who are African American.

cities. In the more urban counties, a large proportion of the accidents will be written up by the local (city) police. It is not obvious what this means in these urban counties for the rates of African Americans cited when compared to the rates of accidents involving African Americans.

Figure 2.1

Four Possible Aggregate Units of Analysis



It might mean, for example, that if a high proportion of African Americans in urban counties have their accidents written up by the local police (perhaps because African Americans drive proportionately more within the city than around the city),⁷ then the proportion of accidents recorded by NCSHP involving African American drivers outside of the city limits may be relatively low. Accident rates are generally lower on large, four-lane highways, relative to the number of miles driven on those roads. Yet, speeding may be more prevalent on the large, four-lane highways – leading the NCSHP to cite the speeders there, even if the number of accidents per million miles driven is relatively low.

Stops, Citations and Written Warnings By Race

Bearing in mind these methodological issues, we begin to address the question of the extent of racial disparity by presenting the breakdown of stops, citations, and written warnings of drivers by reason for the citation. We show a breakdown by type of behavior in part to make clear what are the primary responsibilities of the NCSHP. The NCSHP works the highways primarily to stop, cite and warn drivers for driving misbehavior so as to make the highways safer. Unlike the local or city police, the primary mission of the NCSHP is to ensure safe highways— most directly through the enforcement of North Carolina traffic law. It is not to control, limit, and “fight crime” as crime is traditionally defined (such as robbery, burglary, larceny, or assault). Only a small fraction of the NCSHP—in recent years approximately twelve troopers— has as its primary purpose the task of screening drivers for suspicion of transporting illegal contraband. As can be seen in Table 2.1, the most common form of behavior that triggers

⁷ This is plausible, assuming greater African American participation in the service industry, for example.

a NCSHP trooper response is vehicular speeding. Data are presented for the year 2000, the year the NCSHP began to collect data on all of their vehicular stops (relatedly, see Appendix C on the charge likely to be the reason for the stop).

There are several interesting aspects of Table 2.1. First, the NCSHP stops and cites many drivers over the course of time, as was pointed out in Chapter 1.⁸ More than .5 million citations were issued in 2000 (the licensed driver population of North Carolina is approximately 6.5 million), resulting in roughly one citation issued for every fourteen North Carolina drivers (some more than once, so the prevalence of citations is somewhat less than one in fourteen). As for the racial breakdown of the citations, stops, and written warnings, we can compare initially the results to the overall percentage of drivers with licenses who are African American, which is 21.2 percent of all of the white and African American drivers.⁹ As can be seen in the table, 24.7 percent of all those stopped are African American, so there is some evidence of racial disparity in this simple comparison (3.5 percent absolute difference between 24.7 and 21.2 percent). The difference is slightly greater for citations (3.7 percent), and lower for written warnings (2.5 percent).

As such, the results in Table 2.1 tell us a lot about the extent of possible NCSHP racial disparity state-wide. We can rule out the possibility of large scale racial bias against African

⁸ We present data on stops here even though we know that a high percentage of stops seem to be “missing” or at least not in the data base that we have available to us (roughly a third of the records seem fall into that category -- partly due to lack of stop data on checkpoint stops). Later in the analysis we will focus on citations and written warnings, as they seem to constitute a more complete data base.

⁹ Note here that the base for the percentage calculation is all white and African American drivers, excluding all others. Also note that 21.1 is within one percentage point of other baseline denominators such as percentage of drivers in accidents who are African American or percent of our estimate of “drivers driving” who are African American – described elsewhere in the report.

Americans of the magnitude claimed against police by the American Civil Liberties Union in other jurisdictions, in which the chances of an African American being stopped were seventy-five times that of a white (various ACLU documents on racial profiling can be found at the Web site, see Reference under ACLU). Nevertheless, the 3.5 percent absolute difference in stops represents an approximately 16.5 percent relatively higher chance of an African American being stopped ($3.5/21.2$) than there are African Americans with licenses (across all charge types). Of course, what some may define as a small difference may still be a matter of great moral and legal concern.¹⁰

There are several reasons why the racial disparity shown in this table, however, should not be interpreted necessarily as evidence of racial bias— although bias of the various forms we have discussed earlier in the report could account for the racial disparity. Something as basic as routine variations in patrolling by district could explain the disparity (for example, more patrolling in urban areas, where African Americans are disproportionately found on the highway). Still, and although preliminary, the data analysis does indicate a first glimpse into the extent of racial disparity, and it seems to be in the direction of more action against African

¹⁰ The relatively higher odds of an African American being stopped (16.5 percent) should also be interpreted with caution because the proportion of African American among licensed drivers is a small percentage – only 21.2 percent. As the denominator in any ratio becomes smaller, very large relative odds can be generated. For example, take the findings for whites in the same table: 75.3 percent of the whites and African Americans stopped are whites, compared to 78.8 percent of the licensed drivers. The -3.5 percent difference ($75.3-78.8$) represents a relatively lower rate of being stopped of -4.4 percent. To say that whites have a relatively lower rate of being stopped (compared to their representation among licensed drivers) of 4.4 percent appears to be a small number, but it is the “same finding” as what we reported for African Americans – a 3.5 percent absolute difference (recall that only African Americans and whites are in this analysis, so the percentage of all licensed drivers and the percent of drivers stopped must each sum to 100 percent)

Americans, at least relative to their number in the licensed resident driver population for the state as a whole.

Initially reviewing this indication of racial disparity statewide, we observe that the percentage of those cited or warned who are African American varies across types of behavior (for example, speeding, lack of driver's license, faulty equipment, et cetera). These variations are interesting, in part because we did not expect there to be large differences in the percentages of cases involving African Americans across types of charge (why would bias be exercised for some types of charges and not others?). However, when presented with differences, we find it useful to try to understand whether the differences might be due to variations in behaviors across offense types. For example, should we interpret the high prevalence of African Americans among those cited for speeding as evidence of bias against African American speeders or as evidence that African Americans speed more than whites? Could the difference be due to, in part, relatively more African Americans on the highways at times of the day when more patrolling occurs (we address this later in the report)? Should we interpret differences in regulatory violations (such as failure to produce a valid drivers license or vehicle registration) as evidence of racial bias for that type of offense (note that troopers rarely know before stopping a vehicle that the driver does not have a license)? Or might it be due, perhaps, to differences in the promptness with which African American and white drivers renew their licenses and registrations?

Thinking about these issues in the abstract, it seems plausible to us that the educational and social class background of the driver may enter into the explanation of disparities in some citation rates. It is plausible that those with less education or who are of a lower social class may not have the same financial resources as higher educated and higher social class drivers, and

therefore may be less likely to renew registration or licenses in a timely way or fix faulty mufflers at the first sound of trouble. It is generally the case that African Americans (on average) have lower income levels and are more likely to have less formal education and to be disproportionately from lower social classes. Unfortunately, we have no measures of social class in our data so as to verify some of these observations. Nevertheless, some readers may prefer to interpret the patterns of data as evidence of class-related behavioral manifestations whereas others may prefer to see the patterns as indicative of bias.

The results shown in Table 2.1 indicate that by far the largest racial disparity in citations is due to license-registration-insurance violations. Although we do not know the social class background of the specific individuals involved in citations, it seems less plausible to assume that troopers' racial bias manifests itself more for license-registration-insurance violations than for all other types of violations than to assume that there is a behavioral basis for the disparity in these types of violations. Our reasoning is that citations and written warnings associated with license-registration-insurance infractions are clearly the responsibility of the driver (whereas there may be some doubt for a somewhat more subjective determination such as a charge for "vehicular weaving" if a driver is seen crossing the center-line).

If there is trooper racial bias operating in this class of violation, it could be that the troopers "look the other way" when they find that a white driver has a revoked license. However, given the seriousness of this offense (license revocation and/or no insurance, for example, require a court appearance), it seems unlikely that giving whites a "pass" would occur frequently, and even less likely that ignoring such an offense would be more common than "looking the other way" for less serious or more common violations such as speeding below a triggering threshold or "driving too close." That is, these data provide some *prima facie*

evidence that some cause other than racial bias (perhaps social class), may be responsible for racial variations in the behaviors that drivers are exhibiting on the highways, resulting in their receipt of citations and warnings. A racial bias explanation of all the patterns in the table would involve a specification that there are some behaviors where bias is exhibited and others where it is not.

We assumed, at the beginning of this research project, that the more subjectively assessed behavior data, such as “driving too close” or “vehicular weaving,” would indicate more disparity against African Americans because the subjectivity lent itself to cognitive bias manifestations. That is, all else being equal, we assume that it is easier to be biased in reacting to the driving of an African American if an objective indicator, such as a radar gun, is absent. The results in Table 2.1, however, suggest that this is not the case. Behaviors that we thought would involve greater subjectivity, such as “unsafe movement” or “failure to stop/yield,” show quite low racial differences in percentages (for citations— however, the percentages are somewhat higher for stops).¹¹

One can see in Table 2.1 that there are variations in the percentages of African Americans who are stopped, cited, or given a written warning. In general, we argue that the three data sources are not equal, however. Recall that we estimate that the stop data base is missing about a third of its records (up to, but probably less than, a third of its record since we do not have data on checkpoint stops – see Appendix G). Thus, these data may be a less reliable

¹¹ However, since we are missing about a third of the stop records, in part due to lack of data on checkpoints, we do not think the stop records are as reliable a basis for evaluating disparity as the citation data are. Written warning records are more complete than stop data, but receiving a written warning is a less severe sanction than receiving a citation, so arguably a written warning sanction rate that is relatively high is inherently ambiguous as to possible racial bias.

source on which to assess the extent of racial disparity. The citation and written warning data bases are reasonably complete. However, written warning data, we argue, are inherently ambiguous. Once stopped by the NCSHP, we suspect that all drivers would likely prefer a written warning to a citation. Consequently, is a high rate of written warnings issued to African Americans possible evidence of racial bias, or rather, is it evidence that African Americans are less likely to receive a citation? If one interprets stops that result in written warning as events that are really a “pre-text” for further investigation and a search, then there must be a substantial number of searches of African Americans resulting from these written warning events. We do not find this to be the case (see Chapter 4 for a discussion of the number of searches conducted by the rank and file NCSHP troopers).

In evaluating the likelihood of racial disparity, we argue that the most complete and least ambiguous official record source of information that may reflect bias is the citation data base (See Appendix G for the details of this argument). In Table 2.1 we can see that African Americans are more likely, relative to their representation among licensed drivers—approximately 21.2 percent-- to be cited for license-registration-insurance violations, speeding, equipment violations, other motor vehicle violations, and Driving Under the Influence (DUI) than are whites. At the same time, African Americans are less likely to be cited for unsafe movement and failure to stop/yield. Their chances of citation here are approximately equal to the 21.2 percent baseline comparison on seatbelt-type violations.

As such, the results for citations have a possible “income” or social class interpretation. Given that African Americans in general have lower incomes and are more likely to hold poorer paying jobs, it would not be surprising that offenses related to income would be more prevalent among African Americans. License or registration expirations, as well as equipment violations,

may occur more often among those having less money to make such repairs as replacing mufflers or fixing tail lights, and thus may disproportionately involve higher numbers of African Americans. Put another way, these results represent *prima facie* evidence that NCSHP stops and citations may reflect what they see to a greater extent than who they see.

The findings on citations of speeders are also interesting in light of recent data from New Jersey (Lange et al., 2001) suggesting that, for at least some highway areas (but not others), African Americans speed more than whites. Similarly, we have found that in a non-representative sample of fourteen highway segments in North Carolina, African Americans speed more than whites in twelve out of fourteen highway segments (our observational baseline study is described in Appendix A). However, neither of these studies provide a solid scientific basis for generalizing the results to other areas. Nor does either study control for obvious sources of speeding behavior that may be associated with race, such as age, gender, or inter-state travel. Put simply, we do not know for sure whether African Americans generally speed more than whites, or if they only do so on some highways under some specific conditions (for example, where there is a high speed limit, during certain times of the day, or differences in origination and destination points). Indeed, it may also simply be the case that, among African Americans, those driving on the highway segments observed in New Jersey and North Carolina are—on average—younger, more likely to be male, or more likely to be driving long distances. All are common correlates of speeding behavior.

Table 2.1 Frequency of Type of Citation, Stop and Written Warning by Race, African Americans and Whites Only (Row Percentages)

Type of Citation	White Citations (most serious charge)	African American Citations (most serious charge)	White Stops*	African American Stops*	White Written Warnings	African American Written Warnings
Speeding	216,142 (75.2 percent)	71,432 (24.8 percent)	133,155 (75.3 percent)	43,761 (24.7 percent)	66,807 (77.2 percent)	19,698 (22.8 percent)
Unsafe Movement	17,495 (79.3 percent)	4,576 (20.7 percent)	5,275 (73.6 percent)	1,893 (26.4 percent)	20,438 (74.3 percent)	7,079 (25.7 percent)
Failure to Stop or Yield	11,870 (80.2 percent)	2,937 (19.8 percent)	4,618 (76.1 percent)	1,454 (23.9 percent)	--	--
DUI	15,390 (73.5 percent)	5,540 (26.5 percent)	3,431 (70.7 percent)	1,421 (29.3 percent)	--	--
Vehicle Equipment	1,557 (76.0 percent)	492 (24.0 percent)	13,365 (74.9 percent)	4,482 (25.1 percent)	24,359 (73.4 percent)	8,812 (26.6 percent)
Seatbelt, Helmet, Child Restraint	84,916 (78.5 percent)	23,311 (21.5 percent)	37,000 (79.3 percent)	9,660 (20.7 percent)	--	--
Registration, License, Insurance	50,640 (68.5 percent)	23,319 (31.5 percent)	13,337 (79.4 percent)	3,458 (20.6 percent)	63,733 (77.0 percent)	18,994 (23.0 percent)
Other	3,913 (71.9 percent)	1,527 (28.1 percent)	21,376 (68.8 percent)	9,692 (31.2 percent)	--	--
Total	401,923 (75.1 percent)	133,134 (24.9 percent)	231,557 (75.3 percent)	75,821 (24.7 percent)	175,337 (76.3 percent)	54,583 (23.7 percent)

*We estimate that approximately one third of the stop records are missing (partly due to the lack of stop data on checkpoint stops) and thus caution should be exercised in interpreting these two columns of data. Percentages for African Americans can be compared to the 21.2 percent of African Americans with licenses in North Carolina.

Although written warnings are somewhat ambiguous to interpret, we will discuss the pattern of findings in the table.¹² Written warnings results indicate that African Americans are over-represented in the unsafe movement violations resulting in written warnings. Thus, it seems that African Americans are stopped more often for unsafe movement, but that they receive a written warning instead of a citation for the offense. One interpretation could be that a written warning is consistent with the level of seriousness of the infraction. An alternative interpretation is that the troopers use “unsafe movement” written warnings as a pretext for evaluating the driver and the vehicle prior to conducting a search. The pattern is similar for vehicle equipment violations, but less pronounced. Given the infrequency of searches by the regular NCSHP troopers, however, we do not think that there are very many “pre-textual” written warning stops. It might be that these higher warnings for unsafe movement and equipment violations are a pretext to check licences, registration, or for the presence (or smell) of alcohol. Still, we do not suspect this to be the case. In our conversations with troopers, they sometimes described discretionary written warnings more as a “gift” to drivers (or as fulfilling a directive such as in the case of equipment violations) rather than an investigative tool.¹³ To the extent that the differences reflect or are interpreted as racial bias, it seems more likely that they would arise out

¹² Written warnings, which the troopers are expected to write for most equipment violations, speeding that is not too excessive, and some other violations, are also issued in accordance with the NCSHP’s policy of promoting good community relations. In general, the average citizen would rather get a written warning than a citation. However, written warnings also have been claimed to be “pre-textual” in nature: troopers stop African Americans ostensibly to give a warning, but in reality the trooper is “checking them out” for more serious offenses.

¹³ By perceived “gift” we do not mean to imply that warnings are issued to individuals based on subjective factors, as opposed to objective ones, such as the seriousness or nature of the offense that the officer is confronting. Presumably a written warning is more likely if the drivers is only driving a few miles over the limit, as opposed to 10 or 15. Thus, in general, warnings are for less serious charges.

of a more subtle cognitive bias process in which low level driving violations appeared more serious to the troopers when the driver was African American.

In summary, Table 2.1 indicates that when compared to the number of licensed drivers, African Americans are over-represented among the ranks of those stopped, cited, or issued written warnings. No clear pattern emerges to differentiate stops from citations and from written warnings. Generally speaking, the percentages of individuals stopped, cited or issued a written warning indicate that there is no large-scale racial disparity (of the magnitude often presented and discussed in the media). Some racial disparity is found in the citation data, but we have no basis to rule out the possibility that there are differences in driving location and driving related behaviors associated with race. Written warnings are clearly more commonly issued to African Americans. However, given the fact that written warnings are preferable to citations and the fact that there are very few searches conducted of African Americans as a result of these types of stops, we see an ambiguous pattern of disparity.

In addition to our interest in written warnings, we are especially interested in verbal warning stops because they are a new source of information on trooper-citizen contact. Verbal warnings began to be recorded by the NCSHP in January, 2000. They represent a form of trooper intervention that was not systematically recorded previously by the NC SHP. Note that verbal warnings represent a very small proportion of reported stops (less than 3 percent). We do not conduct as extensive an analysis of verbal warning stops as we do of citations primarily because we have less information about such stops (due to the questionable quality of the stop data – See Appendix G) and because they very rarely result in an automobile search.¹⁴ Table 2.2

¹⁴ See discussions in Appendix G of the limitations of the stop data, in which we suggest that about a third of the stop forms seem to be “missing” from that data base – in part because of lack of data on checkpoint stops, and the likelihood that the citation and written warning data

shows the percentage of written warnings and verbal warnings issued to African Americans (we repeat the findings on written warnings from Table 2.5 to provide some comparative reference for the verbal warning findings). The results indicate that 22.8 percent of the drivers issued a written warning for speeding are African American, whereas 30.7 percent of those issued a verbal warning are African American. In general, the results show that African Americans represent a higher proportion of verbal warnings than they do of written warnings across all violation categories.

The over-representation of African Americans among those given a verbal warning can be interpreted in two different ways. The first interpretation would be consistent with the hypothesis that the NCSHP shows greater leniency to African Americans, in part due, perhaps, to increased scrutiny of the NCSHP by the media, state legislature, and others. Alternatively, verbal warnings have been discussed as a result of a “pre-textual stop” (to reiterate, a pre-textual stop is a stop for further police scrutiny, commonly expected to be for the purpose of conducting a search of the vehicle.) While neither interpretation can be ruled out, it should be noted that “only” a couple of thousand (2,023) verbal warnings were recorded in the stop data as issued to

base have records not accounted for with stop records. In addition the identity of the trooper is recorded differently on the stop forms (a special assigned identity number is used rather than the trooper’s registry number). This identity number exists for the purpose of protecting the trooper, but it also limits accountability in data processing. For example, if stop records are not always turned in by a trooper, there would be no systematic way to determine that fact. We believe, based on comments by troopers in the focus groups that we conducted, that troopers occasionally do not fill out, file or enter electronically (or have entered for them by a secretary) the stop form information. Thus, for example, in the year 2000 there are more “citation events” (occasions where at least one citation is issued) than stops -- even when we exclude citation events where accidents are involved. We expected there to be more stops than citations since the vast majority of citations result from stops (as opposed to resulting from an accident investigation). That is not the case, however. Because stops, citations, and written warnings are different data bases with different processes associated with them, one should not expect the number of written warnings, verbal warnings, and citations to sum to the number of officially recorded stops.

African Americans in 2000, out of nearly one million stops or events resulting in citation or written warning. Thus, the observed verbal warnings of African Americans represent a very small proportion of all the interventions of the NCSHP with citizens.

The idea that troopers stop African Americans as a “pretext” is a plausible interpretation if in fact African Americans are: a) stopped and given written warnings more often than whites; and b) have their vehicles searched more often than whites. The first condition is not sufficient as evidence of pre-textual stops, as written warnings are less serious forms of interventions than citations, and if such interventions are occurring more often for African Americans than whites, this could be interpreted as evidence of leniency toward African Americans. It could mean, also, that troopers are making legitimate stops for violations of traffic law that warrant no greater sanction than a written warning. The second condition does not hold as far as the rank and file trooper is concerned. The vast majority of troopers do no proactive searches at all (see the discussion in Chapter 4 regarding searches). Thus, we have no empirical basis upon which to suspect that the average trooper in the NCSHP is stopping drivers to issue a written warning in order to check out the driver or the vehicle for signs of more serious violations (drugs or guns), else such searches would be more prevalent.

From the analysis above, it could be said that there is the possibility of some low level of bias, for some driving violations, against African Americans, as African Americans are generally

Table 2.2 Frequency of Type of Written Warning and Verbal Warning to African American Drivers (Counts and Percentages*)

Type of Violation	Written Warning to African Americans	Verbal Warnings to African Americans
Speeding	19,698 (22.8 percent)	615 (30.7 percent)
Unsafe Vehicular Movement	7,079 (25.7 percent)	225 (35.4 percent)
Failure to Stop or Yield	--	36 (32.7 percent)
DUI	--	125 (33.3 percent)
Substandard Vehicle Equipment	8,812 (26.6 percent)	251 (37.4 percent)
Failure to use Safety Devices	--	93 (23.5 percent)
Vehicle Registration, License, Insurance, or Inspection	18,994 (23.0 percent)	116 (28.0 percent)
Other	--	562 (29.7 percent)
Total	54,583	2,023

* Percent = percent of all African Americans and whites issued a written warning or a verbal warning. Example: “22.8 percent of all the whites and African Americans issued written warnings for speeding in 2000 are African American.”

over-represented relative to the 21.2 percent of all African American and white drivers. Yet, there are a number of possible alternative explanations, such as the possibility that African Americans drive more frequently on highways where the NCSHP happens to work, or that they drive more at times of the day when the NCSHP is working (for example, night versus day).

The plausibility of these explanations was enhanced when we conducted our baseline study at fourteen sites (each consisting of 10-15 mile stretches of busy North Carolina highways), finding that the proportion of drivers who were African American on different

highways within the same county could vary considerably. This suggests that a mismatch between the extent of patrolling by the NCSHP and the prevalence of African American drivers could easily exist. We also learned from the observational data that the NCSHP's citation behavior is segmented: some parts of some highways may account for a rather large proportion of all citations (more on this below).

As a consequence of the analysis done so far, we began to understand that, in order to properly account for the likelihood of mismatch, it would be reasonable to explore analysis of data at relatively small units of analysis, such as what we call "county highway areas" (the continuous stretch of a single highway across about one third of a county). We chose this unit because it was the only unit of analysis available that was smaller than a "county area"—all of the highways within about one third of a county. We also surmised that there was probably more similarity in the drivers and driving behavior on a given, single highway area than on an intersecting highway area in the same county. Thus, the county highway areas would be reasonable units of analysis for addressing the mismatch issues discussed above (although far from perfect units of analysis). In addition, we also realized that we only have one type of baseline measure of driving behavior at the county-highway-area level—accident data (for example, the proportion of drivers involved in accidents who are African American), so that comparisons with other proxy measures would only be possible at higher (larger) units of analysis, such as the NCSHP's fifty-three districts covering the state (on average, a district consists of two counties).

Racial Disparity in Districts

The preliminary evidence on racial disparity in stops, citations, written warnings, and verbal warnings leads us to consider the possibility that citizen behavior could be responsible for the disparity. Unfortunately, we have no data base with which to test alternative hypotheses concerning disparity due to other factors, such as the types of racial bias that could be manifesting itself in the form of cognitive biases against African Americans. This unknown plagues us throughout the report. It is difficult to account for bias processes when, for the most part, we are limited to outcome data. Still, the extent of racial disparity in the findings that we have reviewed so far is far smaller than many would have anticipated.

The data in the tables above were for the entire state of North Carolina, and because the state is large, the possibility exists that we might find greater (or lesser) degrees of disparity if we were to look at sub-sections of the state. Researchers refer to this as “dis-aggregating” the data or, more precisely, aggregating the data to smaller units of analysis than the state as a whole. (We say “aggregate” because the process involves combining information for individual troopers to some areal unit of analysis such as a county or a district.)

Proxy Measures of Citizen Driving Behavior

One methodologically weak assumption, relevant to the tables above, is that we compare, for example, the percentage of drivers cited for a traffic violation who are African American to the percentage of African American drivers assumed to be on the highways in the state as a whole. Although the vast majority of those with North Carolina driver’s licenses presumably drive on North Carolina roads, they do not do so equally. In our attempts to interpret the data on

citations it would be preferable to have a baseline of data against which to compare citation rates for smaller units of analysis than the state as a whole.

Except for baseline data collected at fourteen cites (see Appendix A), we do not have data on citizen driving behaviors on the highway. Yet, several proxy measures of driving behavior are available, including the U.S. Census data, the N.C. Division of Motor Vehicles licensed drivers data base, and the NCSHP accident record data base. In some of our earlier analysis, we examined the U.S. Census data and found that a measure such as “proportion of residents who are African American living in a district” was somewhat less highly correlated with the proportion of citations issued to African Americans than was the proportion of African American licensed drivers, and so we dropped the U.S. Census data from further consideration. We reasoned that the number of licensed drivers would be expected to be a more accurate reflection of drivers likely to be on the highway speeding and committing other violations such that the presumed superiority of the Division of Motor Vehicles’ data seemed reasonable. Still, the reader should be cautious of baselines associated with census counts as a proxy for who is really on the highway.

Below, we compare the validity of three types of proxy measures of driver behavior: number of licensed drivers, an estimate of what we call “drivers driving,” and drivers involved in accidents. The Division of Motor Vehicles made available to us the data on licensed drivers with information on the county in which the driver resides. The demographic characteristics of the drivers on the highways in a county could be estimated by making the strong assumption that the drivers driving on the highways and violating the law (speeding, driving erratically) mirrored the drivers who were residents in a county. Of course, we know this not to be true with regard to gender (males drive more than females) and age (the young drive more than the old), but we have

no data to suggest that the racial representativeness of the drivers is substantially different from that of the residents.

Our second proxy measure of driving behavior takes into consideration the fact that drivers drive outside of their county of residency, and for some counties this consideration may affect the measure of the racial composition of the drivers in a county. For example, counties adjacent geographically to a highly urban county with a large African American population may have a higher percentage of African Americans on their highways due to the spillover of drivers from the more populated county. Alternatively, counties disproportionately African American may have large numbers of white drivers pass through them, especially on the interstates, lowering the proportion of drivers who are African American, relative to the population proportion.

To take into consideration the invasion of drivers from one county to another we used the following estimation technique to estimate the proportion of “drivers driving” who are African American: we made the assumption that drivers who drive outside of their county do so in a manner proportionate to their representation in the population of their county of residency. That is, if 20 percent of the residents in a county are African American, then we assume 20 percent of the drivers who drive in another county from the county in question are African American. We estimate the number of drivers from a county who are in another county by using data on citations and matching the records with the DMV data base. Thus, for example, if 500 individuals cited in County Y were residents of County X, then we assume that approximately fourteen times that many drivers from County X actually were driving in County Y in the year in question (fourteen is the approximate multiplier actually used, base on the general risk of receiving a citation in a given year, roughly one in fourteen). The assumption we make is that the demographic makeup

of these “invader” drivers parallels that of the residents of County X. Through this method we were able to estimate the number of drivers driving in each county, and compute the proportion of “drivers driving” who were African American. We refer to the measure as our estimate of “drivers driving.”

The third proxy measure is the most powerful measure in the sense that we can count the number of drivers who are African American and who are involved in accidents for relatively small areas, such as highway areas. A highway area is the area along a highway in what the NCSHP call an “area” of a county (roughly one third of a county). The NCSHP record most of the accidents that occur in the rural areas of the counties, with the exception being the large urban counties where the local police record many of the accidents. Earlier in the report we showed that there is a reasonably high correlation between the proportion of drivers in accidents who are African American and the proportion of speeding drivers who are African American. We would expect that the proportion of accidents involving an African American driver and the proportion of drivers cited who are African American should approximate one another.

Although we argue that the accident data provide the best proxy measure at small units of analysis (See Appendix G), our initial data inquiries discovered an interesting pattern. There are many accidents on the relatively narrow roads in rural areas of the state, where patrolling is generally light. These roads do not have as high a volume of traffic as do the relatively more well-traveled interstate, U.S. and N.C. highways, and thus individually these “country roads” do not warrant heavy patrolling. By contrast, the busier highways are safer (fewer accidents per vehicular miles driven), but because of the high volume of traffic, they warrant heavier patrolling than the rural highways (in part the patrolling is justified because more vehicles can be slowed by the visual presence of troopers on the busy highways). One implication of this for race is that the

majority of rural paved highways in North Carolina have a higher percentage of white drivers compared to the busier highways, which are the more heavily patrolled highways. As a consequence, we find that the more rural counties tend to show excessively low rates of citations of African Americans, and the more populous areas slightly higher rates of citations of African Americans when we compare the proportion of cited drivers who are African American to the proportion of drivers who are African American and in accidents. We will discuss this more below.

Accidents are much more infrequent than citations, and so we found it necessary to aggregate three years of accidents to get a sufficient number of accidents to justify statistical comparisons. We examined the data to see if there was any evidence of a change in the percentage of drivers involved in driving accidents who were African American. Generally the correlations across years were very high, indicating stability in the demographic composition of the drivers over a three-year period (results not shown here). The demographic profile of drivers involved in accidents was explored further to determine whether the “contributing circumstances” of the accident, which had been recorded for only about half of the drivers involved in accidents, were correlated with the types of driving behaviors that elicited citations. (A “contributing circumstance” is perhaps a benign way to express finding fault with the driver’s behavior.) For example, a factor such as “excessive speed” may be considered a “contributing circumstance.”

We thought initially that if, for example, 20 percent of the drivers cited in an area for speeding were African American, then we might expect that 20 percent of the drivers in accidents in which speeding was a “contributing circumstance” would also be African American. Although there was generally a strong correlation between speeding citations and speeding “contributing circumstances,” our analysis indicated that the behavior-specific accident measure was a less

valid indicator of who was driving on the highway and a less valid measure of who was violating a law (such as speeding) than was the measure of the proportion of drivers involved in accidents (regardless of “contributing circumstances”). Part of the measurement error here may lie with the fact that only about half of the accidents have data on “contributing circumstances.” The “lost” observations (accidents with no contributing circumstances indicated on the official forms) are substantial in number, and their omission makes the correlation with citation data worse. In summary, among the accident measures (specific-behavior versus accidents in general), the best correlate of the proportion of citations issued to African Americans (or for any specific type of behavior resulting in a citation) is the proportion of drivers in accidents who are African American. Statewide, the differences across the various proxy measures in the percentage of drivers who are African American is rather small: Census 21.8 percent, resident licensed drivers 21.2 percent, “drivers driving” 21.6 percent, and accidents 19.8 percent.¹⁵ Of the four, the percentage of drivers in accidents who are African American represents the lowest percentage, and thus the more conservative of the four baselines against which to compare stop, citation, and written warning rates. We say “conservative” because accidents are the baseline with the lowest estimate of the percent African American, and thus it is most likely to generate a positive difference score (or a larger positive difference score) relative to the African American rate of stops, citations, and written warnings.

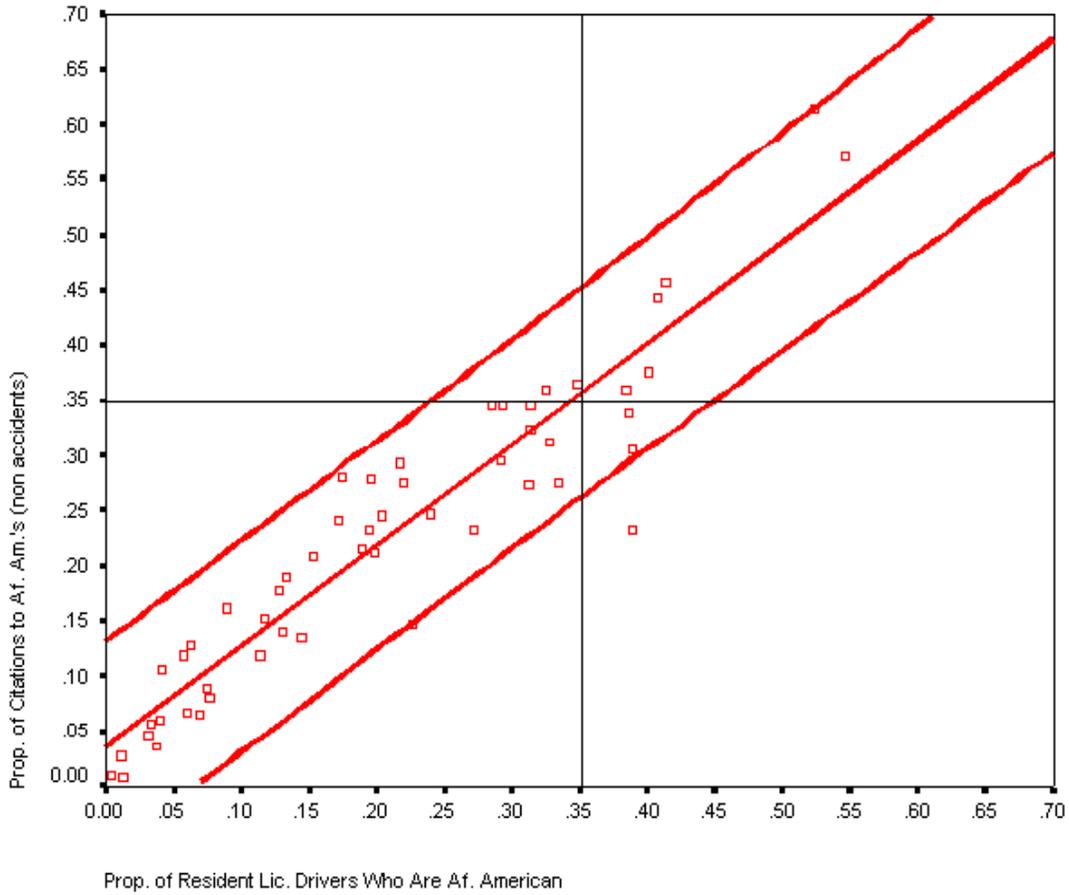
Because racial differences in statewide data could easily be accounted for by the presence of more patrolling in the more urban areas (and urban areas are generally more African American), it is appropriate to examine the data at smaller units of analysis. Figures 2.2 through

¹⁵ The figure 19.8 represents the percent African American of all white and African Americans involved as drivers in accidents in the NCSHP accident data file for the aggregated years 1997 through 2000.

2.4 show the correlation and “scatter” involved in the relationship between three proxy measures of behavior and the proportion of citations issued to African Americans: resident licensed drivers, “drivers driving,” and drivers in accidents. Note that the correlations are calculated at the troop district level (N=53). The correlations are essentially the same across these three proxy measures (r 's range from .93 through .96), such that most researchers would say that the three measures are indistinguishable in terms of predictiveness. That is, the proportion of citations issued to African Americans at the district level can be equally predicted by using residency data, “drivers driving” data, or accident data.

Figure 2.2 provides an example. Here the proportion of resident licensed drivers who are African American is graphed (the horizontal axis) along with the proportion of cited drivers who are African American (the vertical axis). The middle line in the figure is the linear regression line, or our statistical model's best estimate of what proportion of cited drivers should be African American, given the general pattern. The small, hollow boxes that are scattered around the regression line are actual observations of districts (fifty-three of them). As one can see, the scatter of boxes is both above and below the regression line, with most of the scatter occurring within a 95 percent confidence interval around the line. That is, most of the scatter is within a

Figure 2.2 Proportion of Citations Issued to African Americans by Proportion of Resident Licensed Drivers Who Are African American, by Troop District (N=53 Districts)



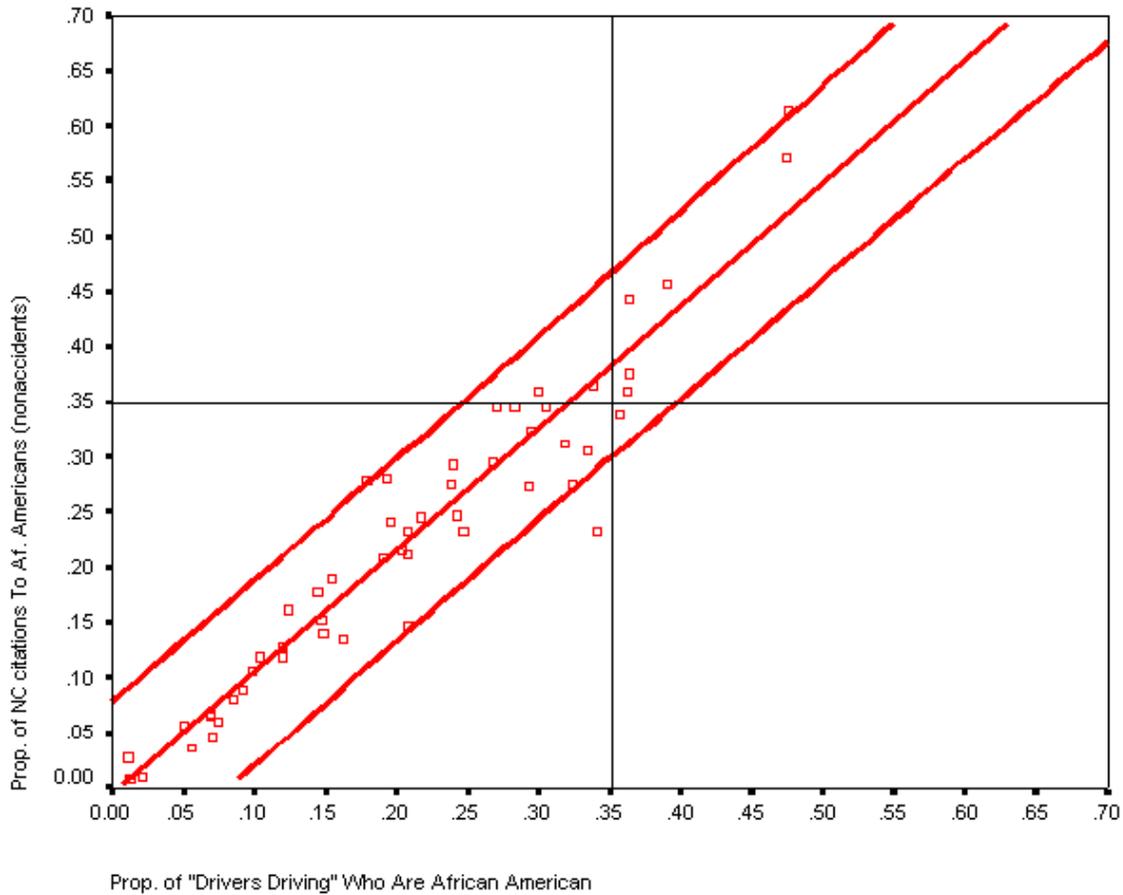
range such that one could not claim that any one observation is different statistically from the estimated point on the middle line.

At first glance, the evidence of Figure 2.2 seems to indicate a remarkable similarity between the proportion of residents and citations involving African Americans. The correlation here is .941. However, there are four points to consider in analysis of these data. First, seemingly small general differences are not assumed to exist in a specific district. For example, there is a district with only .18 African American residents but that has .27 of drivers cited as African American. Such a district should not be ignored. Nine percent more of those cited are African American than who are African American residents. Second, the intercept value (where the regression line crosses the vertical axis) is approximately .04, indicating that more African Americans are cited than are resident by about 4 percent. Thus, the overall disparity evident in the figures is in the direction of excess citations of African Americans. Third, it is important to bear in mind that there are only fifty-three observations in this figure, such that the slope of the regression line could change with only a few observations “tipping the balance” one way or another. More racial disparity, as defined as distance from the regression line shown here, could be the consequence in some districts, less in others. Fourth, the data on the fifty-three districts are aggregated from all the smaller areas in the districts. There could be areas within a district where African Americans are treated disparately, but we cannot determine that using the aggregated numbers presented in the figure because they represent an average for the whole district. We will take up this theme in greater detail below. Thus, in summary, the information in the figure is hardly sufficient grounds to terminate further evaluation, but rather it gives us reason to look further.

Figures 2.3 and 2.4 show the results for our estimate of “drivers driving” and drivers in accidents (proportion who are African American), respectively. Figure 2.3 seems at first glance to look a lot like Figure 2.2. There are several differences, however. For one, the slope is steeper, indicating that as the proportion of estimated “drivers driving” who are African American increases, the proportion of cited drivers who are African American becomes even higher than is the case for changes in the resident proportion African American. For example, compare the intercept value, which is near zero, with the value of the cross of the two reference lines at the .35 proportions. We can see that the regression line estimate lies approximately .03 above the point where the two reference lines cross. Again, as was the intercept value in Figure 2.2, the direction of bias is one slightly detrimental to African Americans. The higher the proportion of drivers driving who are African American, the greater the disparity in the citations to African Americans.

In Figure 2.4, we have the scatter of observations in a regression with proportion of drivers who are African American as recorded in accident data compared to proportion of drivers cited who are African American. Here we see yet a different pattern. This time the comparison of the reference line cross point with the predicted value from the regression line shows an even larger discrepancy, roughly .06, and the disparity grows with the higher proportion of African Americans among the drivers in accidents. That is, where there are more African Americans on

Figure 2.3 Proportion of Citations Issued to African Americans by Proportion of “Drivers Driving” Who Are African American, by Troop District (N=53 Districts)

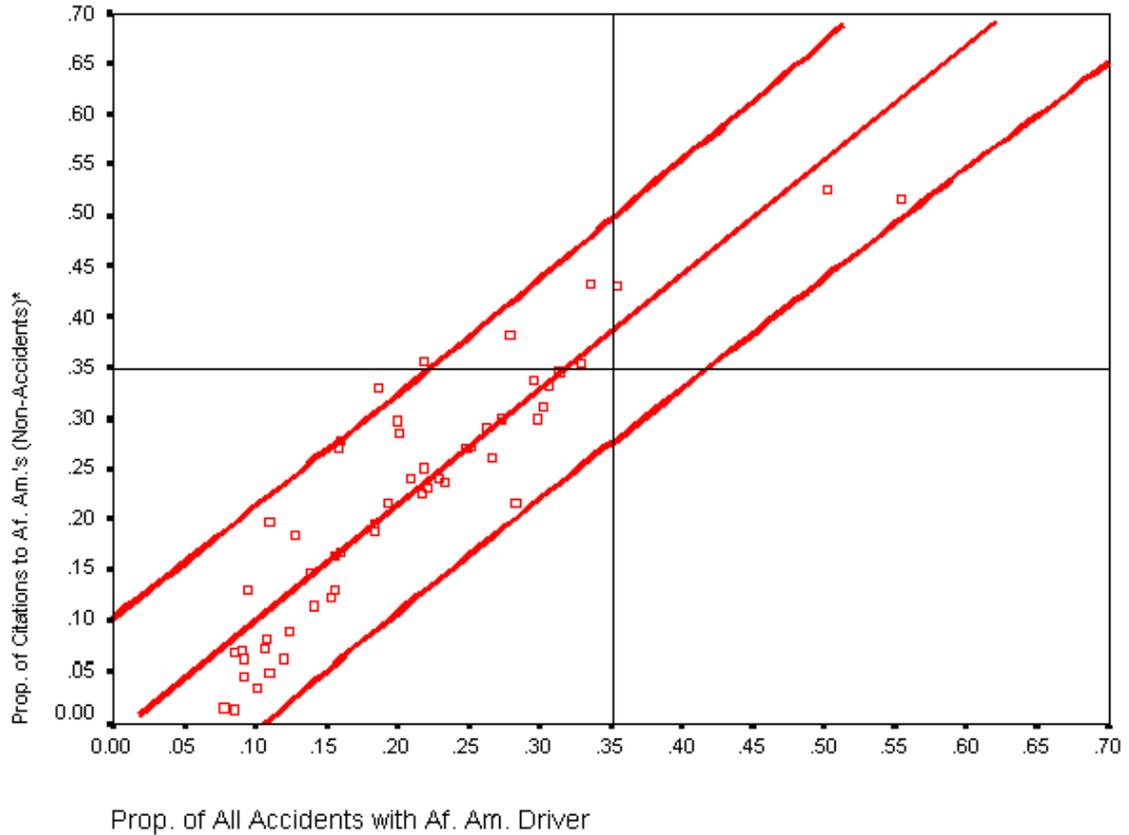


the highway (as measured by this baseline), there is a larger disparity in the proportion of African Americans cited, relative to the proportion in accidents. Thus, it would appear that, despite the high correlations indicating that approximately 90 percent of the variance in the proportion of drivers cited who are African American can be accounted for with one or the other of the three proxy measures of behavior in the figures above, there are nevertheless reasons to examine the data further because they are suggestive of racial disparity and possible racial bias. We need to

see if there is more conclusive evidence of bias than has been presented thus far, since we have been just scratching on the surface of the data. It should also be noted that the evidence lies in the direction that some might hypothesize using a “racial threat” hypothesis: the more African Americans present in an area, the greater the tendency for them to be issued citations. However, we must also point out that we have been looking at the data in a rather superficial way, and that further analysis is necessary at smaller areal levels to justify such a claim.

When we use the term “superficial,” we mean that there are several basic reasons to question the relationships between the variables in the figures above. For example, some of the districts having the highest proportion African Americans are in urban areas where many of the accidents are handled by the local police, thus possibly driving down the number of accidents handled by the NCSHP. That alone would not affect the proportion African American involved in accidents, except for the fact that it is possible that the NCSHP “work” accidents and patrols somewhat differently. They may patrol some highways where the local police typically write up the accidents, yet the NCSHP still issues citations in those areas. They may handle all of the accidents on all highways in more suburban areas, where the more dangerous local roads are responsible for a higher proportion of accidents relative to that of the busier highways (where more tickets are written). That is, the bulk of the patrolling may occur on the busier highways, resulting in a discrepancy between the proportion in accidents who are African American and the proportion cited who are African American.

Figure 2.4 Proportion of Citations Issued to African Americans by Proportion of Drivers in Accidents Who Are African American, by Troop District (N=53 Districts)



* Includes Out of State Drivers

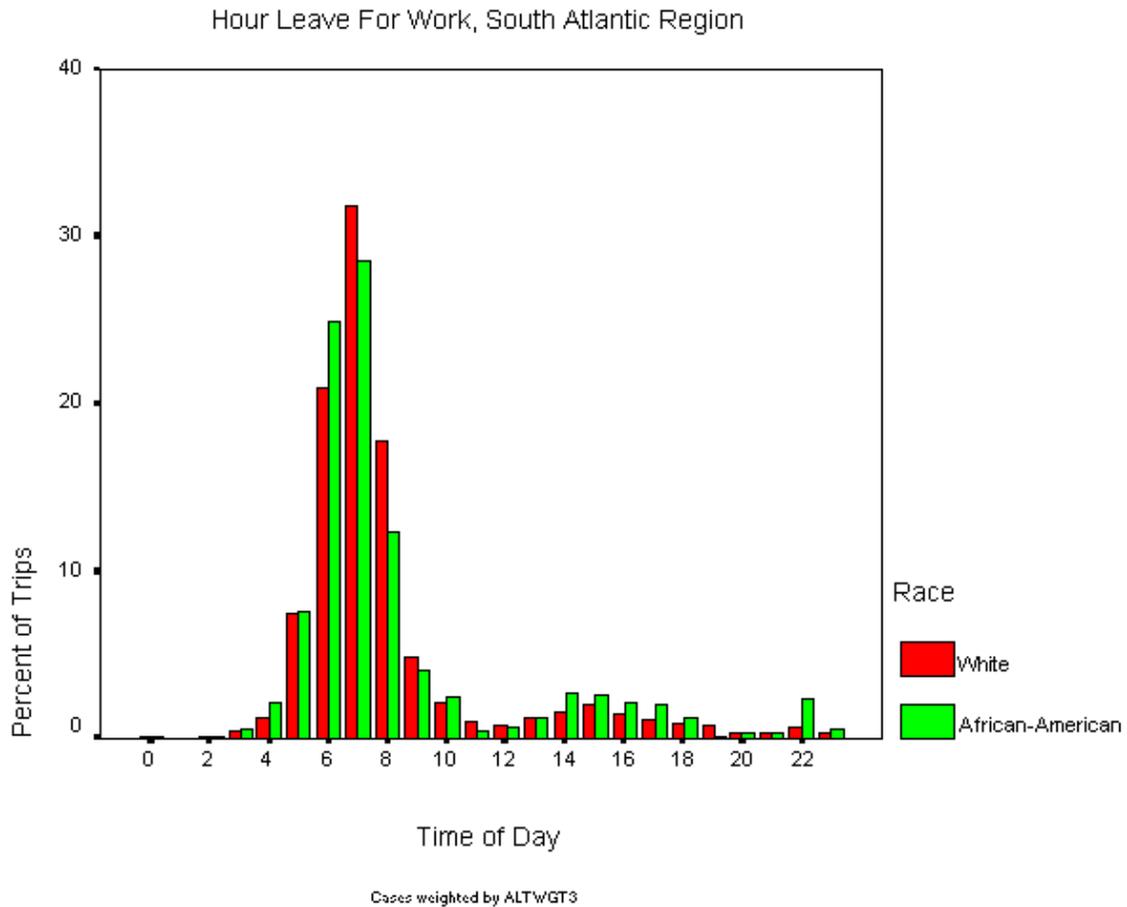
More important than the relative locations of accidents and citations in urban areas, however, is the fact that there is variation by time of day in the proportion of drivers who are African American and in the patrolling of highways. In the more urban areas or where an interstate passes through, there is relatively more patrolling at night, when African Americans are more likely to be on the highway. This driving pattern is not a well known fact, so we need to establish it.

Nationwide Personal Transportation Survey

The data presented in Figure 2.5 are taken from the 1995 Nationwide Personal Transportation Survey (U.S. Department of Transportation, 1997). The purpose of that study was to assess the amount and nature of personal travel in the United States. The figure represents the time of day that respondents who live in the South Atlantic states leave for work, by race. White Americans are more likely to be leaving for work during the hours of 5 a.m. and 9 a.m. than are African Americans. Between the hours of 10:00 a.m. and 1 p.m., there is a steady decline in whites leaving for work, while at the same time there is an increase in African Americans leaving for work. From 2:00 p.m. until approximately 6:00 p.m., African Americans are slightly more likely than whites to be leaving for work. After 6:00 p.m. until about 9:00 p.m., there is not much difference in the time that white and African American respondents are driving to work. Finally, between the hours of 10:00 p.m. and 11:00 p.m., African Americans are again more likely to be driving to work. Overall, the pattern indicates that African Americans are slightly more likely than whites to be driving to work during late-night hours and mid-afternoon hours.

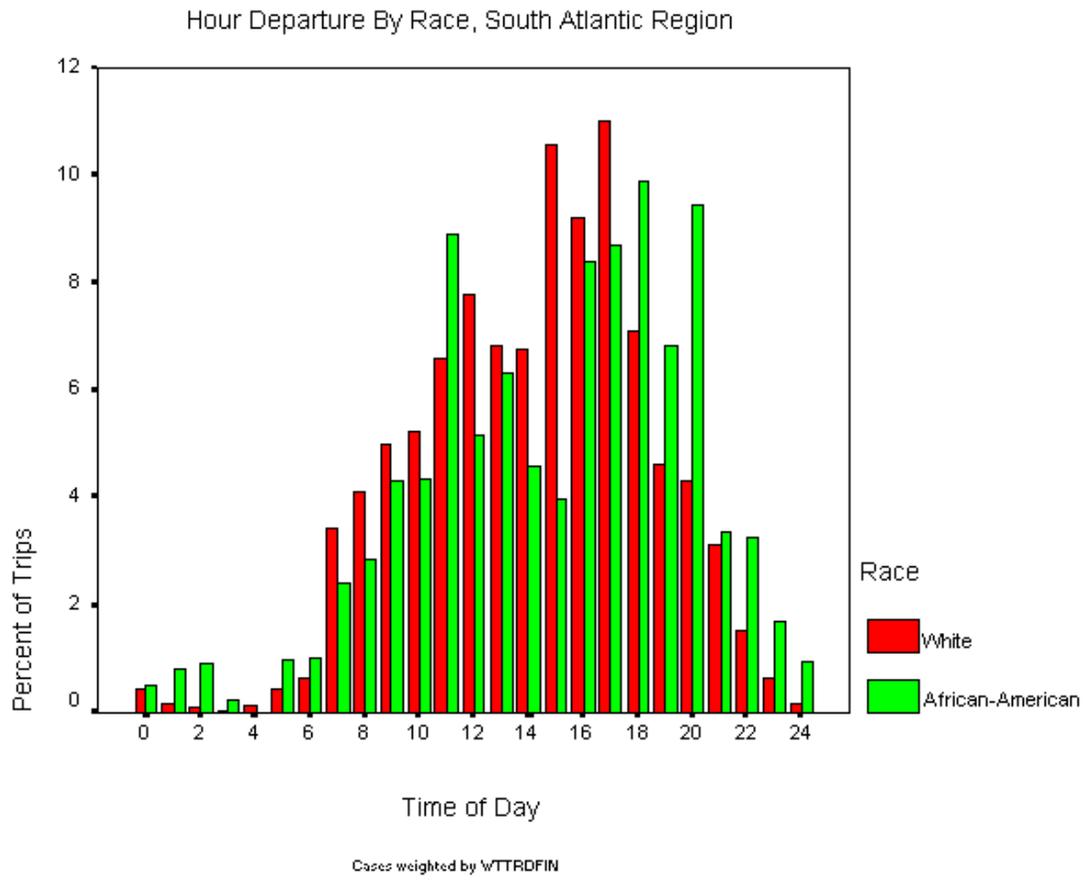
Figure 2.6 shows the time of day by race that respondents are traveling for any purpose (not just work). There appears to be even larger differences in the times of day that white and African American respondents are on the road than were observed above for leaving for work. African Americans appear to be leaving home in the early morning hours and late night hours more frequently. Between the hours of 12:00 a.m. (midnight) and 5:00 a.m., they are more likely to be on the road. This trend significantly decreases between the hours of 7:00 a.m. and 10:00 a.m., when whites are more likely to be leaving home for some type of travel. Further, between

Figure 2.5 Hour of Departure of Trips to Work by Time of Day, South Atlantic States, 1995 Survey



the hours of 12:00 p.m. (noon) and 5:00 p.m., whites also more likely to be on the road. After 5:00 p.m., the trend swings back to the greater representation of African Americans. Overall, much like for work travel, the pattern indicates that whites and African Americans are on the road at significantly higher rates during different times of the day, with greater African American representation at night and in the early morning hours.

Figure 2.6 Hour of Departure for All Trips, By Race and Hour of Day, South Atlantic States, 1995 Survey

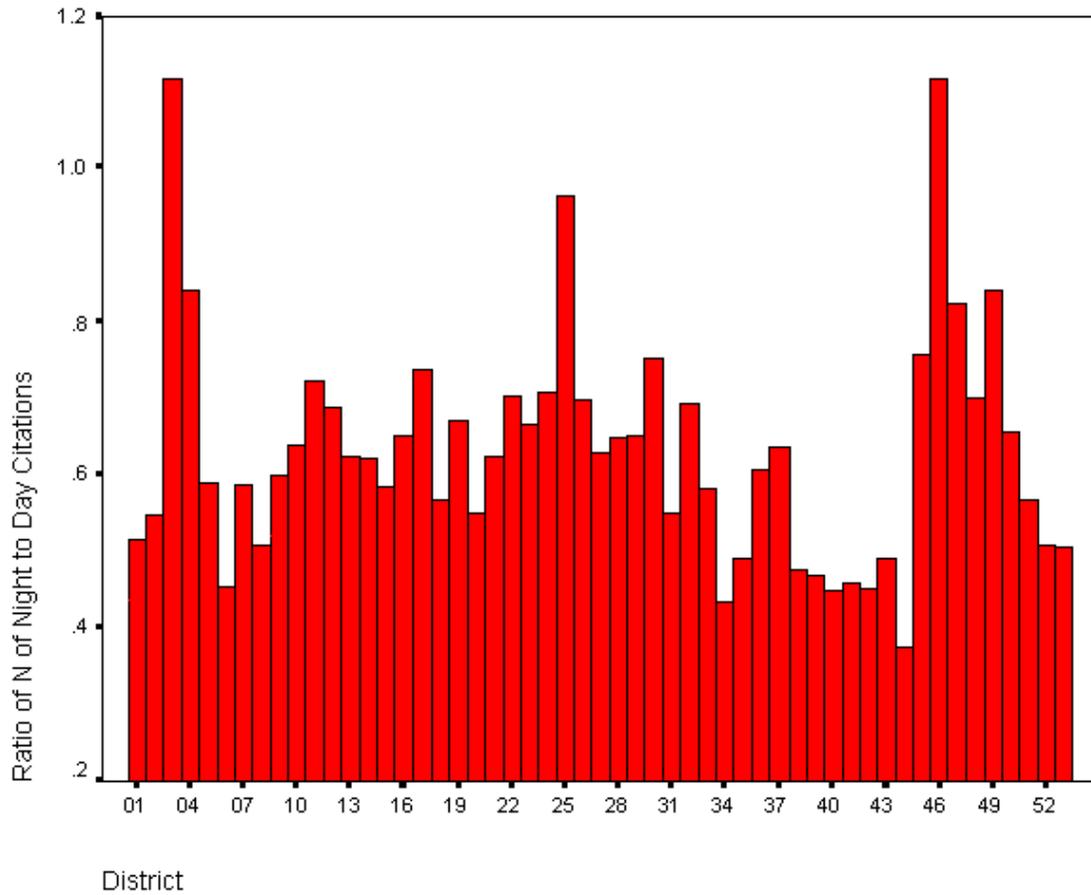


One more point needs to be made regarding the citation rate of African Americans at night versus during the day. One might argue that the NCSHP actually makes decisions to patrol more at night in some areas as a result of racial bias or cognitive racial bias. However, in an analysis not presented here, we found that the accident rates generally determine the citation rates across districts and across time of day, so it would not seem likely that race is involved in the decisions to patrol more or less at night, although we cannot rule out the possibility that some small differences that are observed may be accounted for by such bias.

As can be seen in Figure 2.7, the ratio of night to day citations (number of) substantially varies across districts. In Districts 3 and 46 (not their actual district numbers), for example, there are more citations issued at night than in the day, whereas for most districts the majority of citations are issued in daytime. Since we know that the proportion of African Americans on the highway varies with the time of day and the amount of citations written in a district varies across districts by time of day, it is necessary to control for time of day in the analysis. It should also be noted that although the involvement in accidents of African Americans (and whites) increases at night, they increase more for African Americans. Thus there is some explanation for an increase in the proportion of citations issued to African Americans at night. However, it should be noted that further information could be useful in evaluating the question of whether or not the amount of patrolling at night is warranted by the volume of accidents at night.

In the analysis below, we will present results separately for day and night time stops/citations to control (at least crudely) for the variation in the time of day differences across races. Before doing so, however, we need to discuss another possible explanation of disparity in

Figure 2.7 Ratio of Night Citations to Day Citations, Fifty-Three Troop Districts



the proportion cited and the proportion driving who are African American, what we call “spatial heterogeneity” (Smith et al., 2000).

Spatial Heterogeneity

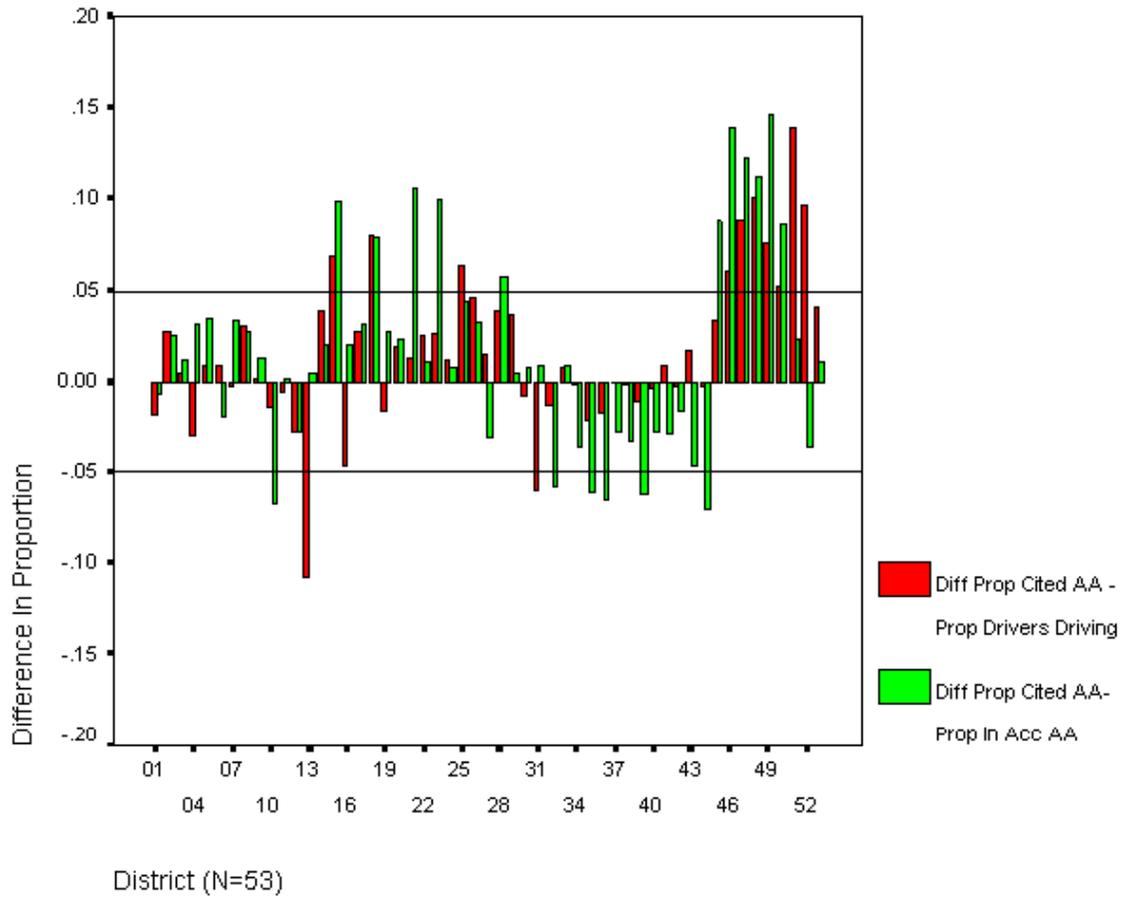
Troopers patrol areas where the highways have many accidents and where there are relatively many violators of traffic laws. They also patrol where it is more convenient to patrol (there are turn-around capabilities— for example, overheads, or no guard rail or fence between

opposing-direction-bound lanes, and other factors). The NCSHP's choice of specific locations may also be made based on the prevalence of certain types of citizen behaviors— such as speeding. If drivers tend to speed on some highways more than others, and those highways have more accidents or are simply more convenient to patrol, there will be more patrol activity, and more citations will be issued there.

In Figures 2.2 through 2.4 above, we saw that whether one used the proportion of licensed drivers who are African American, or the proportion of “drivers driving” who are African American, or the proportion of drivers in accidents who are African American, the correlation with proportion of cited who are African American is very high (.93 through .96). Figure 2.8 shows the same variables in a slightly different format (although we omit the information here on residents as it is very similar to that of “drivers driving”). Here the proportion of “drivers driving” who are African American is subtracted from the proportion cited who are African American. Similarly, the proportion of drivers in accidents who are African American is subtracted from the proportion of cited drivers who are African American. These difference scores show essentially the same information as in Figures 2.3 and Figures 2.4 above, only here we examine difference scores, whereas above we examined a regression of one measure of “proportion African American” on the proportion of cited who are African American. In Figure 2.8, however, it seems easier to “see” the differences in terms of disparity unfavorable to African Americans and disparity in the opposite direction.

What we see in Figure 2.8 is that several districts have differences in the proportions unfavorable to African Americans larger than .05 (for example, Districts 15, 18, and most of the districts between District 44 and 52). Because there will almost always be some differences between the measures in question and unknown sources— or what is called “random error”— we

Figure 2.8 Differences Between Proportion Cited who Are African American and Proportion of ‘Drivers Driving’ Who Are African American and Proportion Cited Who Are African American and Proportion Drivers in Accidents who Are African American



need to adopt some standard of difference below which we will call “likely measurement error” (and which we will ignore here for the sake of discussion), versus a difference that we define as large enough to require further investigation.¹⁶ Here we use a difference of +/- .05 somewhat arbitrarily, but too small a difference would lead to a large number of false positives and false

¹⁶ We say “likely measurement error” but we do not formally derive the estimate of what is likely measurement error, but rather assume that the relatively high variations observable in the figures are more likely to be areas where bias might be occurring in decision making.

negatives. The +/-0.05 criterion gives us a basis for making comparisons in some further analysis below.

In the present graph, we are interested to see if the pattern of “positives” and “negatives”— districts with difference scores above .05 and districts below -.05, respectively— are similar across the two measures. They are somewhat distinct, in that the difference scores with the accident measure tends to identify more positives and more negatives than the difference score based on “drivers driving”(ten accident difference scores are above .05 compared to four “drivers driving” difference scores; while six negative difference scores are below -.05 compared to only two “drivers driving” scores). But the accident difference score (again comparing the proportion cited who are African American to the proportion of drivers in accidents who are African American) also has more “negatives”—districts with less than -.05 difference scores (in other words, fewer African American cited than in accidents—six to two). What the reader cannot learn from the graph is that the districts (which have an arbitrary identifier code attached to them to identify them only to the researchers) are generally geographically adjacent. So, for example, most of the districts to the far right of the graph are near one another geographically, as are many of the districts with difference scores in the middle half of the chart. The implication of this is that the patterns of positives and negatives are likely to be due to some shared unmeasured characteristic (perhaps some organizational or geographic characteristic associated with the difference score variables or their component variables). Ideally as researchers, we would like to identify those characteristics.

The comparison in Figure 2.8 of “drivers driving” and accidents as a baseline against which to evaluate the proportion issued citations who are African American, indicates that more districts qualify as “positives” using accidents as a baseline. For that reason (to be conservative

in testing the racial disparity hypothesis),¹⁷ as well as the fact that accident data are available for measurement at relatively small units of analysis, we will proceed to analyze further the accident data, and return to a discussion of drivers driving and resident licensed drivers at the end of the chapter.

The possible advantage of small units of analysis centers around the question of whether patrolling occurs evenly across geographic areas within districts compared to the distribution of African American drivers (and their driving behavior) within districts. The issue we raise is that there may be a mismatch of where patrolling occurs (too little or too much) relative to where African American drivers drive. There is a possible mismatch if “spatial heterogeneity” occurs in both the geographic distribution of African American drivers and in the distribution of patrolling.

For illustration purposes, Figure 2.9 below shows an area of Nash county in North Carolina, where two major highways intersect, I-95 and U.S. 64. (the other roads on the diagram are fictitious). The proportion of drivers who are African American among those we define as “threshold speeders” is presented in the figure. We traveled each of these highways for a week as part of what we call our observational baseline study (see Appendix A) to determine which proportion of drivers there drove at speeds higher than the local “speeding threshold.” The speeding threshold is defined as the speed at which a citation for speeding was likely to be issued (this we define more precisely as the median speed at which drivers were issued citations). The proportions of African American drivers at or above this threshold are shown for each highway, and they differ substantially (12 percent absolute difference). The aggregated data for Nash County of the number of citations of African American threshold speeders who were issued

¹⁷ By conservative here we mean that we would prefer to err on the side of rejecting the null hypothesis of no racial difference.

citations would be greatly affected if it happened to be the case that there was more patrolling of I-95 than U.S. 64 (the proportion of citations issued to African Americans would be lower), or if there was more patrolling of U.S. 64 (the proportion of citations issued to African Americans would be higher). If the patrolling was evenly distributed across highways, there would be no “spatial heterogeneity” issue, as the overall rate comparison would be based on a composite average of the citations and the speeding drivers.

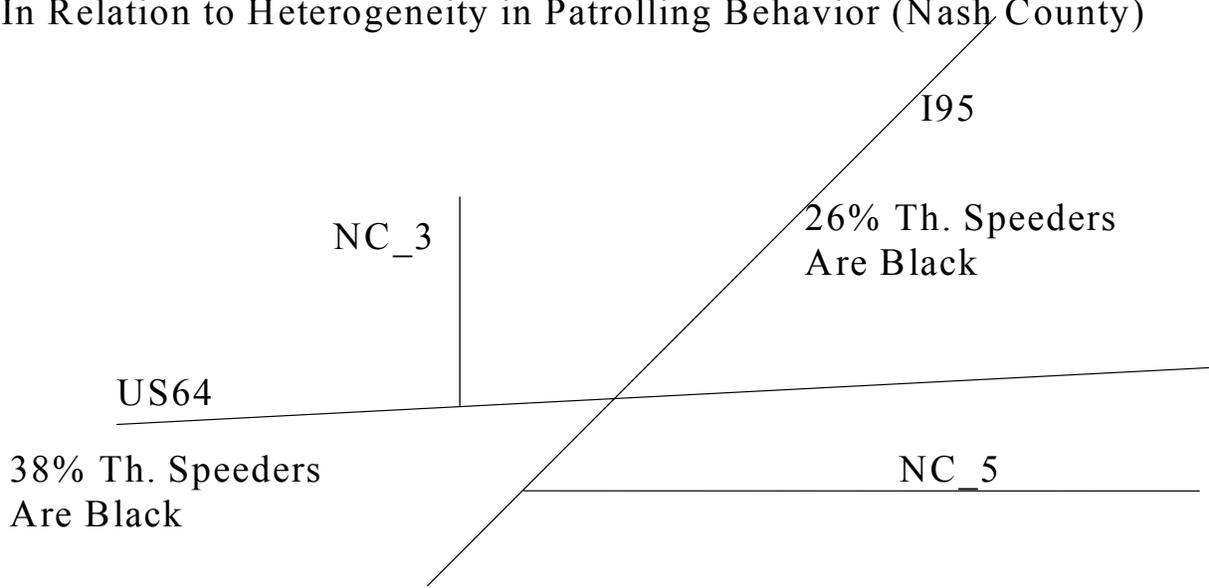
The highways in Figure 2.9 are two of fourteen that we have observed (see Appendix A). In the four counties where we observed two different highways, we found that the proportion of threshold speeders who are African American varies substantially between the two highways, with an average difference of about 5 percent. In other words, there is considerable spatial heterogeneity in the distribution of African American speeders between highways. Since our measure of their speeding is reasonably accurate and objective (in other words, based on observation, not on official records), we must conclude that there is little doubt that there is variation within a district in the racial composition of speeding drivers. (We will show below that patrolling varies considerably from highway to highway.) Thus, comparison of rates at the level of county or district (such as the fifty three districts in the figures above) will be prone to biased estimates in the face of a mismatch of where drivers drive and where the NCSHP monitors vehicles. This mismatch is a manifestation of the more general problem in areal analysis of “spatial heterogeneity.”

To address the questions of the mismatch of where drivers drive and where the NCSHP looks for violations (spatial heterogeneity), we present difference scores based on small units of analysis, the highway area (as defined above). It consists of a stretch of highway covering a portion of a county (usually about a third of a county). The advantage of the highway area as a

unit of analysis is that we have at least two reasons to believe that the mismatch of drivers and patrolling discussed above is lessened by studying highway areas as a unit of analysis rather than larger units of analysis such as counties or districts. First, we have examined the distribution of the proportion African American cited along stretches of highway within areas where mile markers exist, and where they are recorded in the citation data base, as compared to the proportion on intersecting highways. We find greater consistency (less heterogeneity) in the proportions along stretches within the county areas. Secondly, we know that patrolling often takes place in the form of driving up and down rather long segments of highway (we rode with troopers and observed troopers in their routines of patrolling), thus providing some evidence that areas of highways are patrolled in segments. Although far from ideal in terms of testing a “mismatch” hypothesis, we think that there should be less of a mismatch between trooper patrolling and vehicular driving behavior within smaller geographic units of analysis (the smaller the unit of analysis the less mismatch is likely).

Figure 2.9 Two Major Highways in Nash County, North Carolina

Example of Spatial Heterogeneity in Threshold Speeding Behavior In Relation to Heterogeneity in Patrolling Behavior (Nash County)



If we had more observations, we could define an extremely small unit of analysis, such as the area of highway between intersections. For these extremely small units of analysis, presumably the demographic make up of that segment of highway would be homogeneous across its span (since by definition no turnoffs exist).¹⁸ Within such a unit there would be a decrease in the number of issues associated with spatial heterogeneity. Essentially, the amount of patrolling (frequency of patrolling) of a highway segment would be assumed to be equal between the two intersections, allowing us to compare the proportion cited who are African American to the

¹⁸ Thus once a vehicle enters a stretch of highway at an intersection, the vehicle cannot exit the highway until the next intersection (except in the rare instance of a U-turn on the highway).

proportion involved in accidents on those same segments of highway. We do not have such data, however.¹⁹ We only have data statewide for the accidents and citations in a county highway area.

One problem with studying highway areas, however, is that we must be concerned with a small number of observations within a highway area. There can be either too few citations or too few accidents in an highway area to justify comparing the two proportions (African Americans cited to those involved in accidents). For purposes here, we find that requiring five or more citations and five or more accidents, and then constructing weighted averages of all those highway areas meeting those criteria, gives us the same results as requiring twenty or thirty citations and twenty or thirty accidents in a highway area.

To demonstrate that the “five or more” restriction does not lead us to radically different conclusions, we compare the difference scores counting all accidents and citations versus the aggregated difference scores based on a minimum of five. Figure 2.10 shows the relationship

¹⁹ It is not obvious that a geographic unit of analysis defined as a stretch of highway between two intersections (they have been called “face blocks” in other research) would not suffer from possible spatial heterogeneity concerns. Assuming that a patrol car that enters such a geographic area must exit it, however, it can be argued that all parts of the area are equally patrolled, and that over time the proportion African American cited for violations should be constant or homogeneous within each unit (face block). There could be variation across areas (face blocks) in the extent of patrolling, as some areas are patrolled more often than others. However, when all the areas of a county or district are aggregated to the county or district level, and weighted by each area’s contribution to the count of citations and accidents, the difference between the proportion cited who are African American and the proportion in accidents who are African American should be unbiased. Take an extreme case where NCSHP rides up and down one highway in a county, ignoring all others in the county (except when they are called to accidents). One might think that in such a situation the estimate of proportion cited who are African American would be biased relative to the proportion in accidents who are African American, but if the data are weighted by the contribution of citations and accidents, there could only be an estimate based on the highway patrolled, since there could be no calculation of the proportion of those cited who are African American in the other districts (no citations occurred there, and division by zero is impossible). Take another example, where 90 percent of the citations occur on one highway in a county, and 10 percent are scattered across other highways. Again, because the data are weighted when aggregated to the county level, and because the unit of analysis ensures homogeneity within units, there will be no bias in the estimates.

between the aggregated difference score measures using weighted averages of the proportion cited who are African American minus the proportion in accidents with African American drivers (aggregating all highway areas with five or more citations and five or more accidents), as compared with the difference scores between citations and accidents for the districts as a whole (for example, what we presented in Figure 2.9 above). The weight used for the “five or more” difference scores is the sum of the number of incidents (citations plus accidents) in a district divided by the total number of incidents (citations plus accidents) for the district. Our intent with the figure is to illustrate that the dropped highway areas do not have a large impact on the two types of difference scores compared here (all accidents versus five or more accidents/citations in a year). Yet, even though there is little overall difference in the proportion African American using all accidents or five or more per year, results vary for specific districts, depending on which difference score is used. For example, in District 18, the aggregated difference score results in an 8 percent higher estimate for citations to accidents using the “five or more” restriction than do the aggregated counts, which are then calculated as proportions and then subtracted from the proportion of citations issued to African Americans.

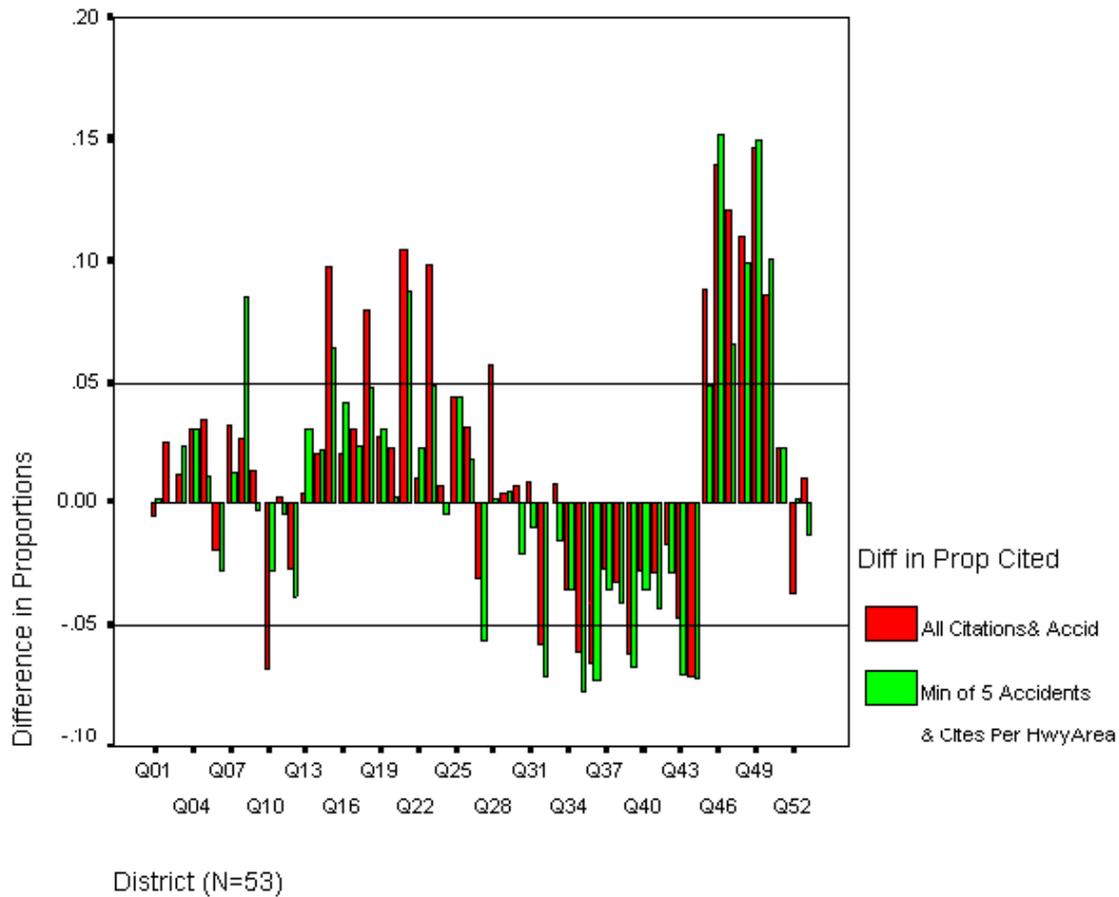
It should also be noted that the requirement of “five or more” citations and accidents is designed to limit the variation in the error when calculating “proportion African American cited or proportion African American driving vehicles in accidents,” but is in no way intended to be a sufficient empirical basis for comparing proportions for any one highway area. Rather, comparisons are made using the aggregated highway area data (summing across highway areas within a district to the district level), as opposed to differences for any one highway area. By aggregating the highway area differences and weighting them, the district comparisons are predominantly affected by the highway areas with the larger numbers of incidents (citations and

accidents). When the highway areas are aggregated to the district level, hundreds of highway areas are typically aggregated and weighted by the proportion of all the citations and accidents each highway area represents. The effect of these weights is to limit the amount of error generated for each district with small numbers of observations.²⁰

In summary, thus far we have seen that time of day and the mismatch of patrolling and driving in local areas can lead to differing results in the comparison of driving behavior to the citation receiving experiences. What remains is to combine the aggregated difference scores at the highway area level separately for daytime and nighttime, so that we control for both time and space mismatch of patrolling and driving behavior (as measured by accident involvement).

²⁰ We tested for whether or not 1 or 5 or 10 or 15 cases or up to 200 made any difference to the analysis by comparing correlations between the proportion cited who are African American and the proportion drivers in accidents who are African American, and found that the correlations deteriorated below .500 when less than 5 citations/accidents were used. The number of highway areas drops from 10,474 to 3,233 when we require 5 or more citations and accidents, so we lose information on a substantial number of highway areas where there is little action (accidents or citations). We know of no a priori reason to believe that bias should be exercised in less trafficked areas than in more trafficked areas. Also, when we examine the data are large units of analysis, the racial disparity is not large in absolute terms, so it is very doubtful that racial bias is occurring in less busy places.

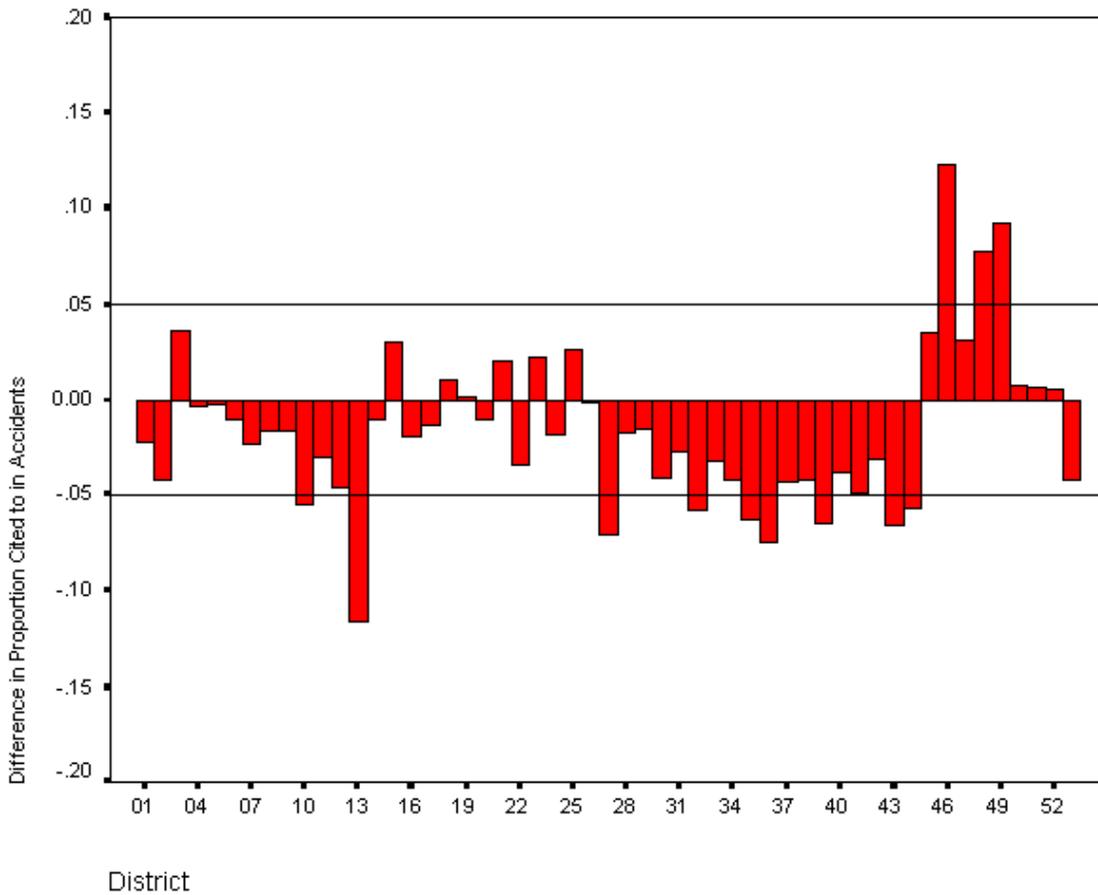
Figure 2.10 Comparison of a Baseline with All Accidents and a Baseline with Highway Areas of 5 or More Accidents and 5 or More Citations



Day and Night Rates

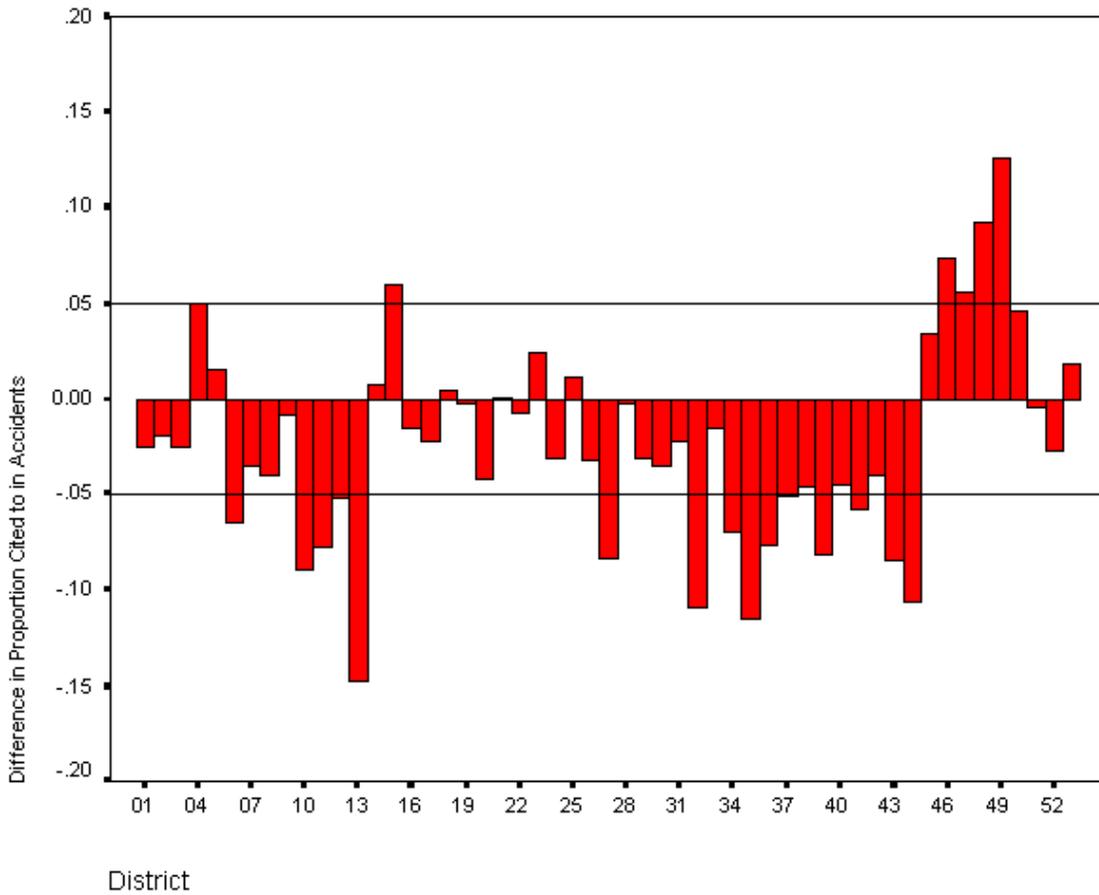
In the analysis below, we distinguish between citations and accidents occurring between 6 p.m. and 6 a.m, (night) from those occurring between 6 a.m. and 6 p.m. (day). Figures 2.11 and 2.12 illustrate the proportion of citations issued to African Americans and the proportion of accidents involving African Americans for the fifty-three districts for day and night times, respectively. In Figure 2.11, the differences in the positive direction that we observe are substantially fewer in number than we observed previously in Figure 2.10. That is, examining

Figure 2.11 Difference in the Proportion Cited African American and the Proportion in Accidents Who are African American, Daytime Only (Min of 5 Accidents and 5 Citations Per Highway Area)



daytime rates and controlling partially for spatial heterogeneity mismatches result in only 3 districts having greater than a .05 difference score (proportion of those cited who are African American minus proportion in accidents who are African American). Examining negative difference scores, there are now nine districts with less than -.05 difference score values (indicating that African Americans are under-represented among the ranks of those issued citations in those areas).

Figure 2.12 Difference in the Proportion Cited African American and the Proportion in Accidents Who are African American, Nighttime Only (Min of 5 Accidents and 5 Citations Per Highway Area)



In Figure 2.12, the difference scores are compared across districts for citations and accidents at night. Here, there are six districts with difference scores of .05 or higher, while there are fifteen with negative difference scores of -.05 or lower. Again, we are controlling for time because we are limiting the observations to nighttime (again 6 p.m. to 6 a.m.). Looking at difference scores calculated at the highway area level and aggregating to the district level, we see yet a different evaluation of the districts than that which we observed by simply comparing the differences in the proportions at the district level.

What are the Mechanisms that Generate Positives (and Negatives)?

Several of the districts in Figures 2.11 and 2.12 show racial disparity in the proportion cited who are African American relative to the proportion in accidents who are African American, and several of these districts are geographically adjacent to one another. Not visible in the figures is the fact that several are part of the same troop. This raises several questions about these districts. Are these districts home to troopers who engage in some form of racial bias? If so, how does the bias manifest itself? Can we determine if the bias is likely to be at the individual trooper level, or can it be accounted for at the aggregate level? Furthermore, is it inadvertent, or possibly even rationally based (for example, the result of deployment to highways with high accident rates)? Still, one need to address the districts in the figures with a negative differential (less than -.05) in proportion cited who are African American to the proportion in accidents who are African American (those areas with too few African Americans issued citations)? What could be accounting for the “reverse discrimination” pattern? Could it be some form of reverse racial bias or aversive bias (avoidance of African Americans) or even attempts on the part of some troopers to avoid the appearance of racial bias by giving breaks to African Americans by issuing them fewer citations?

We will address possible racial disparity on the part of individual troopers more directly in the next chapter. Here we consider further subtleties in the processes being studied here. We begin by examining the six positives in Figure 2.12 above (nighttime positives), Districts 4, 15, 46, 47, 48, and 49. One possible explanation for these positives has to do with where patrolling occurs relative to where accidents occur. We discussed earlier that there is a tendency for the NCSHP to “go where the action is” in the sense of traffic, rather than patrol where the accidents are (so as to prevent accidents through their greater visibility in the area). Table 2.3 shows the

breakdown of accidents and citations for the six districts with more than a .05 difference in proportion cited who are African American to proportion in accidents who are African American. The percentage of all accidents and of all citations occurring on the road type is presented. Thus, for example, in District 15, 8.2 percent of the accidents occur on the interstate, whereas 13.5 percent of the citations occur on the interstate. In general, there is a mismatch between where accidents tend to occur and where troopers patrol (almost one-half of the accidents occur on rural paved roads across districts [District 49 excepted], whereas about one-quarter of the citations [varying up to 33.8 percent] occur on rural paved roads, suggestive of under-patrolling of rural paved roads). Of course, this does not mean that the NCSHP troopers should change what they are doing and focus on rural paved roads. Rural paved roads are numerous and constitute the most frequent type of road and the most cumulative road miles (data from the N.C. Department of Transportation, not reviewed here, indicate this to be the case). That is, it may be inefficient to patrol such highways because they are too dispersed, whereas the N.C. highways, U.S. highways, and interstates are not as dispersed (indeed, they are often the central traffic arteries of an area.).

In three of the six districts shown, the type of road most heavily patrolled is the type of road with the highest or second highest proportion of drivers in accidents who are African American. Thus in essence, the NCSHP “over-patrols” the type of highways in these districts where African Americans are more prevalent drivers. This over-patrolling could account for much of the positive difference between proportion cited and proportion in accidents who are African American. In two of the remaining three districts, there are small differences in the proportion of citations issued to African Americans across highway types, and in both districts there is a relatively high percentage of rural paved roads patrolled (about a third of the citations occur on these roads and the proportion cited who are African American is substantially higher

than the proportion in accidents). While this could be the result of racial bias, another possibility is that the daytime rush hour traffic, which is predominantly “white” (assuming the National Transportation Survey results hold here), accounts disproportionately for many of the accidents, driving down the proportion of African Americans involved in daytime accidents. By contrast African Americans are somewhat more likely to be on the road in off-rush hour times – such as at night, when accident rates are generally lower. See Appendix G where we discuss the fact that the ratio of accidents to injuries varies considerably from high-volume traffic roads to less-volume roads: the “fender-bender” type accident may be more numerous in rush-hour traffic, but also more likely to involve whites. This is a possibility that warrants further exploration.

One exception to the general pattern in Table 2.3 is District 49, in which virtually all of the accidents are occurring on the interstate, and citations there are relatively less than called for by the accident rate. Still, 81.6 percent of the nighttime citations in the district are written on the interstate. In this district, citations are also issued on U.S. highways, and both the interstate and U.S. highway citations tend to drive up the proportion African American number cited. We are not sure what could be the cause (racial bias or something else), but, again the predominantly white rush hour may be accounting for a high percentage of the accidents, driving down the proportion African American rate.

Table 2.3 Percent of All Accidents and Percent of All Citations by Road Type, Six “Positive” Districts, Nighttime

District	Interstate		U.S. Highway		N.C. Highway		Rural Paved Roads	
	Accid’s	Citation	Accid’s	Citation	Accid’s	Citation	Accid’s	Citation
4	-	-	37.2	46.5	28.4	31.8	34.4	21.7
15	8.2	13.5	20.4	35.9	29.8	31.3	41.6	19.2
46	-	-	38.8	58.1	6.4	8.4	54.8	33.5
47	-	-	25.5	59.5	18.0	14.1	56.5	26.4
48	4.4	7.8	12.1	35.8	37.4	22.6	46.2	33.8
49	99.0	81.6	.6	8.3	.2	8.6	.2	1.5

Read: “37.2 percent of all accidents in District 4 occur on U.S. highways, whereas 46.5 percent of all citations occur on U.S. highways in that district.”

For nighttime differences in the proportion of those cites who are African American relative to the proportion in accidents who are African American, the results for the districts with positive difference scores greater than or equal to .05 (not shown here) also reveal a relatively high percentage of the drivers in accidents who are African Americans on the highway types where the highest proportion of citations are issued. The patrolling in these districts at night time is most often (modal category) on interstates or on U.S. highways, and it is on these same highways that African Americans have a relatively high representation in the ranks of those involved in accidents (indicating presence on the highway, and to some extent culpability or driver error).

Citation Zones as a Causal Mechanism

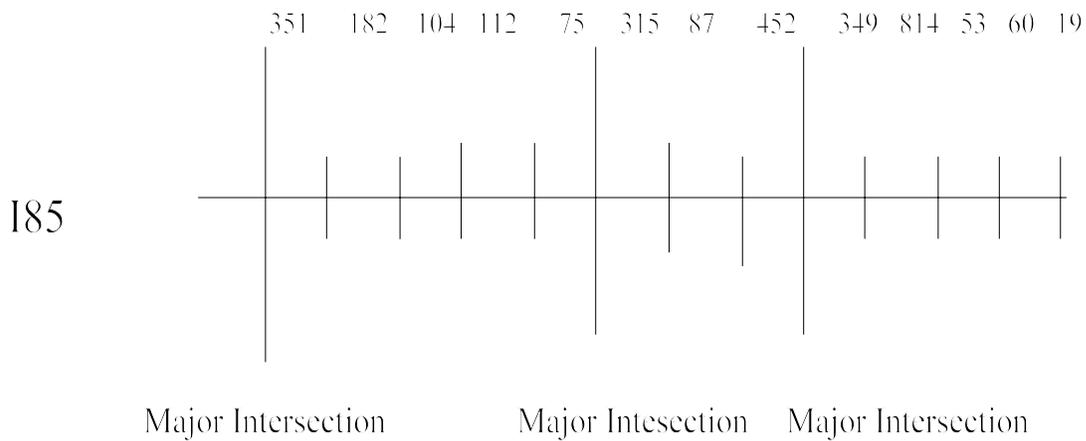
In addition to variations in rates on road types within districts and within what we define as “daytime” and “nighttime” hours, we think that it is useful to consider the importance of “citation zones” in the citation process within a stretch of highway. That is, there is a very local geographical distribution of citations, and examining them may shed light on how deployment might account for the positives and the negatives in the figures above.

In Figure 2.13, we review a stretch of I-85 in North Carolina that has several major intersections. The hash marks indicate mile markers, and the longer lines indicate intersections. What is striking about the figure is the variation in the number of traffic stops across miles of highway. They vary considerably from 814 to nineteen (with only 2 miles separating these two 1 mile stretches of I-85). The mechanism by which stops occur in the district where this segment of I-85 is located, is probably a function of deployment (defined simply here as where the NCSHP troopers stop vehicles) and also somewhat on the behavior of those vehicles. Notice how the number of citations are substantially higher near the major intersections. This is not because the density of traffic is necessarily higher at those points—there are no other intersections along this stretch of I-85.²¹ Drivers’ behavior may be involved to the extent that those entering the highway quickly attempt to approximate the traveling speed of the other vehicles on the highway. Obviously this involves accelerating the vehicle, and may involve surpassing not only the speed limit, but the “threshold” speed limit in doing so (the speed limit as enforced on that stretch of I-85, roughly about 15 mph above the posted speed limit). Other factors may include geographic inclines or declines, but we did not collect data on such attributes.

²¹ Thus once a vehicle enters a stretch of highway between an intersection and another intersection, the vehicle cannot get off that highway until the next intersection.

Essentially, the areas in the figure with hundreds of citations issued could be classified as “citation zones” (including what some may call “speed traps”— although more than speeding could have triggered a stop in that location). As discussed earlier, by the term “citation zone,” we mean to include more than what is usually meant by the term “speed trap,” typically troopers with

Figure 2.13 NCSHP Stops on Interstate 85 by Milepost Number of African American and White Stops



radar guns sitting stationary, hiding in bushes or behind an overpass. A citation zone is located where there is a high volume of stops for any reason. Citation zones account for a high proportion of all stops (the top 4 miles in total stops account for 66 percent of all the stops on this 13-mile stretch (in other words, 31 percent of the miles account for 66 percent of the stops).

We do not know if accidents co-occur proportionately to the stops because we do not have milepost (locational) data for accidents. However, it would be surprising to find a close correspondence of accidents by milepost markers. For example, we find it hard to believe that

there would be forty-three times as many accidents in a 1-mile stretch of I-85 as compared to a stretch 2 miles away (we have examined the distribution of accidents across county areas—one third of a county— for I-85, and find that the length of the highway in an area and its proximity to large urban centers are strongly associated with the number of accidents). Even if, say, the mile with 814 stops had disproportionately many of the total number of accidents along this 13-mile stretch of highway, it is unlikely that the stops would be proportionate to the accidents. Rather, other factors, such as the volume of violating behavior and availability of turn-around spots are associated with making a “stop place” attractive. More research needs to be done on regarding this consideration.

In summary, one of the key “mechanisms” by which citations are generated along a busy interstate is the “citation zone” (anywhere a disproportionate number of tickets are written). Accidents in a district, by contrast, tend to occur in areas where the citation zones are probably seldom employed: small, relatively narrow, rural roads. Recall Table 2.3 above, which shows the distribution of accidents by type of road across the districts with “positive” scores in Figure 2.12. Also shown are the proportion of citations by type of road. Clearly, there is a mismatch. It would seem that troopers do not necessarily patrol roads where accidents are more likely to occur, but rather patrol highways “where the action is”—the larger interstates, U.S. highways, and N.C. highways. The type of accident and the prevalence of types of highway could account for the mismatch of patrolling and accidents. Accidents due to road design (such factors as narrow roads, poorly maintained road shoulders, steep inclines, or the presence of stop signs or lights) are associated with less dense traffic, such as accidents on what are called “the rural paved” roads. Also, there are many such roads across the rural areas of North Carolina, such that the few NCSHP troopers on duty on any one shift would not be wisely using their time to patrol these low

density highways. Rather, more violators (and possibly more deserving violators) are available on the busier highways where the troopers are best deployed. It is ironic that troopers may be best deployed on the relatively safe (but highly used) roads.

How then might the positives of Figure 2.11 and 2.12 be generated? Deployment decisions to patrol the busier highways (making NCSHP cars “highly visible”) could account for the disparity in the racial component relative to accident rates. If African Americans happen to be on the larger, busier highways more so than whites, the rates of citation will be higher for African Americans, independent of racial bias on the part of troopers. If deployment decisions are irrationally based, as opposed to rationally based (“we patrol there because the fishing is good” rather than “we patrol there because preventable accidents occur there”), the argument could be made that the racial disparity is inadvertent, but preventable by a change to a more statistically grounded deployment strategy.

Summary

In this chapter we have seen how the data, when aggregated to the entire state level, show some disparity in the citation rates, indicating over-stopping and over-citing of African Americans. However, the extent of racial disparity varies considerably by type of charge. Only some types of charges show disparity against African Americans. Moreover, there are many possible reasons for such statistics at the statewide level, potentially having little to do with bias. As we disaggregate²² the data to smaller units of analysis, we begin to get a clearer picture of the extent and whereabouts of disparity, and of the possible bias forms it could take (conscious bias,

²² By “disaggregate” here we mean more literally that we aggregate the data to relatively smaller units of analysis, such as the highway area, as opposed to the trooper district.

cognitive bias, inadvertent-irrational bias, or rational bias). Looking at the a somewhat disaggregated analysis for districts (N=53), we see some differences in terms of racial disparity, with some districts having more disparity evident than others.

The results speak to the possible value of using different baselines, against which to compare the percent of African Americans cited or warned. We can compare citation and written warning data to the resident licensed population proportion who are African American, or to the “drivers driving” who are African American, or to the proportion of drivers in accidents who are African American.²³ We argue that the latter may be a convenient, if not superior, type of data for looking for bias because of its widespread availability, and the fact that it can be measured at rather small units of analysis— here the highway area (a stretch of highway that covers an NCSHP officially designated area, or about one third of a county).

At the highway area level, when we calculate the difference scores for each highway area with five or more citations and five or more accidents, and aggregate that information to the district level (N=53), we find several districts that have high rates of citations of African Americans (also several with low rates, relative to the accident rates). We focused on these districts for further analysis, and found that the racial disparity is probably in part due to deployment. Specifically, it appears that the NCSHP deploys troopers to busy highways, which generally are not the highways where most accidents occur, but are the highways where African Americans may more often be found as drivers.

²³ We propose that the percent of reported accidents in which the drivers are African American is a measure of the prevalence of African American drivers on the highway. Some may prefer to attribute driving ability to be measured by involvement in accidents, but we do not make that claim. Note that we are referring here to accident reports and not to citations nor to any attribution of responsibility for the accidents.

Other researchers working in the area of racial bias and profiling have struggled with the problem of identifying baselines that are appropriate for making comparisons. The most widespread baseline used is still census counts of residents in an area. We find here that the accident baseline, while far from perfect, provide information that at relatively small units of analysis. Because of the availability of these data at small units of analysis, accident data probably should be used as a baseline more generally in research on policing and possible bias or disparity.

Looking at the results in terms of the bigger picture, the differences in racial disparity observed in the early tables of this chapter are probably largely accounted for by deployment decisions and by variation in vehicular behavior across the races. Further analysis would be necessary, and further discussions with the NCSHP leadership, to determine the likelihood that the deployment decisions have been arbitrarily made -- and could be changed -- if inconsistent with a more rational plan to allocate troopers to highways. Such a plan would presumably be based on preventable accidents (such as is part of the Total Quality Management approach of the NCSHP in the past few years). Such an analysis and discussion is beyond the scope of this report, but would nevertheless be worthwhile.

As researchers who are confronted with the data and analysis available to us, we believe that we have demonstrated that some racial disparity exists in the findings, whether large or small units of analysis are used, but that what seems to account for the disparity is probably mostly behavior of the drivers of the vehicles, as well as deployment decisions, whether made by the commanders, sergeants, or the troopers on the highway. The possibility also remains that some of these decisions are guided by conscious bias or alternatively by cognitive bias. In the next chapter we will explore further the possibility that cognitive bias plays a role in the day-to-day

decisions of troopers. That is, we assess individual trooper propensities to depart from the citation standard set by other troopers in the trooper's same district.

References for Chapter Two

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Chapter 3 Racial Disparity at the Individual Trooper Level

Goals of Individual Trooper Analysis

The question of whether or not racial disparity is present, as well as the extent and nature of it, can only be understood fully if the behavior of individual troopers can be evaluated. In the previous chapter, we argued that the context in which a trooper works will be important in accounting for the racial make-up of the citizens he or she stops, cites, and warns. In the present chapter, we will make use of the measures of context from the previous chapter and assess the extent to which individual troopers deviate from an expected number of citations written to African Americans. That expected number (based on a statistical model) may be largely based on a trooper's colleagues issuing citations in a similar, or even the same, context.

Our purpose in this chapter is, in part, to consider in some detail whether or not a methodology can be devised to serve as a source of information to identify troopers who deviate from the norm (baseline) in their citations of African Americans. We would like to know if we can explain the citations of African Americans by the contexts in which the troopers work, as well as by the characteristics of the trooper, such as his or her race, gender, or age. As such, this chapter's topic is a matter of much concern to both the NCSHP and the public. Troopers are genuinely concerned that their behavior might be labeled as racially biased, and the public wants to know if particular troopers behave in biased manners. It should be made clear from the beginning that our goal is not to identify or name individual troopers, but rather to develop and illustrate statistical methodologies that could be used by police organizations to generate information on racial disparity (generate one type of information) at the individual trooper level.

This information would require further validation by an organization before any conclusions could be drawn about whether or not racial disparity is a problem.

Presumably, the methodologies described in this chapter would be supplemented with additional information to validate that a racial disparity problem exists. Such additional information could include a more detailed analysis of the patrolling patterns of a trooper who has a seemingly high rate of African American citations. Or it might include reviews of citizen complaints, or even discussions with troopers in a district about the possibility of the existence of a hostile racial climate in that district. We want to be clear that the proposed methodologies here represent “pieces of the puzzle” rather than a definitive indication of the presence of a racially biased trooper.

Individual Level Characteristics

As mentioned above, and as discussed in the previous chapter, there are some individual characteristics of troopers that may be pertinent to any disparity that may be found in their citation behavior. For example, one might hypothesize that white troopers would be more likely to display bias toward African American drivers than African American troopers. Tables 3.1, 3.2, and 3.3 show the demographic breakdown of all NCSHP troopers working in North Carolina in calendar year 2000. As can be seen, only 15.4 percent of the troopers are African American (a small number of other minorities have been excluded from the table), compared to approximately 22 percent of the adult population of North Carolina. Women are much more under-represented relative to their representation in the population (only 1.7 percent). The majority of troopers are between the ages of 31 and 44 (54 percent), with 25.5 percent younger than 31 and 20.5 percent older than 44.

Table 3.1 Racial Make-up of North Carolina State Highway Patrol

Race of Trooper	Frequency	Valid Percent
African American	212	15.4
White	1168	84.6

Table 3.2 Gender Make-up of North Carolina State Highway Patrol

Gender of Trooper	Frequency	Valid Percent
Female	24	1.7
Male	1383	98.3

Troopers not only vary in age, but also vary in years on the force and the extent to which they have participated in training programs. The more training programs that a trooper participates in may be understood as a measure of professionalism. Troopers who take the time to participate in various training programs might be considered to be among those who take their work more seriously than others, who want to improve their job performance, or, alternatively, who have acquired a more professional attitude toward their work.²⁴

Table 3.4 shows the number of troopers participating in specific types of training programs, and Table 3.5 shows the number of training programs completed by varying lengths of time of employment. In Table 3.4, the most popular training program is CPR, with about 82 percent having completed this form of training. The second most popular courses are training classes for security involving “executives”—usually government leaders or dignitaries for whom

²⁴ The number of training programs completed obviously can vary across officers not only for the reasons of professionalism, but also because of age, tenure on the force, and other factors.

the NCSHP may provide security. The third most popular training program is Division of Criminal Information (DCI), which involves some training in evidence gathering and basic crime scene forensics. We will not discuss the specific nature of all of these programs, but present them to make the reader aware of the nature of possible training programs available to the trooper. Also, as we mentioned above, the participation of a trooper in multiple forms of training programs may be an indication of “professionalism” on his or her part, but this is—as of yet—speculative on our part. Our hypothesis is that the more professional the orientation of the trooper, the less likely he or she would apply the law in a racially biased manner (at least in an overt way). Later in the chapter, we will assess whether or not our hypothesis is supported.

In Table 3.5, we take up the question of whether participation in training programs is a function of time on the job (tenure). As expected, those troopers only on the force for a year or two have had the least opportunity to accumulate training experience (45.6 percent of troopers in their first two years have had no training beyond the academy). Approximately 18 percent troopers have managed to complete three or more of the training programs. In general, the longer the years of employment, the more training programs are completed. The majority of those troopers with six years of tenure have completed six or more training programs. At the same time, it is noteworthy that among troopers on the job for twenty-one or more years, about 47 percent have less than six completed training programs. Troopers on the force for less time have generally completed more training programs. (Possibly the older troopers already had years of experience before some of the training programs became available, and many of them may have thought the training was unnecessary since they were performing ably at the job already.)

Table 3.3 Age Distribution of North Carolina State Highway Patrol

Age of Trooper	Frequency	Valid Percent
30 years old and younger	359	25.5
31 thru 36	376	26.7
37 thru 44	384	27.3
45 and older	288	20.5

One might argue that the race of the trooper would be important to examine relative to the race of the citizen, under the assumption that if white troopers are biased, a higher proportion of the citizens they stop or cite would be African American than the proportion cited by their fellow African American troopers. Table 3.6 shows that the reverse is true, a result that is puzzling only because we have failed to take into consideration the fact that African American NCSHP troopers tend to work in areas where there are high percentages of African American citizens and African American drivers. In Table 3.6, it can be seen that 31.7 percent of all citations issued by African American troopers are issued to African American citizens, whereas only 24.2 percent of citations issued by white troopers are to African American citizens. A similar pattern is observed for specific types of reasons for citations (speeding, unsafe movement, and failure to stop/yield).

In an analysis not presented here, we found that troopers on the force for more than twelve years seem to be less likely to issue a citation to an African American than are troopers with less time on the force. However, like the analysis above for “race of trooper,” this is probably due in part to the fact that older troopers are more likely to work in areas where there are more whites (less urban counties). Thus, the context in which the trooper works may help explain the pattern of data at the individual trooper level. The smaller volume of traffic— and thus greater safety—can make more rural areas an attractive area for older troopers to work, whereas younger

troopers must “pay their dues” by working more densely populated areas. Most of the rural areas of North Carolina are predominantly white, while large African American populations disproportionately inhabit the larger cities as well as several rural counties.²⁵ The general pattern of lower percentages of African American citizens cited by white troopers holds true across the reasons for the citation (speeding, unsafe movement, and failure to stop/yield).

Measuring the Context of the Trooper’s Behavior

In the last chapter, we learned that there are various types of measures of context that could be used in the analysis, such as the racial composition of the drivers with licenses, or the racial composition of what we call “drivers driving.” We also saw that the racial composition of accidents could be a valuable measure because it allows us to measure the context of the trooper’s work in yet another way (and arguably more precise way)—the racial composition of smaller units of aggregation than the fifty-three NCSHP districts. We assume that the smaller unit of aggregation would be the more ideal, as it would lessen the dangers associated with spatial heterogeneity, that is, the mismatch of where drivers drive and troopers patrol, as discussed in the previous chapter. Two such units of aggregation seemed attractive to us: the county area (roughly one third of a county in size), and the county highway area (the segment of a particular highway in a county area). The analytic question before us is how to link information about the contexts in which troopers work with the individual level data. That is, what is a reasonable mathematical model of the processes that we have been discussing at the individual level of analysis?

²⁵ The claim that the work context accounts for the pattern is purely speculative on our part, but we will take it up again in the analysis below.

Table 3.4 Type of Training Received by North Carolina State Highway Patrol

Type of Training	Number of Troopers who Received Training	Percent of Troopers who Received Training
Advanced Accident Investigation	115	8.2
Accident Reconstruction	103	7.3
CPR Training	1151	81.8
Division of Criminal Information	241	17.1
Drug Interdiction Program	231	16.4
Emergency Medical Tech	129	9.2
Executive Security	322	22.9
Field Training Officer	337	24
Field Training Supervisor	218	15.5
Cert Instru Specialized Defensive Driving	105	7.5
Certified Instru General	296	21
Mobile Data Training	707	50.2
Certified Radar Operator	392	27.9
Riot Control	503	35.7
Mini 14 Rifle	365	25.9
Intoxilyzer 5000	1205	85.6
Certified Vascar and Radar Operator	812	57.7

Table 3.5 Years Employed with North Carolina State Highway Patrol by Number of Training Programs

Number of Training Programs	1 to 2 Years of Employment	3 to 6 Years of Employment	7 to 12 Years of Employment	13 to 20 Years of Employment	21 or more Years of Employment
No Training	57 (45.6%)	8 (3.3%)	2 (.6%)		1 (5%)
1 to 2 Trainings	46 (36.8%)	14 (5.7%)	2 (.6%)	1 (.3%)	12 (4.6%)
3 to 5 Trainings	19 (15.2%)	169 (69%)	150 (42.5%)	98 (25.4%)	108 (41.5%)
6 to 7 Trainings	3 (2.4%)	48 (19.6%)	130 (36.8%)	138 (35.8%)	79 (30.4%)
8 or more Trainings		6 (2.4%)	69 (19.5%)	149 (38.6%)	60 (23.1%)
Total	125 (100%)	245 (100%)	353 (100%)	386 (100%)	260 (100%)

Table 3.6 African Americans Cited by Race of Trooper and Type of Citation

	White Troopers	African American Troopers
All Citations Issued to AA	79102 (24.2%)	19638 (31.7%)
Speeding Citations Issued to AA	56119 (23.6%)	13705 (30.1%)
Unsafe Movement Citations Issued to AA	3614 (19.4%)	831 (27.1%)
Failure to Stop or Yield Citations Issued to AA	2247 (18.4%)	589 (25.5%)

Obviously, this exercise could be done at any level of aggregation that matched the available data associated with a stop/citation or written warning with where troopers actually patrol. We use the spatial units that follow (primarily county area and county highway area, as

described below) because they represent the smallest units of analysis that we have with sufficient observations to warrant statistical analysis. For many applications in other venues, only data at larger units of analysis may be available. At such levels the general method employed here could still be used, although, as the spatial unit becomes increasingly larger than the actual patrol area, we would expect more measurement error.

Before describing the analysis further, it occurred to us that the individual trooper data allows for yet another measure of the context in which the trooper works—the citations of the other troopers working in the same contexts as an individual trooper. That is, in addition to the measures of context that we could examine as per the discussion in the last chapter, the individual level data allow us another measure: the percentage of those cited by other troopers working in the same areas as the trooper whose citations of African Americans we are evaluating. To arrive at this contextual measure, we must first establish all of the areas in which a trooper issued citations. We do that for two levels of analysis: the county area (roughly a third of a county) and for what we call the county highway area (all citations on a highway within a county area, minus the trooper-in-question's citations). For example, if the other troopers issuing citations on U.S. 64 in a county area issue 25 percent of their citations to African Americans, then we would expect that the trooper in question should issue citations to African Americans at about 25 percent. That is, we assume that the 25 percent is a reasonable value measuring the racial composition of the context in which the trooper works. It represents a baseline against which to compare the individual trooper's behavior. Note that below we will suggest a reservation that we have about the use of the citations of the other troopers for the purposes of identifying possible racial disparity—a concern that will lead us to suggest that the accident measures of context may be superior. In a nutshell, our concern is that, if there is racial disparity in a particular trooper's

citations, then troopers working in the same district may share in the same bias. This will be discussed more below.

There are at least two options available to us in defining the metric of the dependent variable in the mathematical model. We could define a dependent variable as the proportion of a trooper's citations issued to African Americans, and compare that to the proportion in the baseline measures (for example, the proportion cited who are African American, or the proportion of drivers in accidents who are African American). However, we have found that the analysis of those dependent variables that are defined as proportions to be rather complex (Cohen and Cohen 1983:73-76). We opt instead to simply use the count of the number of African Americans issued a citation by a trooper as the metric of the variable.

We will primarily rely upon ordinary least squares regression to model the number of African Americans cited in the year 2000 by each trooper in the NCSHP (below, we present the results for average troopers on the highway, minus any troopers holding higher ranks such as sergeant or lieutenant). The choice of estimation technique reflects the distribution of the dependent variables. Analyses in other contexts or at other levels of aggregation might have less normally distributed outcomes and so require other estimation techniques. We will present two models for each of four measures of context. The first model, which we call the deployment model, will consist of the percentage of those cited or in accidents in the same county area or county highway area, plus the proportion of citations issued by a trooper by hour of the day. These measures constitute one baseline against which to compare the number of citations issued by a trooper to African Americans.

Another measure expected to be related to the number of African American cited is the number of whites cited by a trooper. Essentially, we are controlling here for the volume of work

done by a trooper (some issue many more citations than others). The more whites cited, the more African Americans should be cited. Recall that we found that time of day was an important determinant of the proportion African American on the highways and roads of North Carolina. Troopers work different shifts and those who draw more night shifts are likely to issue a higher proportion of their citations during nighttime hours, and thus, for example, would be expected to have a higher number of citations issued to African Americans. We control for this by calculating the percentage of all of a trooper's citations occurring in each 2-hour period (that is, twelve 2-hour periods).²⁶ In the tables below, only two of the two-hour periods are represented (the others were not found to be statistically significant and were dropped from the equations). We also attempted to control for weekday versus weekend (specifically, number of weekday citations), but that variable had to be dropped because it was too highly correlated with the number of whites-cited variable. Finally, we added the percentage of the trooper's citations issued on the interstates and on rural highways, as preliminary analysis showed a tendency for African Americans to be cited disproportionately on interstates and under-represented on rural highways. An individual trooper patrolling disproportionately more on interstates than U.S. or N.C. highways would be expected to have more citations of African Americans. A trooper working more on rural highways would be expected to have fewer citations of African Americans.

In summary, our first model represents what might be called the deployment model. The deployment model includes statistical controls for the area and highway types in which the

²⁶ We arbitrarily divided the day into twelve 2-hour periods: 12:00 a.m. (midnight) to 2 a.m. was the first period, 2 a.m. to 4:00 a.m. was the second, and so on. The number of citations of some of the adjacent 2-hour periods are correlated, as would be expected, since a trooper issuing citations between 2 a.m. and 4 a.m., is probably also issuing citations between 4 a.m. and 6 a.m. We found the correlations to be modest, however.

trooper issues citations and the time of day he or she works. Note that we think it is reasonable to call this model a deployment model—in that the variables are measuring spatial and temporal contexts in which the trooper works. While it is possible that the choice of where one works and at what time of day is not a racially neutral decision, we think it generally unlikely that racial disparity would serve as a basis for deciding what district one works in, or shift of work.

Our second model includes all of the variables from the deployment model, plus several individual level characteristics. These include such demographic characteristics of the trooper as race (African American or white), age, and gender. It also includes the number of training programs in which the trooper has participated (as a crude measure of professionalism), and how long the trooper has been employed with the NCSHP. Finally, we add two measures of the gender and age composition of a trooper's citations. Specifically, we include the proportion of a trooper's citations issued to those under the age of 23 and the proportion issued to women. We included these variables because, in some earlier analysis, we found some evidence that the rates of citations for those under the age of 23 seemed to be rather high, suggesting to us that some vehicular misbehaviors may be more prevalent among the young. If so, it is possible that some of the citations of African Americans could be accounted for by the age composition of the trooper's citations. That is, the more young cited, the more African Americans cited. We also include the gender composition variable to explore whether or not most driving misbehaviors occur among men, and whether or not, by extension, there may be some targeting of African American men in particular by the NCSHP. If so, we would expect the proportion of cites of women to be related to the number of African Americans cited (negatively related). However, the percentage female variable was found to be not statistically significant in any of the models, and so we dropped it from the equations and tables to follow.

Before discussing the results, it should be noted that the models presented below do not include the variable “trooper’s age.” It was dropped from preliminary models when it was found to be too highly correlated with the variable years of experience with the NCSHP. The decision to drop one versus the other was arbitrary, and one could reasonably include age instead of years on the force. There are no other multi-collinearity problems in the variables remaining.²⁷ Also, it should be noted that we have dropped all hours of the day from the equations except 2:00 a.m. to 3:00 a.m. and 10:00 p.m. to 11:00 p.m.. The other hours were dropped because they were not found to be predictive of any dependent variable, and we wanted to fit the results on a single page.

Results for the first two context measures are presented in Table 3.7. The first column lists all of the independent variables in our regression analysis. The deployment model involves the percent cited who are African American (cited by troopers other than the trooper in question) within the same county areas as those worked in by the trooper in question. In the deployment model, we see that for every white cited, .253 African Americans are cited. For every percentage increase in those cited who are African American (cited by other troopers) 5.5 additional African Americans are cited by troopers. In the parentheses are standardized coefficients, which are often used to compare the relative magnitude of variables’ effects. Thus, for example, the contextual measure of percentage of citations issued to African Americans by fellow troopers in the same county area is the strongest determinant of the number of African Americans cited by a trooper (.790). Stated another way, for each one unit change in a standard score of the percentage of those citations issued to African Americans by fellow troopers, we would expect a .790 change in

²⁷ Multicollinearity in the equation is minimized in part because we have centered all the variables (subtracted the mean from each variable), so as to reduce the chances of multicollinearity with the constant in the equation.

the standard score of number of African Americans cited. If there is no asterisk next to the standardized coefficient, neither the unstandardized nor standardized coefficients are statistically significant at the .05 level. Thus, for those variables, we cannot safely rule out the possibility that there is no effect of the variable. If a dash or hash mark is presented in the table, the variable was not included in that model.

As for other results, we see that the proportion of citations issued in the late evening hours (10 p.m.–11 p.m.) is associated with .936 more of an African American citation, while 2 a.m.–3 a.m. is associated with 1.138 more citations of African Americans. Thus, a trooper issuing a higher proportion of his or her citations late at night would be expected to have more African American citations than a trooper with a lower proportion. Other hours of the day were tested and found to be statistically insignificant, and were excluded from the table (and from the equation represented in the tables here), as they were not found to be significant in any of the models. We also find that the more citations that a trooper issues on rural highways, the fewer citations he or she issues to African Americans (the unstandardized coefficient is $-.251$, indicating that for each percentage increase in the citations in rural highways, there is a reduction of $.251$ in the number of citations of African Americans).

The deployment model with the first contextual measure accounts for about 70 percent of the variance in the number of African Americans cited. On the one hand, that would be considered by many to be a high proportion of the variance. On the other hand, it indicates that 30 percent of the variation cannot be accounted for by the deployment measures used here.

We turn next to the full model, which includes the deployment variables as well as the individual level variables discussed above. The results of the full model are similar to that of the deployment model. Note that we are examining this model for the purpose of better

understanding what might account for higher levels of African American citation behavior on the part of the troopers. We are not trying to “explain away” any possible bias in the citation behavior of troopers by controlling for these individual characteristics. For example, if we found that white troopers issued more citations to African Americans than African American troopers, then we could not use the predicted value from this full model as a basis for comparison. Essentially, the predicted value would itself be biased. We have no intention of using the full model for such purposes. Rather, our intention here is simply to see if there are any correlates or determinants of African American citing behavior from these individual characteristics.

As it turns out, there is only one statistically significant determinant of African American citations: the number of training programs in which a trooper participated. The more training (of a variety of types) a trooper has, the fewer African Americans cited. This indicates some support for our “professionalization” hypothesis: the more professionalized the trooper, the fewer the African American citations. However, one should be cautious in making this interpretation, since, as discussed earlier, age of trooper is related to the number of training programs in which a trooper has participated. (Recall that age has been dropped from the equation because of high multi-collinearity, primarily with the years of experience variable). Also, it may be that selection could be an interpretation of the finding: those with more training are patrolling highways with less traffic, and this is correlated with the racial composition of the highway not otherwise measured in our model.²⁸

²⁸ The model assume linear relationships among the variables and may not adequately capture the subtleties associated with where and when a trooper patrols. For example, a trooper may patrol more in one county area than the other troopers, or more during specific hours of day or even days of the week. Our model may be too crude to rule out such “selection” interpretations.

In summary, the county area analysis variable—citations of African Americans issued by others in a trooper’s work area—shows a strong relationship with the number of citations issued to African Americans by a trooper. Controlling for workload (number of whites cited), we find some hour of day effects, type of road patrolled effects, as well as effects of participation in training programs.

Because there is a possibility that the trooper working in the same county areas as other troopers may be working different highways, we also evaluate the effect of the percentage of citations issued to African Americans at the county highway area level. This differs from the previous analysis, because only those highways on which a trooper has cited someone are evaluated comparatively with the citations of other troopers. That is, if a trooper has issued a citation on U.S. 64 in a county area, then only those citations issued by other troopers on U.S. 64 in the same county area are used as a baseline for comparison. The results of the county highway area analysis are presented in the fourth and fifth columns of Table 3.7.

The results are generally similar to those found for the county area analysis. Both the contextual effect and the road-patrolled effects are similar, although in the county highway area, the interstate patrolling is associated with fewer African Americans cited (a surprising finding given our preliminary hypothesis that there would be more African Americans cited as proportionately more citations were issued on the interstate). In the county highway area analysis, the variance explained is somewhat higher (.715 and .717 across the two models) than in the county area analysis. The number of training programs is again found to be negatively associated with the number of African Americans cited by individual troopers. The magnitudes of the effects that are statistically significant are similar to those of the earlier models. The only

substantive difference in the county highway model from the county area model is that only one hour-of day effect was found (2 a.m.–3 a.m.).

One concern that we have about the use of the racial composition of citations of other troopers to measure the context in which a trooper works, is that it may mask some racial disparity due to the selection processes associated with the assignment and movement of troopers from one patrol district to another. For example, it is plausible that if racial bias was to manifest itself in attitudinal or behavioral displays, that the “principle of homogeneity” would operate: “like attracts like.” Those troopers with bias might, over time, find themselves working with others who are, like themselves, biased. If so, it would not be useful to compare the racial composition of a trooper’s citations to his co-workers.

A more independent measure of context, yet one still available at the relatively small units of analysis, is the racial composition of drivers in accidents (here we use accidents over three years: 1998, 1999, 2000). Table 3.8 shows the results of the analysis using the percentage of drivers in accidents who are African American as a baseline, and otherwise using the same substantive models as in the earlier table. In the first column of coefficients, we show the results of the deployment model and find that the results are similar to that of the earlier deployment model. We find that early morning citations (2 a.m.–3 a.m.), and late evening citations (10 p.m.–11 p.m.) are related to African American citations. Here, however, we find that the higher the percentage of a trooper’s citations on the interstate, the higher the number of African Americans cites. Note, too, that the explained variance is less than we found using citation racial composition as a context measure. Here it is approximately .60, compared to .70 using the citation contextual measures.

In the full model, we find that white troopers are less frequently citing African American drivers, as are those troopers who have been employed longer (net of the effects of other variables in the analysis). Also, the higher the percentage of those cited who are younger than 23 years of age, the fewer African Americans cited. The latter finding is contrary to our initial hypothesis that there might be some targeting of younger African Americans. Here we find that the younger the composition of those cited by a trooper, the fewer African Americans cited. As for the white trooper effect, it could be construed as evidence of avoiding citations of African Americans (perhaps due to concern over possible scrutiny by the media, legislature, research evaluators, or the public in general). It could also be explained by possible selection effects that are not controlled for statistically in the model. African American troopers, for example, could be deployed to highways that disproportionately have African American drivers. The results of the full county highway area model support the latter interpretation, because the white trooper variable is no longer statistically significant when we conduct analysis at the county highway area level.

Turning then to the county highway area accident measure of context (the percentage of drivers driving in accidents who are African American), we see that the explained variance rises to .65 (approximately). The deployment model is about the same as before, but the full model's effects are somewhat different because the individual level measures that previously were statistically significant (race of trooper and percent younger than 23 years of age) are no longer significant. Thus, there may be some value in using the county highway area as a unit of measurement as it lends plausibility to the interpretation that the previously seen white-trooper effect could very well be a deployment effect. Note that the hour of day effects observed for the analysis (using the county area accident measure) remain intact for the county highway area

analysis: early morning (2 a.m.) and late evening (10 p.m.–11 p.m.), in both the deployment and full models, and similarly for the percentage of a trooper's citations on the interstates. The only individual level measure that remains statistically significant between the full model of the county area and county highway area is the percentage of those under the age of 23 years old, which has a negative effect. Thus, somewhat surprisingly, the higher the proportion of younger people cited, the lower the proportion of African Americans cited.

In summary, the results of the analysis of Tables 3.7 and 3.8 indicate that virtually the entire variance that can be explained with the available measures in the number of African Americans cited by individual troopers, can be explained by deployment factors.²⁹ The deployment factors we find to be statistically significant include the racial composition of accidents in the same context, other troopers' citations in the same county area or same county highway area, the percentage of citations issued on interstates or on rural highways, the number of whites cited, and the hour of day in which citations are written. Of the two generic types of context measures (citations and accidents), accidents probably represent a more independent measure of context because the citation measures may mask the extent of bias due to possible clustering of biased troopers in the same districts. Of the county area and county highway area models, the latter model is probably preferable, since spatial heterogeneity issues are likely less of an issue within the smaller unit of aggregation (aggregating or adding together all the accidents in the county highway areas where a trooper issued citations in 2002.³⁰ In the county highway area

²⁹ We do not have, of course, direct measures of the troopers' level of cognitive bias or racial animus, so some would argue that our test for disparity might lead to different conclusions with such measures.

³⁰ However, a more sophisticated analysis could be done with controls for auto-correlation problems due to the fact that the contexts are not measured independently since the same accidents recur across the areas where a trooper issues citations. That is, it is likely that the

analysis, only deployment factors are found to be statistically significant. As such, the results are very similar to the county highway area analysis using other troopers' citations to establish a baseline of percent African American against which to compare an individual trooper's African American citations, except in the latter we found that the number of training programs was also statistically significant.

The equations that we have been evaluating are somewhat crude, in that some of the assumptions of ordinary least squares regression analysis have probably been violated (specifically, lack of independence in the error terms of the equation, causing what is known as auto-correlation). Yet, the analysis does propose a general method that can be used and improved upon for the purpose of determining whether or not individual characteristics or contexts are important in determining the racial composition of a trooper's citations. However, we have one more methodological contribution to explore that more specifically addresses the question of whether individual troopers stand out from his or her peers in his or her citations of African Americans: residual scores.

Identification of Troop Districts with Troopers as Statistical Outliers

We now demonstrate how the models, summarized in Tables 3.7 and 3.8, could be used to begin to flag the presence of troopers who might be out of line with their fellow troopers in their troop district (fifty-three troop districts). Because the models used here are presented in order to demonstrate a method rather than to identify specific individuals, we caution the reader against coming to any conclusions about the actual presence of racially biased troopers in any troop.

error terms in the equations suffer from auto-correlation because troopers in the same districts will tend to have similar values in the error term of the equations.

Rather, the figures below are presented to show a method with which to identify troopers who are “outliers”—those who have more citations of African Americans than would be expected using the models. It should be noted that the determination of who is an outlier will vary somewhat depending on the statistical models employed. At best, these models should be seen as providing information that may be weighed by decision makers and compared to other information to help diagnose the presence of racial disparity in a troop.

In demonstrating the method, we will utilize the deployment model that uses the accident measure of context at the county highway area level. By doing so, we only use deployment-related variables (percentage of drivers in accidents who are African American, number of whites, type of highway, and hour of day measures), and not such individual-level variables as race or gender. Therefore, an outlier will be defined as such relative to deployment factors alone.

Table 3.7 Number of African Americans Cited by Citation and Other Contextual- and Individual-Level Variables, Models Control for Racial Composition of Context by Using Citations of Other Officers in Same Context (Regular Troopers Only; N=925; Standardized Betas in Parentheses)

	1. Model Includes Measure at County Area Level		2. Model Includes Measure at County Highway Area	
	Deployment	Full	Deployment	Full
N Whites Cited by Trooper	.253 (.482)**	.245 (.467)**	.255 (.486)**	.249 (.474)**
Percent Cited Who Are African American (Cited by Other Troopers) at County Area Level	5.522 (.790)**	5.460 (.782)**	-	-
Percent Cited Who Are African American (Cited by Other Troopers) at County Highway Area Level	-	-	5.569 (.806)**	5.533 (.801)**
Percent Trooper's Cites 2 a.m.–3 a.m.	1.138 (.042)*	1.103 (.041)*	1.104 (.041)*	1.088 (.041)*
Percent Trooper's Cites 10 p.m.–11 p.m.	.936 (.042)*	1.101 (.049)*	.674 (.030)	.799 (.036)
Percent Trooper's Cites on Interstate	-.085 (-.023)	-.075 (-.020)	-.176 (-.047)*	-.159 (-.042)*
Percent Trooper's Cites on Rural Highway	-.251 (-.040)*	-.267 (-.043)*	-.252 (-.040)*	-.271 (-.043)*
Trooper White	-	-3.477 (-.015)	-	-2.229 (-.009)
Trooper's Years On Job	-	.186 (.014)	-	.206 (.015)
N of Trooper's Training Programs	-	-2.348 (-.064)*	-	-2.229 (-.061)*
Percent Trooper's Cites of Those Less Than 23 Yrs. Old	-	-.286 (-.023)	-	-.122 (-.010)
Constant	111.057	114.433	110.850	113.154
Adj. R ²	.697	.700	.715	.717

Table 3.8 Number of African Americans Cited by Accident and Other Contextual and Individual-Level Variables, Models Control for Racial Composition of Context Using Accident Data (Regular Troopers Only; N=925; Standardized Betas in Parentheses)

	3. Model Includes Measure at County Area Level		4. Model Includes Measure at County Highway Area	
	Deployment	Full	Deployment	Full
N Whites Cited by Trooper	.246 (.470)**	.228 (.435)**	.257 (.490)**	.243 (.462)**
Percent Drivers African American in Trooper's County Area Level	6.434 (.715)**	6.139 (.682)**	-	-
Percent African American Of Those Cited by Other Troopers at County Highway Area Level	-	-	6.805 (.753)**	6.590 (.730)**
Percent Trooper's Cites 2a.m.– 3 a.m.	3.221 (.120)**	2.941 (.109)**	2.872 (.107)**	2.700 (.100)**
Percent Trooper's Cites 10 p.m.–11 p.m.	1.270 (.057)*	1.404 (.063)*	1.317 (.059)*	1.411 (.063)*
Percent Trooper's Cites On Interstate	.327 (.088)**	.286 (.076)**	.285 (.076)**	.266 (.071)**
Percent Trooper's Cites on Rural Highway	.071 (.011)	.035 (.006)	.076 (.012)	.045 (.007)
Trooper White	-	-10.266 (-.043)*	-	-5.507 (-.023)
Trooper's Years On Job	-	-.671 (-.050)*	-	-.439 (-.033)
N of Trooper's Training Programs	-	-.914 (-.025)	-	-1.104 (-.030)
Percent Trooper's Cites of Those Less than 23 Yrs. Old	-	-.880 (-.071)**	-	-.596 (-.048)*
Constant	111.388	120.179	111.256	116.135
Adj. R ²	.601	.610	.652	.656

Figure 3.1 shows the box-plots for the first nine districts (of fifty-three) in North Carolina, in which the difference score has been calculated between the number of African Americans cited by a trooper on a highway type in a district, and the expected number of African Americans he or

she “should have” cited as predicted by the deployment regression model. We define outliers as a proportion of all citations written by a trooper. Thus, most troops show a range, within which nearly all troopers do not have greater than 20 percent more citations of African Americans than they “should,” relative to the number of citations of African Americans the model indicates they would be expected to have (based on the deployment factors). (These nine districts in Fig. 3.1 have been chosen and assigned arbitrary identification numbers, 1 through 9, to protect the anonymity of the district and the troopers.)

The box plot has four parts. For example, District 1’s box plot has a dark line representing the median value, which is close to zero. That is, in District 1, the median value is close to the expected value of the number of African Americans cited. The dark red area³¹ represents the inter-quartile range of values (all the observations between the twenty-fifth and the seventy-fifth percentiles). The small “T,” or so called “whiskers,” off of the top and bottom of the inter-quartile range, represents the range of the observed values (minus what are defined as “outliers”). Outliers are defined as observations more than 1.5 box lengths from the top or bottom edge of the box. We limit the analysis here to regular troopers who have more than seventy-five citations issued in the year 2000, thereby to avoid classifying a trooper as an outlier based on a small number of observations (roughly 97 percent of the regular troopers remain in the analysis). In District 4, there is one positive outlier (with relatively many citations of African Americans), and in District 7, there are two troopers who are negative outliers (having about 40 percent fewer citations of African Americans he or she “should” have relative to the predicted value from the model).

³¹ Of course, if you are looking at a copy that was not printed on a color printer, the color will be a dark gray.

As can be seen in a review of the nine districts presented in Fig. 3.1, the ranges of the number of African Americans cited varies considerably from one district to another, with District 2 having a wide range and relatively high range (from approximately 5 percent above, to 20 percent below the median number of African Americans cited). In Figures 3.2 through 3.7, each of the fifty-three districts' citation patterns is shown. In general, the results indicate that there are many outliers, both positive and negative, across the districts. Specifically, Districts 4, 11, 12, 22, 23, 28, 30, 35, 38, 39, 41, 42, 43, 44, 45, and 49 have at least one positive outlier, and Districts 7, 10, 11, 13, 16, 17, 18, 19, 20, 23, 25, 26, 33, 34, 45, 48, 49 and 51 have at least one negative outlier. In all, eighteen districts are found to have positive outlier values, while twenty-five districts are found to have negative outlier values.

An examination of these outliers reveals that the positive outliers are somewhat more likely to involve citations on interstate and U.S. highways, while negative outliers are disproportionately from rural highways. Positive outliers were found (analysis not presented here in table form) to be somewhat less experienced troopers and troopers who work weekends. In examining the citations of specific troopers, we realized that further fine-tuning of the regression models could be possible. Although we control for such factors as type of highway and time of day, the variables are crudely measured. For example, some of the rural highways pass near mostly African American communities within larger, mostly white areas. While in general, rural highways in North Carolina would be expected to be associated negatively with the percentage of drivers who are African American and involved in accidents, there are many rural African American communities in North Carolina. Thus, some troopers who happen to issue a high number of citations to African Americans do so on rural roads proximate to communities with relatively high percentages of African American residents. If such roads happen to be relatively

safely-constructed rural highways, the accident rate could be low, possibly making the estimate of the percent African American among those in accidents unreliable. Obviously, this points to the need to conduct further analysis and to refine the regression model with more information.

It is also possible that the mechanism by which large numbers of African Americans are cited may have a lot to do with the “citation zones” discussion in the previous chapter. Many of the positive outliers were noticed to patrol more often on interstate and busy U.S. highways than the negative outliers did. This is suggestive of the important role “citation zones” may play in creating a positive outlier. That is, an individual trooper may “work a zone” frequently over the course of a year and develop a large number of African American citations because he or she works a selected area where there are a relatively high percentage of African American drivers on the highway (and not often involved in accidents). It is also possible, however, that the trooper is biased—perhaps both in the selection of vehicles to pull over, or in the selection of an area where he or she will “work the zone.”

Conclusions

In conclusion, we have shown in this chapter that it is possible to model the citations of African Americans at the individual officer level, and to do so with considerable success by conventional standards of explained variance. Although more complex statistical methods can be applied,³² the models presented show that statistical outliers can be found in some troops but not in others. (Our experience with some of the more complex modeling techniques is that although they can improve on ordinary least squares regression models, the latter will be a reasonable

³² Other techniques include hierarchical linear modeling, Poisson or extra-Poisson regression, or models that control statistically for the auto-correlation problem.

approximation of the results found using the more sophisticated models). Our goal here is to show the methodological principles of baseline and individual covariates, as determinants of the number of African Americans cited by individual troopers. The techniques show the extent to which a trooper's individual citation behavior varies relative to that of other troopers. Outliers can be neatly displayed in boxplots, and further analysis can be done to help verify whether other considerations, such as specific highway patrolled, frequency of patrolling, and excessive use of speed zones, are mechanisms that generate some of these high positive outliers.

Finally, it should be reiterated that the determination of disparity cannot be equated with the determination of bias. Other pieces of information should be gathered to supplement any statistical analysis similar to what has been done here. Nevertheless, the analysis here, although it could be improved upon with further considerations of relevant factors, does demonstrate that a method can be used to provide yet another indication of what behaviors are occurring in the day to day actions of individual troopers.

Figure 3.1 Box-plots of Districts 1 to 9, (Arbitrary ID Number) With Positive and Negative Outliers (Defined as Percent of Trooper's Citations)

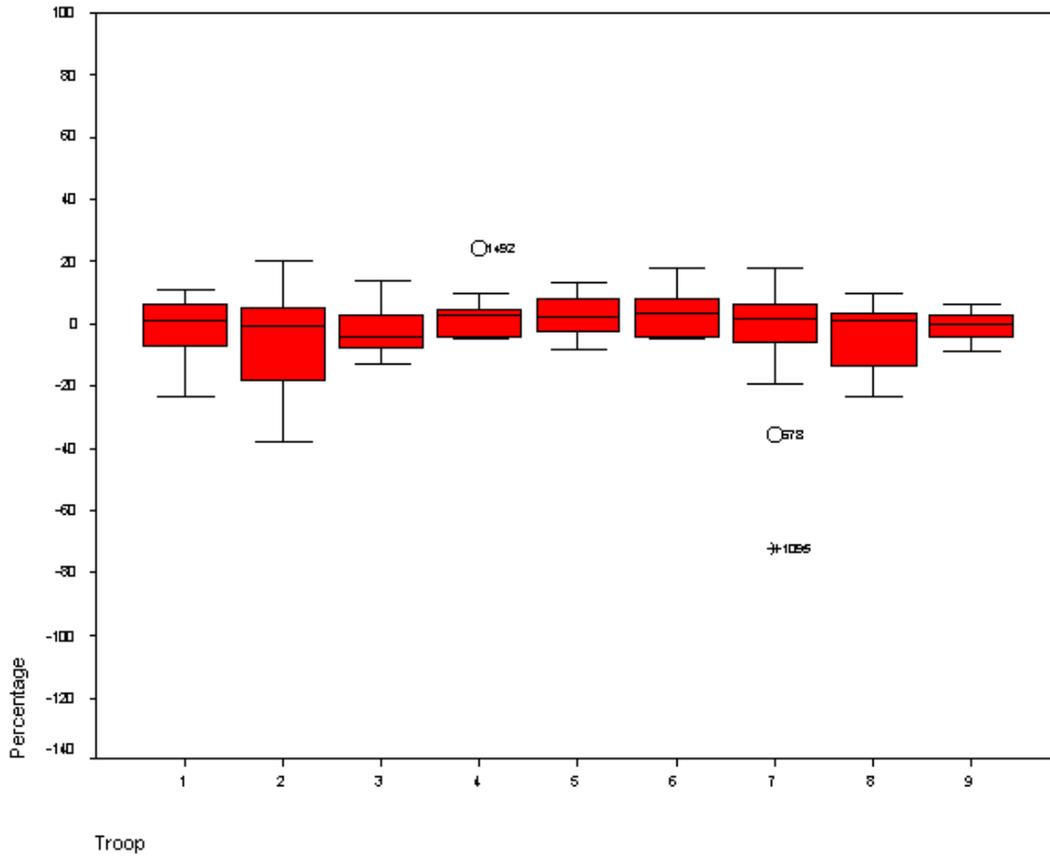


Figure 3.2 Box-plots of Districts 10 to 18, (Arbitrary ID Number) With Positive and Negative Outliers (Defined as Percent of Trooper's Citations)

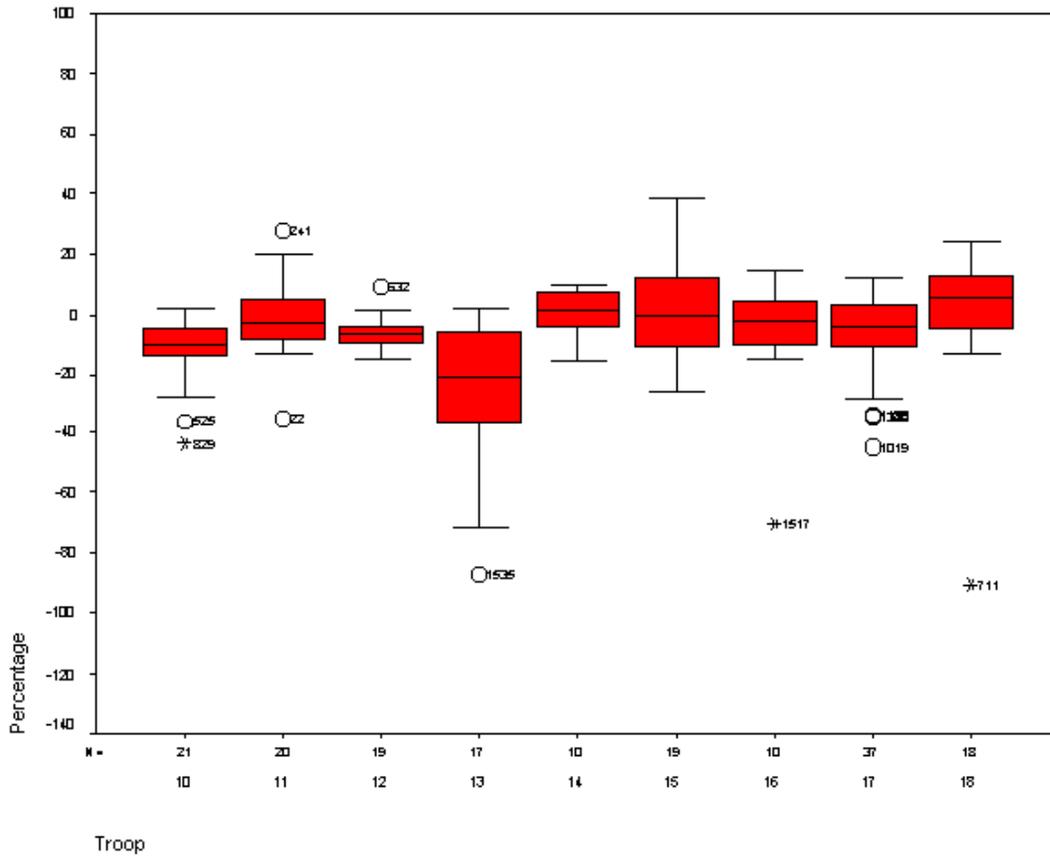


Figure 3.3 Box-plots of Districts 19 to 27, (Arbitrary ID Number) With Positive and Negative Outliers (Defined as Percent of Trooper's Citations)

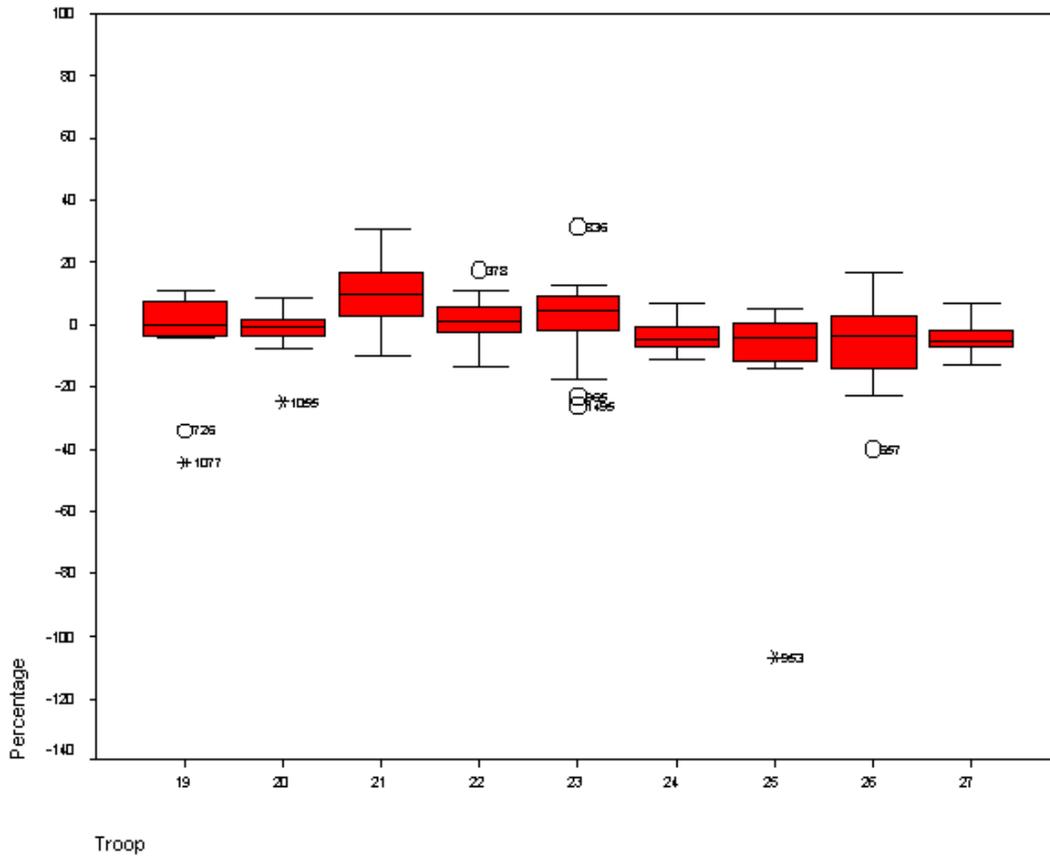


Figure 3.4 Box-plots of Districts 28 to 36, (Arbitrary ID Number) With Positive and Negative Outliers (Defined as Percent of Trooper's Citations)

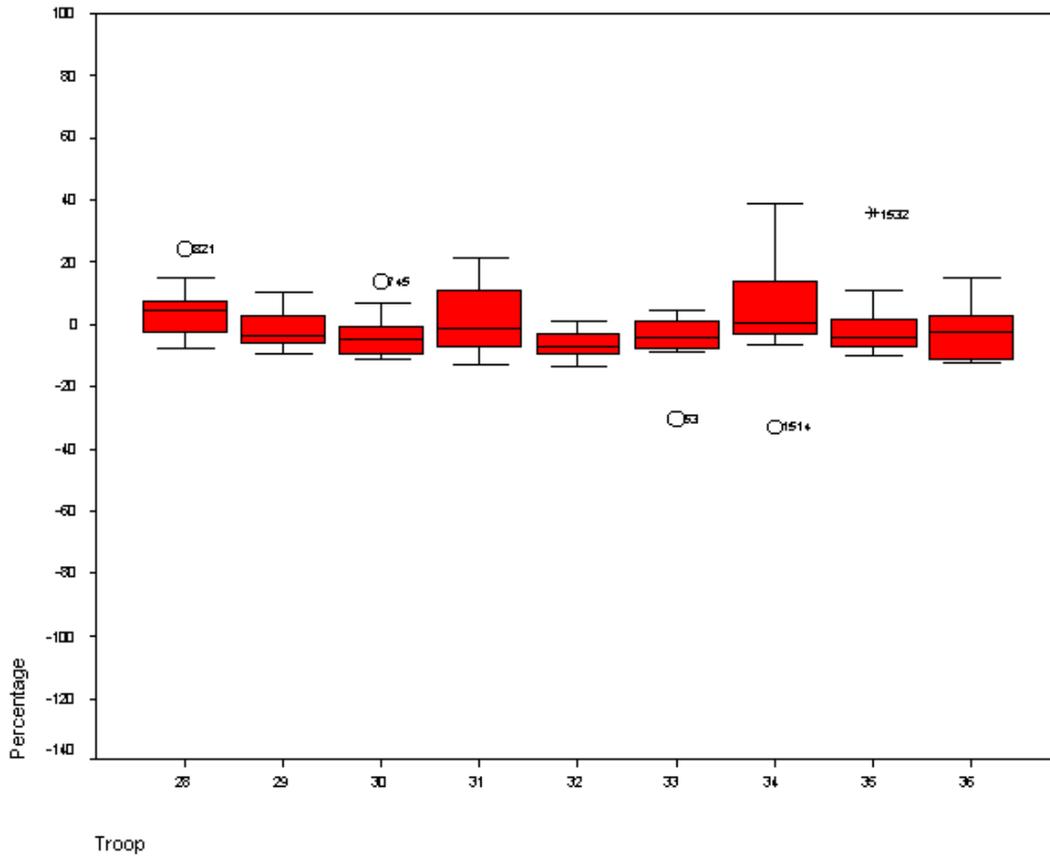


Figure 3.5 Box-plots of Districts 37 to 45, (Arbitrary ID Number) With Positive and Negative Outliers (Defined as Percent of Trooper's Citations)

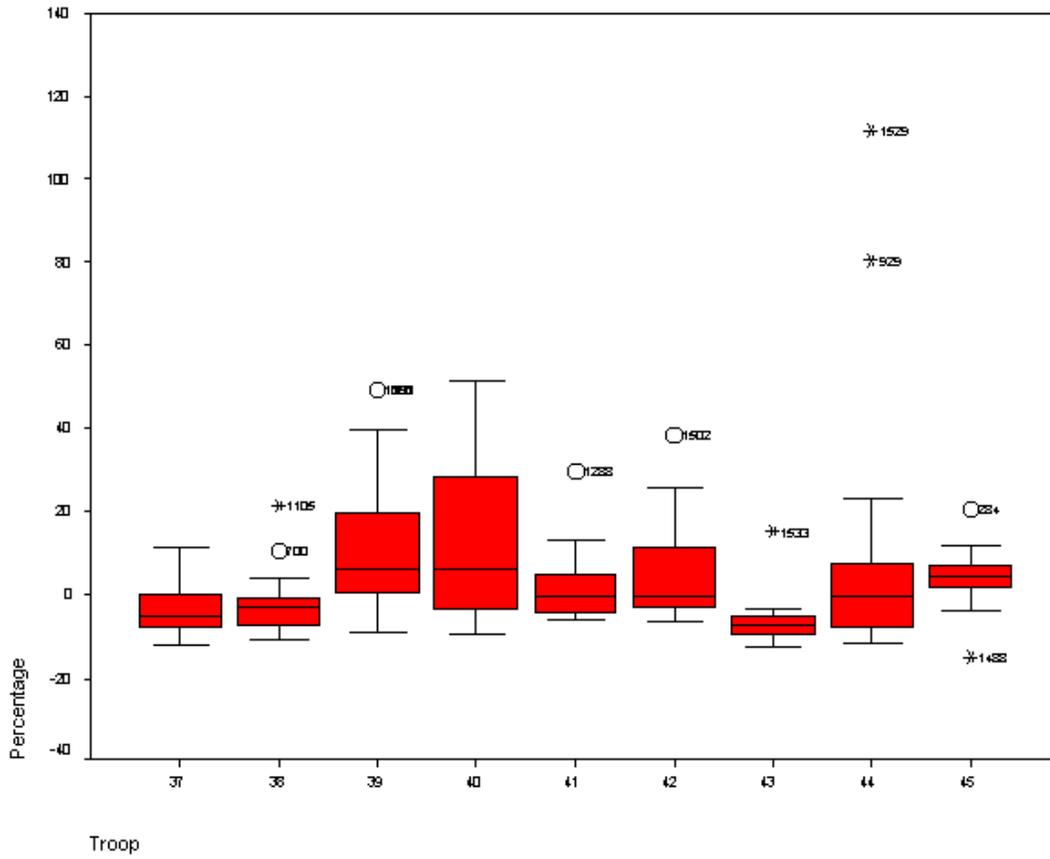
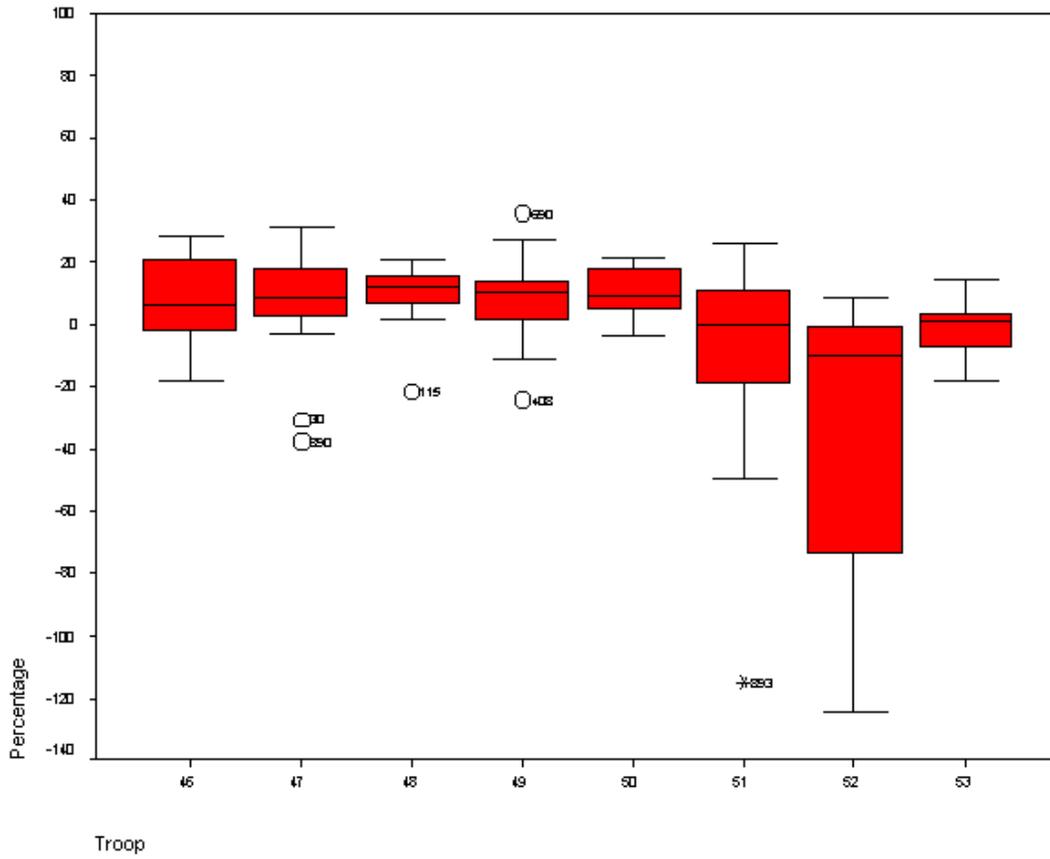


Figure 3.6 Box-plots of Districts 46 to 53, (Arbitrary ID Number) With Positive and Negative Outliers (Defined as Percent of Trooper's Citations)



References for Chapter Three

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Chapter 4 Searches by the North Carolina State Highway Patrol

One of the central issues in many of the national accounts of racially biased policing has been the consideration of the race of a driver as part of a profile used in vehicle searches for drugs and other contraband. It is clear that the U.S. Drug Enforcement Agency (DEA) and Federal Highway Administration have, in the past, offered drug interdiction training to local police forces suggesting, implicitly or explicitly, that race or ethnicity—along with other vehicle or driver characteristics—might be used to decide whom to stop and search. There have been a number of cases around the country documenting that state law enforcement agencies search many more minority drivers than would occur in the absence of such profiles. To the extent that race is used as a profiling tool to identify potential carriers of drugs or other contraband, we would expect to find large disparities in the odds of being searched between African American and white drivers.

Racial profiling is a practice in which a police organization generates or uses a profile meant to describe a typical offender where that profile includes race as one of the criteria. Racial profiling—although at first glance, it may seem a useful tool for police work—is a form of institutional discrimination or institutional racism. Institutional racism refers to organizational practices which produce racial inequality. In policing, there seems to be at least one area in which explicitly race-sensitive institutional rules are still used. This is the practice of developing offender profiles—typical characteristics of drug couriers being a prominent example. There is little question that the DEA generated such profiles in the past as part of the war on drugs, and that these profiles include race and ethnicity, among other characteristics, which have been promoted as useful factors in deciding which cars to stop and search for drugs.

The exposure of racial profiling by drug-seeking state police in both New Jersey and Maryland played a prominent role in bringing the issue of “driving while black” to national attention. Racial profiling, when it exists, is a fairly specialized police practice. Many troopers never search cars for drugs. Where racial profiling is part of an organizational routine, we would expect there to be evidence of high levels of racial disparity in searches. We would also expect the practice to be embraced by most, if not all, troopers doing proactive searches. By proactive searches, we refer to actual use of the profile, resulting in a traffic stop to look for contraband. Sometimes troopers observe contraband (or indicators of contraband—smells, drug paraphernalia). In this case, the trooper is confronted with direct evidence prior to the search and is free and justified to initiate a probable cause search. Troopers who are looking for contraband, but do not have probable cause, must ask drivers to consent to a search.

Race and Searches by the North Carolina State Highway Patrol

As early as 1996, journalists investigated racial disparity in searches by the NCHSP. They reported that the group of troopers specifically assigned to drug interdiction, the Criminal Interdiction Team (CIT), stopped African American men at twice the rate of other NCHSP troopers patrolling the same areas. They also reported that of the 3,501 vehicles searched by the CIT in 1995, contraband was found in only 210 vehicles (Neff and Smith 1996). In this chapter, we look at search activity by the NCSHP from 1997 to 2000, and pay particular attention to searches by the CIT.

Troopers' Accounts of Search Behavior

We spoke with both CIT and regular troopers as part of this project in early 2001. The CIT troopers were adamant that they did not consciously use racial profiling in deciding whom to stop or search. Instead, it was their practice to aggressively enforce the traffic laws of North Carolina. They did describe the use of other characteristics of drivers that made one car more suspicious than another, and therefore deserving of more attention, and they did note that when they turned on the blue lights, there was some selectivity involved. This might include legally-reasonable selectiveness of drivers' actions, including more than a single violation, a serious violation, or an unsafe movement among a pack of vehicles. Other factors might include more benign indicators that come to troopers' attention, such as loud music, stickers referring to music groups connected to the drug culture (such as the Grateful Dead or Phish), or cars about which they felt "something" was out of place, or about which they had a "gut feeling."

When troopers gave examples of the use of indicators (we would refer to them as stereotypes) at work, they tended to be drawn from both white and African American youth or sub-cultures. The allusions to African Americans were far more general—rapper music or "the way young African American males dress"—than the examples given for whites—Phish follower, tie die shirts, biker with a ponytail. From the troopers' accounts, we have little reason to expect racial profiling to be going on in a self-conscious way as late as 2001. Still, there is little reason to ignore the potential for stereotyping and cognitive bias to influence face-to-face highway encounters. Further, the stereotypes used to describe suspicious African American drivers tend to be much broader and more diffuse than the stereotypes more likely to apply to whites.

CIT troopers reported that they typically use a fairly standard method of questioning after a stop that is designed to see if the driver had a plausible and consistent story, and also to

determine if the driver became less or more nervous across the course of the conversation. It is assumed that the non-combative and friendly general conversation should relax the driver. Troopers said a search would likely follow in three general scenarios: if there was probable cause—for example, visible contraband or the smell of marijuana; if the conversation resulted in too many inconsistencies; or the driver was still visibly nervous after the initial interview. In the latter case, the interview method is a tool that produces reasonable suspicion that something might be wrong. An excessively nervous or confused driver is viewed as more likely to be doing something wrong, therefore providing troopers with reasonable suspicions, and hence, a good choice for troopers to request a consent to search.

The CIT members talked about writing a lot of warning tickets. Someone who was still nervous after having been written a warning ticket often heightens the suspicions of a trooper, because warning tickets are viewed as an outcome of the encounter that should relieve most citizens' anxiety. Although the CIT's primary task (and pride) centers on drug interdiction, the troopers made it clear that their interview protocol was cued to nervousness and body language—therefore making it a useful tool for identifying felons, people with outstanding warrants, or other violators. Some troopers in the focus groups recognized that the use of nervousness might produce unreliable results, but the general tone was one of great faith in their methods of interdiction by conversation. In fact, the use of the interview method was seen as counteracting any personal stereotypes (biases) one might have about drivers. The troopers also viewed their leadership to be intolerant of any racial bias.

There was general agreement among the CIT members that certain cues are used to determine whether or not to gather additional information, and that these cues, in their totality, made some people seem more suspect than others. (We have referred earlier to decision making

processes based on incomplete information as a stereotyping process.) These troopers believed that their high standards for determining when to search (they noted that “articulable and reasonable suspicion” is required before a CIT member can request consent to search), their meticulous attention to recording information in detail, and their aggressively “by the book sergeant” have precluded racism or stereotyping among the CIT.

We also discussed searches with regular road troopers. We did this, in part, because we had been struck by the extremely small number of searches attributed to them. We found searches among this group to be so rare as to simply not be part of a trooper’s normal routine. The troopers we talked with were only a small fraction of the 1,200 or so troopers patrolling the state highways, but to an individual they either claimed to do no searches, or claimed to search only reluctantly. At least part of that reluctance was attributed to safety concerns, because troopers patrol alone. A search would occur, they said, when contraband was visible or the driver had to be searched incident to arrest. Searches extend contact with a driver, and are thus seen as dangerous, because they could lead to a confrontation with the driver or occupants. Additionally, if contraband, especially of the type and volume they might be likely to encounter, was to be discovered, it would lead to a great deal of undesirable paper work and potential time in court. The regular road troopers we talked to were not enthusiastic about searches in the least, and it is clear that they view unnecessary searches, in general, as a nonproductive use of their time. It also seems clear that the threshold at which a “cue” or “indicator” would rise to the level of reasonable suspicion is high for regular road troopers.

Given their accounts, we would expect that searches would be rare among regular troopers. This expectation will be confirmed shortly. In addition, the accounts given by the CIT members seem consistent with practices that could produce some racial disparity in searches, but

not wholesale racial profiling. The CIT accounts look more like cognitive bias processes, perhaps exaggerated by the routine use of what we refer to as stereotypes, to decide whom to stop but not whom to search. Since the interview method is an interactional and a conversational accomplishment, it seems reasonable to suspect that the cues and indicators (stereotypes) built into the process, and cognitive bias—both individual and organizational—might influence not only the interaction, but also the interpretation of the interview’s outcome in ways that disadvantage some, but not all, of the persons stopped. Of course, the conversational method is biased inherently against anyone who is nervous, confused, or belligerent. We know from prior research and Chapter 6 of this report that African Americans have lower levels of trust in the police. This may lead in some instances to nervousness in its own right.

Throughout the discussion below, a distinction will be made between consent searches and probable cause searches.³³ For a consent search, as the name suggests, a search cannot be conducted if the person in question refuses to grant permission for the search. Yet there must be grounds for suspicion of the person. For a probable cause search, no such permission is needed as the trooper has seen sufficient evidence to warrant the search, such as drug paraphernalia or a weapon. However, in our conversations with CIT troopers, it was stated that often it was the practice in a probable cause situation for a trooper to ask for permission for the search since a consent search would lessen the subsequent chance that the trooper would be accused of

³³ We ignore searches incidental to an arrest because we assume that if there is sufficient cause to arrest someone, the search of the person is perfunctory from the point of view of whether or not there is disparity. While there may be some bias in arrest procedures, it is simply beyond the scope of this report to assess such processes (as we have no data to assess). We think, however, that there are some checks and balances in the criminal justice system regarding cases brought to court with insufficient evidence. Presumably a trooper who repeatedly arrests without sufficient cause would come to the attention of prosecutors and of his or her superiors. While such checks and balances are undoubtedly imperfect, it is simply beyond the scope of this research to address such cases.

fabricating the probable cause evidence (also, by asking permission to search, the trooper might be perceived as being more respectful and therefore less likely to antagonize the suspect).

Whether or not the trooper recorded a specific event as a probable cause or a consent search is not clear. Still, we suspect that during the years of this study, some searches recorded as consent searches were in fact probable cause searches. Unfortunately, any shifts of this sort make it difficult to evaluate trends in probable cause and consent searches.

To the extent that the troopers we interviewed capture general NCSHP activity, the above discussion suggests that regular road troopers should record a higher proportion of probable cause searches—because of their general reluctance to search and their higher threshold for articulable suspicion. That would lead us to expect them to also record higher proportions of searches resulting in the seizure of contraband. The CIT troopers, on the other hand, note that searches are based on the results of the interaction with the driver (articulable and reasonable suspicion including nervousness and other signs related to the car or driver), and of course, probable cause, such as visible contraband. Thus, we might expect a higher proportion of consent searches and a lower rate for identifying contraband among the CIT.

Recorded Search Activity

In this section we analyze the incidence and distribution of searches recorded by the NCSHP. We contrast the activity of the CIT and other NCSHP troopers during the 1997–2000 period. We think it is important to recognize that in 1996, the CIT came under public scrutiny for racial profiling in searches. It is reasonable to suspect that the fairly dramatic change in search behavior in both the CIT and among other troopers soon after was, in part, a response to that scrutiny.

Figure 4.1 displays separate time trends for recorded searches by the CIT and all other NCSHP troopers. As shown, the CIT conducts substantially more searches than other troopers. In 1997, for example, the CIT recorded 769 searches, while the largest other troop recorded sixty-two searches. In 2000, the comparable numbers were 387 and 110. There was a rapid decline in searches by the CIT across the period. The number of troopers assigned to the CIT declined across this period as well, dropping from twenty-five active troopers in 1997, to thirteen in the year 2000. These troopers were averaging thirty-one searches per year in 1997, and thirty in 2000. Outside the CIT, many troopers record doing no searches in a typical year. Many of the troopers who do report searches average only one or two per year. Regular road troopers slightly increased their volume of searches in 1998. There was a small decline in searches by regular troopers after 1998.

Figure 4.1 Searches by CIT and Non-CIT NCSHP Troopers

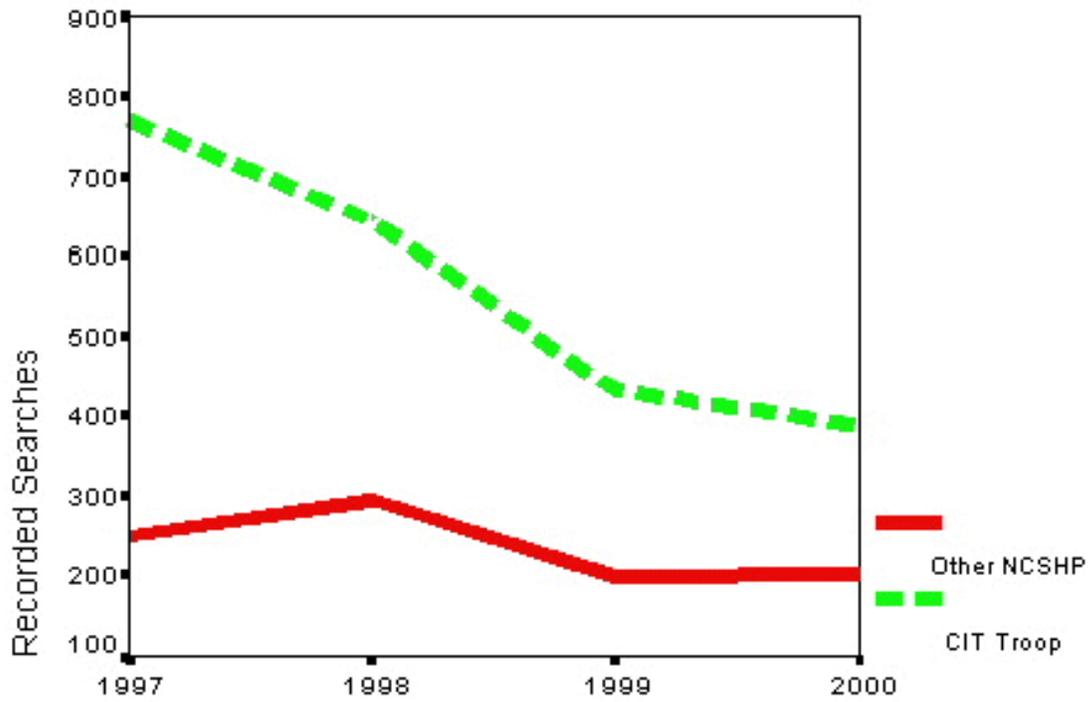
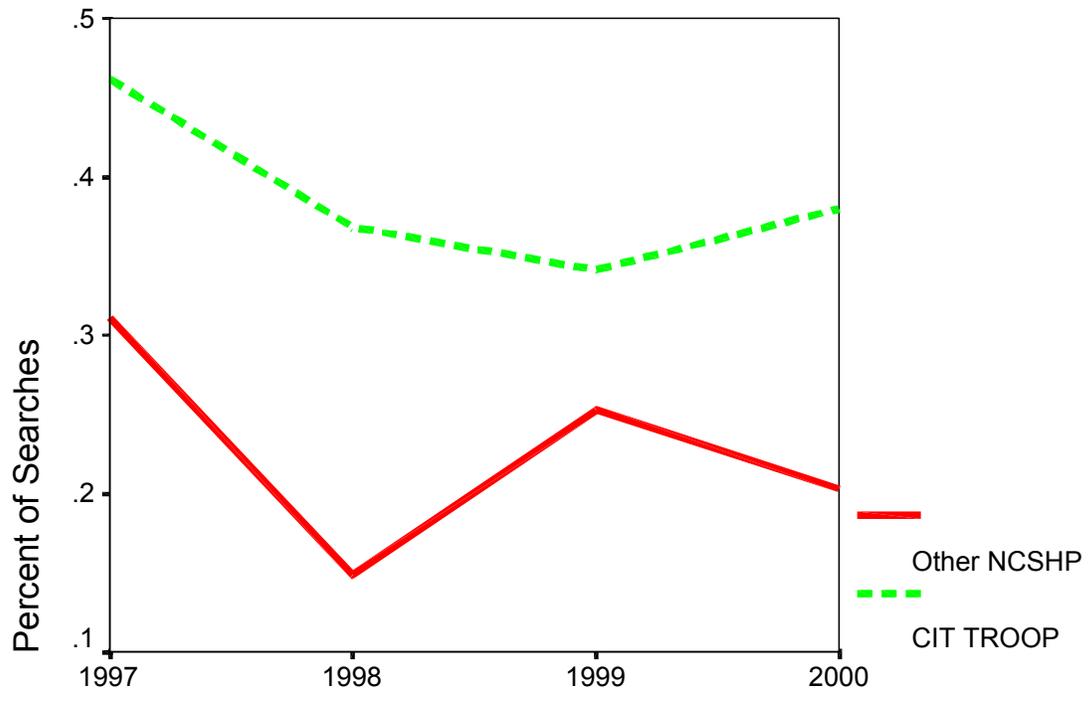


Figure 4.2 shows the time trends in the proportion of all recorded searches of African Americans. There was a sharp decline in the proportion African Americans searched in 1998 for both CIT and non-CIT troopers. The percent African American increases somewhat for both non-CIT troopers in 1999, and for CIT troopers in 2000. As we have seen in earlier chapters, about 20 percent of drivers are estimated to be African American. In 1997, 46 percent of searches by the CIT were of African Americans and slightly more than 30 percent of searches by non-CIT troopers were of African Americans.

In order to ascertain if this rate of searches of African Americans is excessive, we need to establish a baseline of drivers at risk to be searched. Because drivers are searched after they are stopped, a reasonable baseline is the racial composition of drivers stopped. The baseline used in Table 4.1 and Figure 4.3 is measured at the troop level. Except for the CIT, other troops are identified by a fictitious identification number. This baseline is potentially misleading to the extent that the few troopers who actually conduct many searches may patrol in areas within their troop's territory with higher or lower concentrations of African American drivers. This potential problem is not present for the CIT, since all troopers actively search cars. Later, we repeat these analyses at the individual trooper level and use the trooper's own distribution of citations and warnings as the baseline to compare with the racial distribution of searches.

Figure 4.2 African Americans as Percent of Searches, CIT and Non-CIT NCSHP Troopers



We use combined data on the racial composition of warnings and citations for 1998–2000 to establish a baseline of drivers at risk to be searched. For 1997, only citations are available, and so we use the racial composition of citations as our baseline estimate of who is at risk to be searched. As we saw in Chapters 1 and 2, the racial composition of warnings and citations tends to be very similar. In previous chapters, we have examined variation in racial disparity for the fifty-three troop districts in North Carolina. Search events are much too rare to be analyzed at this level. We use instead the eight regular NCSHP troops plus the CIT as the organizational units. Table 4.1 displays the racial composition of drivers stopped and searched for these nine troops for 1997. Many troops recorded very few searches in 1997. In all troops except Troop 7, African Americans are a higher proportion of searches than they are of drivers stopped and cited. In all troops, including 7, whites were searched at lower rates than they were stopped in 1997.

The next to last column displays the relative odds of being searched after being stopped and compares African Americans and whites ($[\text{African American searches/stops}] / [\text{white searches/stops}]$). For all troops in 1997, African Americans have higher odds of being searched than do whites, although the magnitude of this increased risk varies tremendously across troops. At the low end, in Troop 8, African Americans were 1.27 times more likely than whites to be searched after a stop. Troop 7 is almost as low, at 1.33. These two troops record very few searches. At the other end of the distribution for 1997, CIT members searched African Americans who had been stopped at more than four times the rate of whites. Although only 48 searches were recorded by Troop 1 during 1997, African American drivers were searched at four times the rate of white drivers. For 1997 across the NCSHP, there is substantial racial disparity in searches.

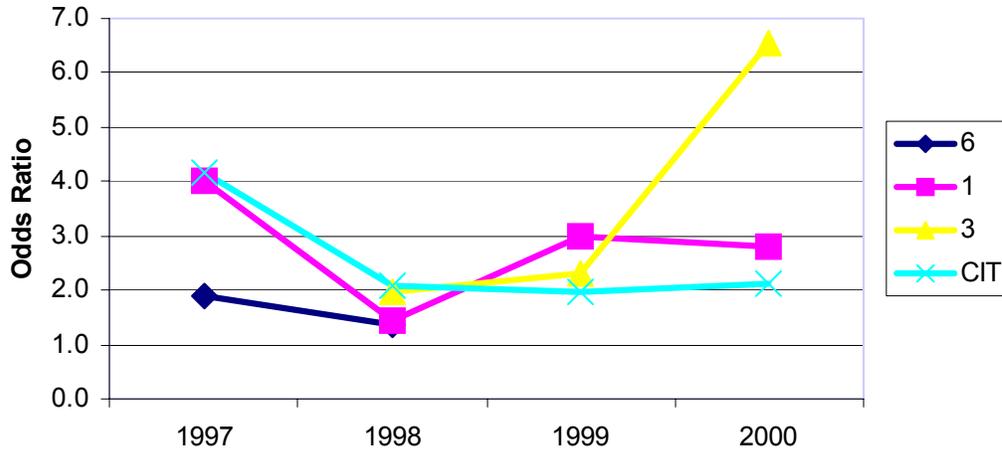
Troop	Percent African American Stopped	Percent African American Searches	Percent White Stopped	Percent White Searched	OddsRatio (African American Searches/Stops) / (White Searches/Stops)	Number of Searches
CIT	22.4	46.2	72.1	35.8	4.15	769
1	5.3	18.8	90.5	70.8	4.01	48
2	15.3	40.0	78.3	60.0	3.41	5
3	28.3	46.2	65.4	42.3	2.52	26
4	8.0	17.7	87.4	77.4	2.50	62
5	24.8	38.1	67.8	42.9	2.43	21
6	31.5	48.1	63.8	51.9	1.88	52
7	33.8	33.3	56.5	41.7	1.33	12
8	25.9	28.0	61.0	52.0	1.27	25

One of the most striking things about Table 4.1 is that, with the exception of the CIT, the NCSHP does not routinely search vehicles. At the organizational level, if racial profiling accounted for the observed disparity in 1997, regular road troopers were not involved. While individual troopers in these units may have been using a racially influenced drug offender profiling method, troops as a whole were simply not in the search business.

Because there have been dramatic changes in the search behavior of NCSHP troopers since 1997, it is appropriate to see if these racial disparities in search rates are stable over time. Figure 4.3 displays the time trends in the relative odds of African American to white searches for

the four NCSHP troops recording thirty or more stops in two or more years. Across the whole period, the vast majority of troops record very few searches.

Figure 4.3 Trends in Relative Odds of Black/White Searches, Troopers with 30 or More Searches for Two or More Years



In addition to the CIT troop (I), only a single troop was heavily involved in searches across the years. The time trends are quite dramatic for all four troops. In 1998, there is a sharp drop in the relative odds of African Americans being searched for all four troops with thirty or more searches. By 1999, Troop 6 recorded only twenty-four searches and the time series ends. Clearly the NCSHP not only drastically reduced the number of searches it conducted in 1998, it also searched African Americans at lower rates. It is still the case across the entire period, however, that for the few troops with substantial search activity (as defined here), the NCSHP searches African Americans at higher rates than it stops them.

The patterns are not the same for each troop. The CIT's racial disparity in searches drops dramatically from 1997 to 1998, and plateaus with an African American/white odds ratio of about 2:1. Prior to 1998, the CIT had a very high racial disparity in search rates that, on its face, is consistent with a practice of racial profiling. By 1998, its level of racial disparity was much smaller and was also lower than the other two troops examined. This pattern looks very similar to the time trends for the CIT reported in Figures 4.1 and 4.2. After 1997, the CIT searched fewer drivers and especially fewer African American drivers. The disproportionate searches of African Americans by the CIT reported in the press in 1996 and still present in 1997, quickly eroded in 1998. This suggests a dramatic shift in selection criteria for whom to search, with race playing a much smaller role after 1997.

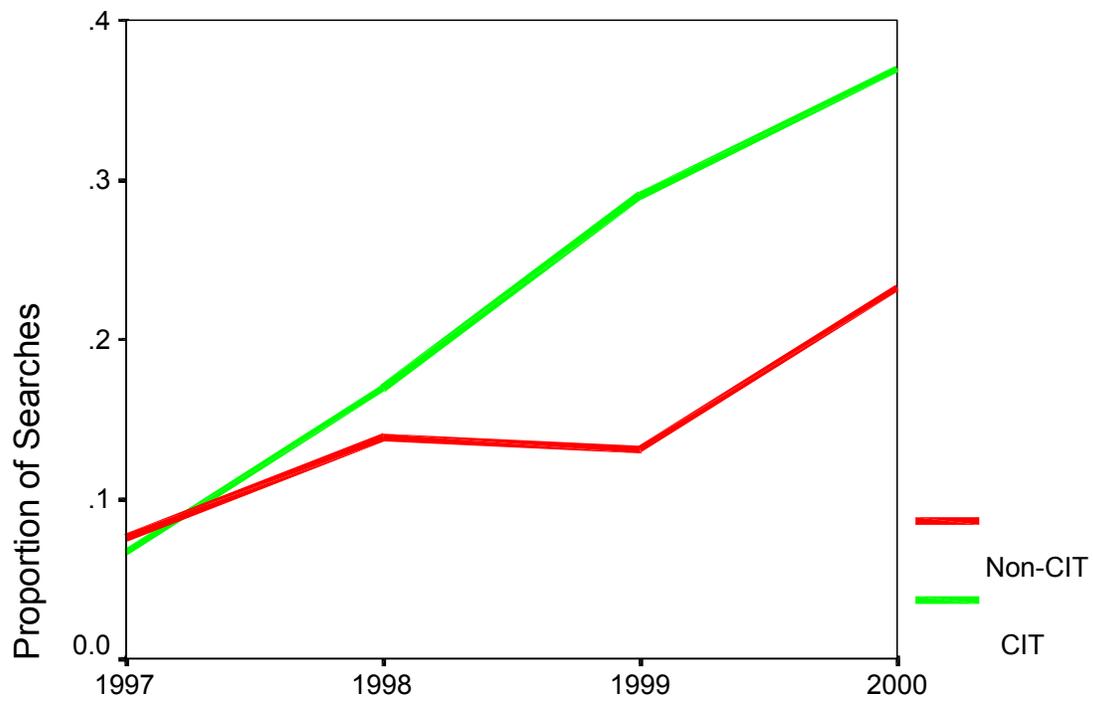
In 1997, African Americans stopped by troopers in Troop 1 were four times more likely to be searched than were whites who had been stopped. Although these odds dropped dramatically in 1998, they rose to three times the white odds in 1999 and 2000.

Troop 3 shows an even more dramatic pattern. In 1997, Troop 3 recorded less than thirty searches. Therefore, its time series begins in 1998, with an odds ratio just below 2. This then climbs slightly to 2.3 in 1999, and jumps to 6.5 in 2000. In the year 2000, African Americans account for 44.1 percent of the cars searched by Troop 3, but only 28.3 percent of those stopped. While this is a substantial disparity, more striking is the very low rate of white searches. While whites made up 69.4 percent of stops by the troopers of Troop 3, they account for only 14.7 percent of those searched. People of other races were 10 percent of stops and 32 percent of those searched by troopers in Troop 1.

There remains evidence that African Americans are searched at twice the rate of whites by troopers of the CIT in the year 2000, but this represents a dramatic decline since 1997. The much lower racial disparity in searches is consistent with the abandoning of racial profiling practices, or increased reliance on nonracial driver indicators by the CIT, given the accounts of how the CIT make their search decisions. The remaining disparity could easily be generated by stereotyping and the use of the conversational method for generating suspicion prior to asking for consent to search. It is also possible that the remaining disparity is produced by some non-bias process that we have not accounted for.

The other two troops showing substantial search activity in 2000, search African Americans at higher rates relative to whites who have been stopped. Because very few of the troopers in these troops ever search a car, it does not suggest racial profiling at the troop level. Individual troopers could be racial profiling, however.

Figure 4.4 Probable Cause as a Proportion of CIT and Non-CIT Searches, 1997-2000



Probable Cause versus Consent Searches

Consent searches have been identified in other jurisdictions as the most likely search type to exhibit racial disparity. Certainly the visible presence of contraband leading to probable cause leaves little room for bias, although, of course, some situations attributable to probable cause may be more subjective (for example, a strange odor). When a trooper's "sense" about drivers, including nervousness, influences a request for consent to search, much more room for bias is provided.

Figure 4.4 compares the proportion of probable cause searches for both the CIT and other NCHSP troopers. In 1997, less than 10 percent of searches were attributed to probable cause. Probable cause searches rose rapidly across the period. By 2000, 37 percent of CIT searches and 20 percent of other troopers' searches were based on reported probable cause. Based upon our interviews, we had expected to find that searches by regular troopers would be more likely than those by the CIT to have probable cause attributed to a search. This was not the case. In 1977, both the CIT and regular troopers who recorded searches rarely reported probable cause (10 percent). After 1998, the CIT was more likely than the few regular troopers conducting searches to record probable cause for a search. Because regular troopers rarely search, it may be the case that probable cause searches are under-recorded. It may also be the case that the CIT have made a greater effort to selectively search and, in so doing, are less inclined to request consent without substantial reason. Figures 4.5 and 4.6 compare the racial/ethnic proportion of probable cause searches across the same period.

Figure 4.5 presents searches initiated by CIT members. In 1997, African American and white drivers are equally unlikely to have probable cause attributed to the search. After 1997, for both white and African American drivers probable cause becomes more common, although the line is much steeper for African Americans. This chart also includes searches of people of other races and Hispanics combined. These represent only forty of the 387 CIT searches in 2000, and show the same basic pattern of increased probable cause, although consent searches are even more prevalent among CIT member searches of drivers who are neither white nor African American. The decline in the use of consent searches is consistent with a decline in the influence of federal guidelines for drug interdiction by the CIT.

Figure 4.5 Race and Probable Cause in CIT Searches, 1997-2000

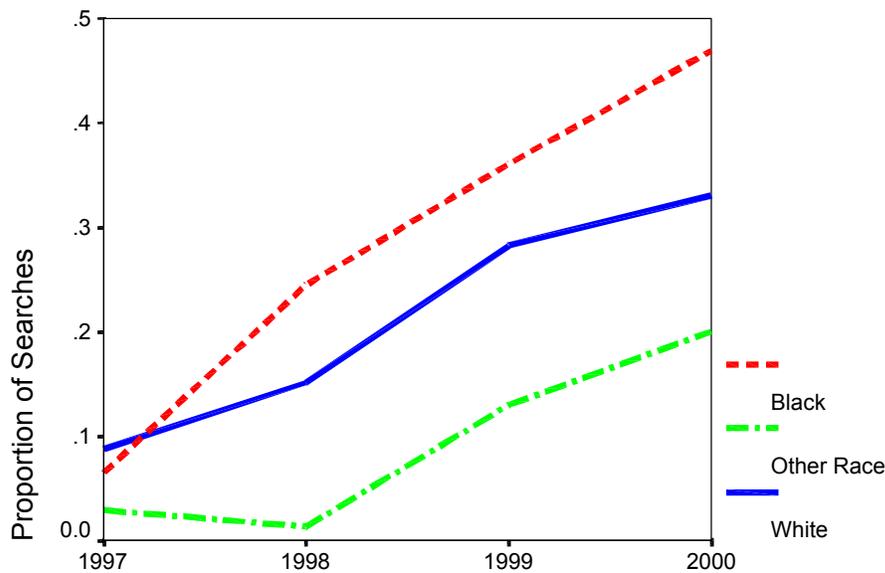


Figure 4.6 Race and Probable Cause in Non-CIT Searches

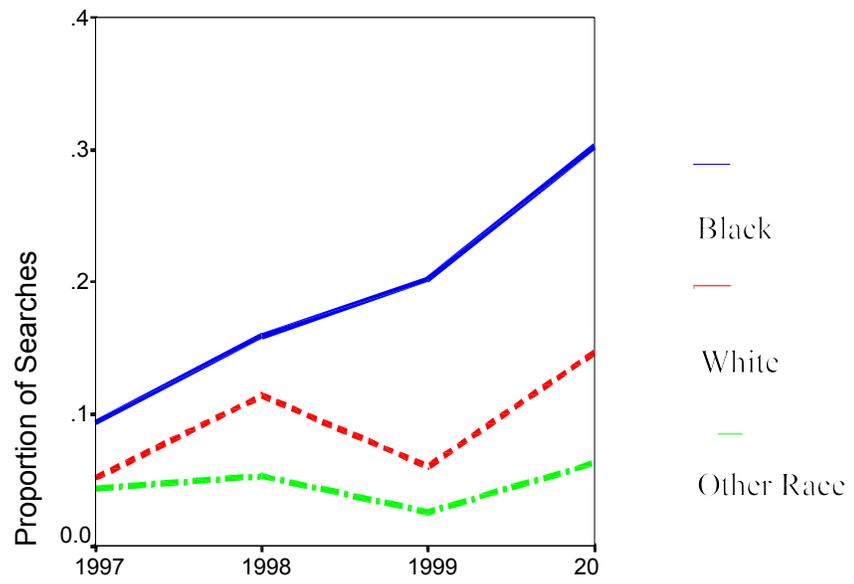


Figure 4.6 shows the results for non-CIT trooper searches. Searches attributed to probable cause increased dramatically in the searches of African Americans by regular NCSHP troopers. Surprisingly, the trends for white and “other race” stops are not so clear. If the decreased searches of African Americans and the decreased use of consent searches is a reaction to the 1996 charges of racial disparity, it seems to have changed the behavior of the CIT troopers for searches regardless of the race of the driver. For other troopers, consent searches seem to have declined only for African Americans. Although we do not report separate graphs here, the racial composition of probable cause searches has not declined, while the proportion African American among consent searches has declined since 1997 for both CIT and regular troopers’ searches. Thus, the decline in searches of African Americans documented in Figure 2 occurred entirely because of the decline of consent searches.

Our analysis of the incidence of probable cause reveals that between 1997 and 2000, probable cause searches became a higher proportion of all searches, as the use of consent searches declined. This pattern is particularly strong among the CIT members, and this occurred across all racial/ethnic groups compared in the CIT analysis. Regular road troopers do few searches and have dramatically reduced the use of consent searches of African American drivers—but not of white or “other race” drivers. The prediction based on troopers accounts that the CIT would make more use of consent searches is not supported by these analyses. Of course, the vast majority of regular troopers conduct few or no searches.

Hit Rates

Hit rates, or the proportion of searches which result in the successful identification and seizure of contraband, are potentially useful indicators of racial disparity in search decisions. If troopers are searching minorities at higher rates because of race rather than because they have good professional reasons to suspect contraband, then we would expect that a lower proportion of minority vehicles will be found to contain contraband. Essentially, in the presence of racially biased search decisions, we expect more innocent minorities to be searched than innocent whites.³⁴ Examining hit rates by race will provide some insight as to the likelihood that the search decision may be influenced by race over and above the magnitude of reasonable suspicion.

Figure 4.7 shows the hit rates for the CIT searches. In 1997, the proportion of searches that generated contraband were quite a bit lower for searches of African American and “other race” drivers than the hit rate for white drivers. The hit rate for “other race” drivers is considerably lower than for either African American or white searches. To the extent that race played a part in the determination of who to search, these results would suggest that CIT troopers were quite unsuccessful in their predictions and choices related to search decisions. The white and African American rates converge across the period. While the hit rate for “other race” drivers does rise, it still remains lower than that of the other two groups. These results overall show substantial racial disparity in the success rate for searches for 1997. This effect seems to have disappeared after 1998, as fewer African American drivers have been searched and fewer consent searches have been initiated by CIT members. Given that the CIT hit rates for African American

³⁴ There are issues of social efficiency around absolute hit rates that should be recognized as well. Even if there are no racial disparities, if hit rates are low, then many innocent drivers are being searched. The question then becomes at what level of hit rate is the trade-off between the invasion of innocent citizens’ privacy and potential drug interdiction acceptable?

and white drivers converged after 1998, the hit rates for searches of African Americans after 1998 surpass those of whites after 1998, and that the racial disparity in searches has dropped dramatically, the evidence points to earlier disadvantage for African Americans, possibly as a result of racially influenced practices by the CIT in 1997, but little or no disparity in current search practices.

Figure 4.7 Proportion of CIT Searches Yielding Contraband By Race, 1997-2000

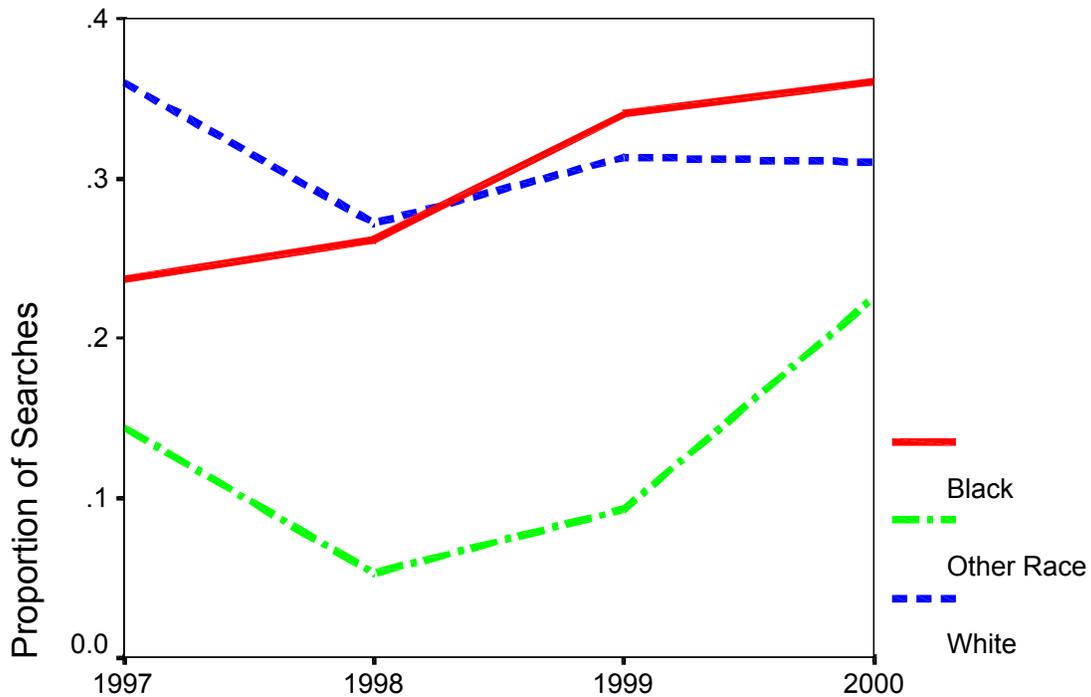


Figure 4.8 Proportion of Non-CIT Searches Yielding Contraband By Race, 1997-2000

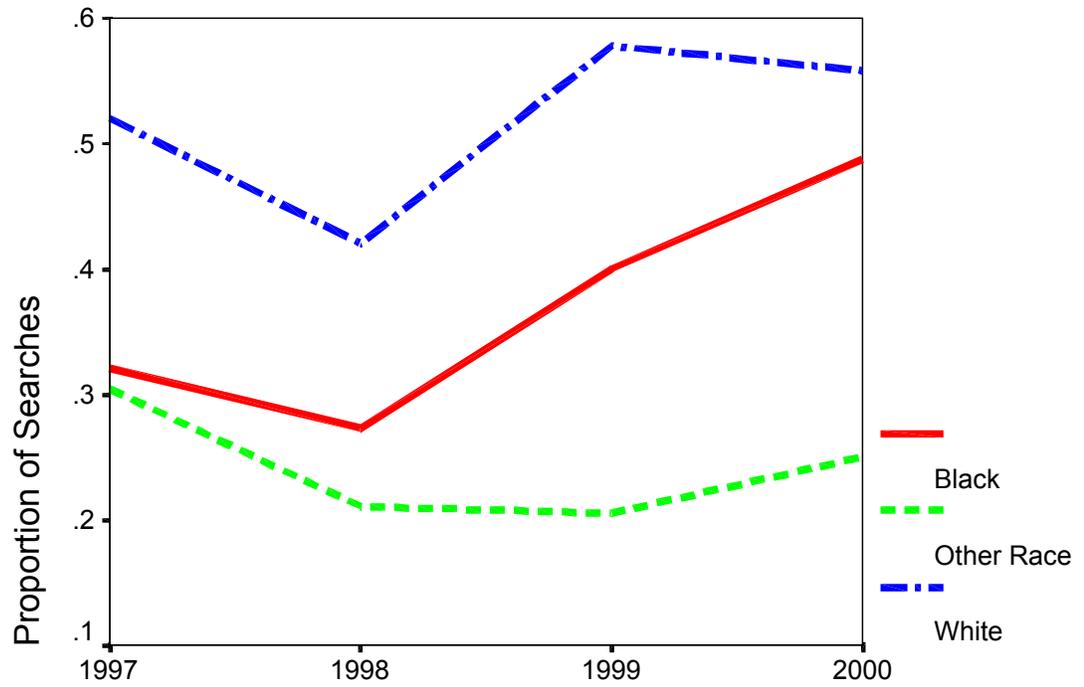


Figure 4.8 reports the same basic data for searches by the rest of the NCSHP. For all years, hit rates are higher for whites who were searched than for African Americans or drivers of other races. After 1998, the hit rates rise for searches of both African American and white drivers, reflecting the lower use of consent searches documented above. If there is any evidence of convergence in African American and white hit rates, it does not occur until the year 2000. There is no evidence of convergence in hit rates for drivers of other races. Although regular NCSHP troopers do very few searches, and these searches are concentrated by 2000 in only two troops and among five troopers, there is evidence not only of continued racial disparity in searches, but also of lower rates of contraband found in minority searches.

Individual Trooper Analyses

The analyses of search disparity reported above suggest that, outside of the CIT, a few troopers may be responsible for the disproportionate searching of African American vehicles relative to the proportion African Americans stopped. Inside the CIT, there is evidence of some race-linked disparity in searches, although it is much reduced compared to earlier years. In both cases, however, we used the racial composition of stops at the troop level as the baseline. Since there could easily be spatial and temporal heterogeneity within troops as discussed in previous chapters, these higher odds of African American searches may simply reflect where and when troopers patrol, rather than some racial disparity in search decisions.

In the year 2000, there were sixteen troopers with fifteen or more searches. These troopers include members of the CIT and the handful of regular NCSHP troopers who are actively (by our measure) searching vehicles. Three of the sixteen showed no excess searches of African

Americans compared to those of whites, when using the trooper's own racial distribution of citations and warnings as the baseline from which to compare the racial distribution of searches. Although the number of searches per trooper in 2000 is low, eight of the troopers had odds ratios of higher than 2, suggesting that they search African Americans at or above twice the rate of which they search white drivers. Given the racial distributions of citations and warnings, these troopers display a slightly elevated proportion of searches of African Americans relative to white searches. This suggests some racial disparity in the decision to search African Americans' vehicles.³⁵

Conclusion

There is considerable evidence in this chapter that there was a decline in search activity by the NCSHP after 1997, and that much of this decline arose from searching fewer African Americans. In 1997, the vast majority of searches were recorded as consent rather than probable cause searches, leaving more room for trooper discretion in whom to search. In 1997, all troops searched a higher proportion of African Americans than they did whites, although this varied

³⁵ There are an additional five troopers who had odds ratios above 1 but below 2. As stated in the main text, there are eight troopers who have odds above 2, suggesting they search African Americans at or above twice the rate of white drivers. Two of these troopers had odds ratios above 4, suggesting that they search African Americans at least four times as often as whites, given the number of African Americans and whites they stop for routine driving violations. The absolute number of searches is not high, ranging between 1 and 5 per month. Given these low absolute number of searches it is even possible that for any one of these individuals the disproportionate search of African Americans happened by chance. Also, the low numbers prohibit addressing "selection effect" interpretations of the data, such as time of day, day of week, and spatial heterogeneity concerns. Moreover, it should be noted that we have no information on the circumstances under which these searches are conducted. For example, these select few troopers may be called to the scene of an stop made by another trooper, and are called in because of their expertise in conducting vehicular searches. As such, the selection mechanisms for their searches may be quite different from that of other troopers.

from slightly higher odds of an African American search, to four times the odds in the CIT and in Troop 1. Across the period, there is considerable evidence of changes in the search-decision process of the CIT. The African American-to-white odds of a search fell from four-to-one in 1997, to two-to-one thereafter. The use of consent searches declined for all racial groups, and African American and white hit rates converged, and, significantly, the hit rate for African American searches surpassed that for whites after 1998. Although the CIT search data suggest that the drug interdiction profiles used prior to 1998 were possibly not unrelated to race, from 1998 on, the disproportionate searches of African Americans, to the extent they are related to race, is more likely to arise from the use of stereotypes or the residual nervousness of African American drivers during the “interview method” used to develop reasonable suspicion. The convergence of African American and white hit rates suggests that the CIT is not searching a higher proportion of innocent African Americans than innocent whites. Although there is clear improvement in hit rates, a higher proportion of “other race” drivers who are searched are not found to be holding contraband.

The pattern and extent of searches is different for regular NCSHP troopers. First, the vast majority of troopers do few or no searches. In fact, the vast majority of troops do less than thirty searches in a year. Two troops consistently generate more than thirty searches in a year and also search African Americans at relatively high rates. In the year 2000, five individual troopers accounted for almost all of the search activity in these two troops. In addition, hit rates for searches are consistently lower for African American and “other race” drivers who are searched by regular NCSHP troopers, suggesting that higher minority search rates are not, in fact, founded on the same level of reasonable suspicion.

The basic conclusion to be drawn from this analysis is that the vast majority of NCHSP troopers do no searches and so cannot be racially profiling in the search decision. To the extent that earlier charges of CIT profiling may have described search practices at that time, we find no substantiation for that claim at present. The analysis of individual troopers suggests that there are eight troopers with high levels of African American-white disparity in search behavior. It should be recalled, that even for these eight troopers there may be unmeasured and non-biased explanations for the observed higher searches of African Americans. While these numbers should not be ignored by NCSHP administration, they likewise should not be viewed as proof of profiling given the limited information on the trooper-citizen contact available for analysis.

Chapter 5 Citizen Survey Results: Racial Disparity in Self-Reported Stops

Whereas the earlier chapters focused on official records, this chapter reports the results of an analysis of police stop experiences using citizen self-reports from the North Carolina Driver Survey. We find evidence of racial disparity in self-reported stops, after controls for driver characteristics and reported driver behavior. Local police were found to exhibit a higher level of racial disparity than does the NCSHP. African Americans also report being stopped for somewhat more discretionary reasons and, to a small but significant extent, being treated with less respect during stops. There are no racial differences in the relative incidence of citations, written warnings, and verbal warnings. The evidence in this chapter, based on self-reported data, points toward greater racial disparity in the stop decision than in the interaction after the stop.

Introduction

The North Carolina Driver Survey was designed to complement the official statistics we have discussed previously. Official statistics are essentially the officers' recorded accounts of citizen-officer encounters. When all encounters are recorded, they can provide exceptional data on the racial distribution of stops. Official data do not contain very much information on driver characteristics, especially the ability to determine if drivers who are stopped report different driving practices or behavior than those who are not stopped.

If most drivers break the driving laws at some time, then there may be substantial officer discretion in deciding whom to stop. Where discretion is larger, so is the opportunity for racial bias in the stop decision to enter into the process. The official data we have been examining

focuses on the NCSHP. The various local police forces operating in North Carolina actually stop more drivers than does the NCSHP. The survey data allow us to capture drivers' self-reports of all stops by all police and to make distinctions between stops by the NCSHP and those by officers attached to other police forces.

The survey data also allows us to collect information on typical driving behaviors that may influence the probability of being stopped. In the survey we not only asked North Carolina drivers whether or not they were stopped, but also why they were stopped, the outcome of the stop, and whether or not they felt that they were treated with respect during the stop. Finally, the survey allows us to explore the linkages among race, stop experiences, and self-reports of driving practice and behavior.

Sample Characteristics

The North Carolina Driver Survey is a telephone survey of a stratified random sample of current North Carolina licensed drivers. The sample was stratified by race in order to have sufficient sample sizes to compare the experiences of white and African American drivers. The sampling frame included white and African American drivers who had applied for or renewed their licenses in the previous six months. Using this method we had expected to get phone numbers and addresses that were relatively current. Unfortunately, it turns out that the N. C. Department of Motor Vehicles rarely asks for the telephone number, nor does it require proof of home address for license renewals. Thus, we had to use a telephone match based on surname and address to develop useful contact information for our sampling frame. The return on the telephone match was 48.6 percent, lowest for African American females at 39.0 percent and

highest for white males at 62.8 percent. Cooperation rates on the survey were much better at 59.1 percent, with a high for African American females of 61.8 percent and a low for white males of 56.5 percent. Data were collected between June 22, 2000, and March 20, 2001. Half of the cases were collected by September 11, 2000.

A comparison of our final sample to the actual race-gender-age distribution of licensed drivers in North Carolina shows that our final sample is quite a good match to the state distributions (see Table 5.1). In all four gender-race groups, young adults age 30–39 are under represented. In most statistical analyses we weight the data to correspond to the known gender and age distributions of licensed drivers within the two racial strata. We call this the “DMV weight” because it refers to the distribution of drivers in the N.C. Department of Motor Vehicles’ license registry.

Racial Differences in Stops

One of the primary goals of the survey is to see if there are racial differences in the probability of police stops. Because police are expected to respond to driver behavior in making the decision to stop, we also collected information from respondents about their typical driving behavior. Information on other attributes of the driver that may motivate police stops or influence driving behavior such as age, gender, education, home ownership, and car model and age was collected.

	African American		White	
	Survey Percent	DMV Percent	Survey Percent	DMV Percent
Males 18–29	12.4	13.3	9.3	10.2
Males 30–39	9.4	11.8	8.7	10.7
Males 40–49	11.5	10.3	10.9	10.2
Males 50–59	7.2	6.2	9.1	8.1
Males 60+	6.6	6.0	10.4	10.3
Females 18–29	14.3	13.5	10.2	9.8
Females 30–39	8.6	12.6	9.1	10.4
Females 40–49	13.7	11.5	11.8	10.2
Females 50–59	8.2	7.1	8.9	8.3
Females 60+	9.3	7.6	11.9	11.6

We were concerned about potential reporting errors in response to our questions about police stops and driving behavior. Being stopped by the police for speeding or other reasons is potentially embarrassing. It is well known in survey research that respondents tend to under-report embarrassing behaviors. As described in Appendix E, we conducted a record check survey of almost 600 drivers with known speeding stops in the last year in order to ascertain the degree of under-reporting of stops we could expect in the driver survey. The record check survey showed that 74.8 percent of whites admitted being stopped in the last year. For African Americans the number was 66.8 percent. This racial difference in self-reports is statistically significant at a .019 probability. These results suggest that in the larger driver survey we would expect to find respondents who claimed no stops in the past year, but who were in fact stopped. And, while both groups under-report speeding stops, African Americans do so at a higher rate. This finding is consistent with many past studies that report stronger social desirability effects on survey responses among African Americans. Thus, the North Carolina Driver Survey data will tend to underestimate the number of stops for both African Americans and whites and it will also

underestimate the magnitude of the racial disparity in stops.³⁶ In most analyses we present results that have been weighted to reflect the expected racial difference in non-response to the questions about stops.³⁷ We refer to these analyses as using the Record Check weights. In general, the record check weights increase our estimates of the number of stops and increase the racial disparity in stops, but should more closely mirror the actual racial gap in stop experiences.

Comparison of Driver Survey Estimates of Stops to Official Records

There are reasons to believe a driver survey might underestimate the actual number of stops and other reasons to suspect they might overestimate the actual number of stops (see Bradburn 1983). From the record check survey, we developed race-specific estimates of the degree of under-reporting, and we can use those estimates to weight the data to correct for this possible source of under-reporting. In the survey we asked respondents to recall the stops they had experienced over the last year. Survey questions that ask the respondent to recall events are also subject to telescoping errors in which respondents report on events that actually took place outside time period parameters specified in the survey question. In our case we ask for stops in the last year. If telescoping is occurring, weighting the data to correct for under-reporting may produce larger estimates of the number of the stops in the last year than actually occurred. To the

³⁶ The record check survey also shows that people who do not report a stop also report fewer risky driving behaviors and less speeding. For non-threatening questions such as miles driven there are no differences between those who report stops and those who do not.

³⁷ Given these record check results, we weight white respondents who admit to stops at 1.34228 (1/.78) and African American respondents who admit to stops are weighted at 1.4947 (1/.68). This requires weighting whites and African Americans who do not admit to stops at less than one in order to preserve the original sample size. These weights are .9245 for whites who do not report stops and .8228 for African Americans who do not report stops.

extent that telescoping is associated with race, our estimates of racial differences in stops may be effected.

In the Table 5.2 we produce population estimates of stops by the NCSHP in the last year from the North Carolina Driver Survey with DMV weights and with Record Check weights. We compare these to the estimates of stops from the official 2000 citation records of NCSHP troopers.

The citation estimates from the NCSHP trooper's citation reports are very similar to those based on the survey responses using DMV weights. For both African American and white citation estimates the actual NCSHP citation count is within a single standard deviation of the survey estimates using the DMV weights. When the record check weights are used for both African American and white drivers, the survey estimates of citations are much higher than the recorded citations in the NCSHP citation reports. These analyses suggest that the point estimates based on the DMV weights are surprisingly accurate.

Given the under-reporting we found in the record check survey, we would have expected that the record check weights would have been closer to the official records. It would appear that reports that telescope the time period are prevalent, and that telescoping over-reporting and refusal-to-admit-a-stop underreporting roughly cancel each other out. In general, this suggests that the yearly incidence of stops is best captured by the DMV weighted data.

Table 5.2 Comparison of Survey and Official Record Estimates of African American and White Total Citations in Past Year				
	Survey Population Estimate of NCSHP Citations in Last Year	Confidence Interval at 66 Percent Confidence Level	Confidence Interval at 95 Percent Confidence Level	Estimate from NCSHP 2000 Citation Reports
African American Drivers				
DMV Weights	112,228	99,986 to 124,470	87,744 to 136,715	101,909
Record Check Weights	167,685	152,947 to 182,423	138,208 to 197,162	
White Drivers				
DMV Weights	274,066	248,220 to 299,912	222,374 to 325,758	294,241
Record Check Weights	368,597	338,976 to 398,218	309,355 to 427,839	

The Record Check weights may do a better job of capturing the racial gap in self-reports, since they incorporate information from the Record Check survey on racial differences in the probability to report stops in a survey. These estimates probably refer now to a period closer to eighteen months. We also use the Record Check weights in the multivariate analyses that follow because risky driving behaviors, are associated with the failure to report a stop in the record check analyses, and are some of the primary presumed legitimate causes of stops in the multivariate models below.

Racial Differences in the Reported Stop Experience

Table 5.3 reports the race-gender-age distribution of stops from the North Carolina Driver Survey. These distributions are for drivers who self-report being African American or white and are weighted to correspond to the North Carolina Department of Motor Vehicles distributions of

age and gender within race.³⁸ Young drivers tend to have more stops than older drivers and African American male drivers tend to be stopped more often than white drivers.

Young African American drivers report an average of 1.24 stops in the past year, almost twice as many as young white drivers. Almost all of the racial disparity in stops among young male drivers is produced by stops by local police. NCSHP stops of young African American male drivers (17.0 percent) and young white male drivers (15.5 percent) are very similar. This same pattern holds up for older male drivers. In all cases there are racial disparities in stops, but the racial disparity is larger for local police than it is for the NCSHP. It is also the case that while the absolute probability of being stopped drops for African American and white males as they age, the relative racial disparity in stops (the odds ratio of African American to white stops) tends to rise with age, especially for stops by the local police.

Table 5.3. Male Age and Race Distribution of Stops from the North Carolina Driver Survey, 2000.
DMV 2000 Licensed Driver Weights

	18–22		23–49		50+	
	African American	White	African American	White	African American	White
Number of Stops mean (s.d.)	1.24 (2.48)	.68 (1.38)	.51 (.93)	.33 (.77)	.26 (.59)	.17 (.61)
Any Stop	42.1%	32.1%	32.6%	24.0%	18.8%	12.5%
Odds Ratio	1.31		1.36		1.50	
Local Officer Stop	35.1%	25.9%	23.4%	14.9%	13.3%	7.0%
Odds Ratio	1.36		1.57		1.90	
NCSHP Officer Stop			13.8%		6.0%	5.9%
Odds Ratio	17.0%	15.5%		10.4%	1.02	
Odds Ratio	1.10		1.33		1.02	
Sample Size	104	90	400	385	156	267

³⁸ Although we sampled only people who were listed as African American or white in the DMV records a few respondents claimed other ethnic/racial affiliations. These people are dropped from the analyses. A few people refused to answer the race question, these were assigned the DMV race and kept in the analyses. The original sample somewhat underestimates people in the 30–39 age group for all four race-gender categories. The DMV2000 weights correct for these underestimates.

Table 5.4 displays similar analyses of racial differences in stop experiences among women. Women tend to be stopped less often than men at all ages and among both African Americans and whites. Among women, police stop experiences also decline with age. Young African American women report about a third as many stops as young African American men. Young white women report about half as many stops as young white men. As among men, there are racial disparities in stops and the disparity is larger in stops by local police than in stops by the NCSHP. In fact, among young women, white women report slightly more stops by the NCSHP than do African American women. While among older women African Americans report more stops by the NCSHP, the racial disparity is very small. For local police, the relative racial disparity in stops as measured by the odds ratio increases dramatically with age. Very few older white women are ever stopped by the local police, while 9.6 percent of older African American women report stops in the last year by local police officers.

Table 5.4. Female Age and Race Distribution of Stops from the North Carolina Driver Survey, 2000. DMV 2000 Licensed Driver Weights

	18–22		23–49		50+	
	African American	White	African American	White	African American	White
Number of Stops mean (s.d.)	.38 (.61)	.33 (.63)	.36 (.92)	.23 (.52)	.26 (1.10)	.01 (.31)
Any Stop	32.7%	25.9%	24.6%	19.2%	13.2%	8.2%
Odds Ratio	1.26		1.28		1.61	
Local Officer Stop	22.2%	16.0%	16.8%	11.9%	9.6%	3.7%
Odds Ratio	1.39		1.41		2.60	
NCSHP Officer Stop	10.2%	12.3%	8.8%	8.4%	4.5%	4.1%
Odds Ratio	.83		1.05		1.10	
Sample Size	99	81	411	370	198	294

Table 5.5 reports racial comparisons in stops, driving behaviors, and demographic background weighted to correspond to the 2000 age-gender-race distributions of North Carolina

drivers. In order to make good use of the sample sizes available, statistics are compared for white and African American North Carolina drivers across all age and gender groups. In multivariate models that follow we statistically adjust for gender, age, and other factors that may explain the racial gap in police stops.

Slightly more than a quarter (26.4 percent) of African American North Carolina drivers reported a stop in the last year as compared to 18.1 percent of whites. African Americans are 8.3 percent more likely than whites to report being stopped by the police. African Americans are stopped more often (total stops average .43 versus .25), are 7.6 percent more likely to be stopped by the local police, and are 1.3 percent more likely to be stopped by the NCSHP. All of these racial disparities in stops, except stops by the NCSHP, are statistically significant.

The National Institute of Justice recently released national survey estimates of racial differences in driver stops by police. Their self-report data on police stops show 10.4 percent of white drivers and 12.3 percent of African American drivers reporting a stop in the last year. This suggests that the overall rate of police stops in North Carolina is almost twice the national average. The racial gap in stops by local police is also quite a bit higher in North Carolina than in the rest of the country. Stated another way the self-reported racial gap in police stops by the NCSHP is about half of the national average, but the racial gap in reported stops by local police in North Carolina is almost four times greater than the national average. This may suggest that local police more aggressively enforce traffic laws in North Carolina. It is also quite possible that the national study understated the actual rate of stops.

Racial Differences in Reported Driving Behavior

One of the advantages of a survey approach is that we can collect information on reported driving behavior. If there are racial differences in typical driving behavior they may explain some of the observed differences in police stops. The third panel of Table 5.5 displays racial differences in driving behaviors. The first two entries refer to miles driven. It seems reasonable to predict that people who drive more are at greater risk of being stopped by the local police. This risk could be based simply on increased opportunity to encounter police officers. In addition, if most people occasionally break the driving laws, more driving increases the probability that a person would encounter a police officer while breaking the law. On average whites drove significantly more miles in the last week and across the last year than did African Americans. Whites average more than 3,000 more miles per year than African Americans.³⁹ Thus, miles driven cannot explain the racial gap in stops.

Another way to think about the racial gap in stops is to ask how many miles the average white or African American drives before being stopped. On average, North Carolina African American drivers drive 32,681 miles before being stopped. Whites in North Carolina report driving more than twice as far—68,944 miles per stop. Whites are also significantly more likely than African Americans to drive on interstate highways.

³⁹ Using the 1999 National Transportation Survey, we compared self-reported miles driven in the last year by African Americans and whites for the nine major census divisions. In every region of the country, whites drive more on average than African Americans. The gap was largest in the West North Central and New England regions—at more than 6,800 miles per year and lowest in the Middle Atlantic and Mountain states—at less than 1,000 miles per year. The estimate for the East South Central region, which includes North Carolina, was that African Americans drive 5,400 fewer miles per year than do whites.

In terms of breaking the traffic laws, whites report slightly higher average speeds in 65, 55, and 35 mph speed zones. We experimented with various measures of speeding at thresholds of 5, 6, 10, and 15 mph above the speed limit. In general there are no significant racial differences in self-reports of typically speeding at these thresholds.⁴⁰ In the multivariate analyses that follow, we use a three-item speeding scale based on a five mile per hour speeding threshold for three hypothetical speed limits—35 mph in town, 55 mph on a two-lane highway, and 65 mph on an interstate highway. We chose this measurement because it was more highly correlated with the probability of a stop than alternative measures of speeding behavior. Because there are no average racial differences in self-reported speeding behaviors this variable cannot account for the racial disparity in police stops.

We also asked drivers if they used methods to avoid getting stopped for speeding. Very few drivers use any method to reduce the chance of being stopped, although cruise control is most common. Whites were significantly more likely than African Americans to report using cruise control, listening to a CB-radio, and watching and monitoring the speed of commercial trucks. There were no racial differences in the use of radar detectors. If these methods reduce the risk of being stopped they may account for some of the racial disparity in police stops observed in these data. In the multivariate analyses we use an additive scale called “Methods to Avoid Speeding Tickets” based on these four items. High scores mean the respondent rarely uses these methods. A reliability analysis shows that these items are not highly correlated with each other.

⁴⁰ The one exception is displayed in the table. Whites are significantly more likely to say they typically drive 40 mph or above in a 35 mph speed zone than are African Americans.

Table 5.5. Racial Differences in Stops, Driving Behavior and Demographic Background, Weighted by 2000 DMV Race-Gender-Age Distributions

	African American	White	Significant Difference?
STOP EVENTS			
Any Stop in Last Year	26.4%	18.1%	Yes
Total stops last year	.43 (1.11)	.25 (.67)	Yes
Any Stop by a Local Policeman in Last Year	18.8%	11.2%	Yes
Any Stop by NCSHP in Last Year	10.0%	8.2%	No
Helped by Officer in Last Year	6.5%	5.2%	No
Officer at Accident Scene in Last Year	10.5%	8.1%	Yes
DRIVING BEHAVIORS			
Miles driven last week	211 (428)	274 (452)	Yes
Miles driven last year	14,053 (30,515)	17,236 (34,002)	Yes
How often drive on interstates? (1=everyday...7=never)	3.35 (1.98)	3.09 (1.75)	Yes
Average speed in a 65 mph zone	67.5 (4.4)	68.2 (3.9)	Yes
Typically drive 70 in a 65 mph zone	48.8%	51.9%	No
Average speed in a 65 mph zone	57.3 (4.2)	57.9 (3.7)	Yes
Typically drive 60 in a 55 mph zone	42.9%	45.0%	No
Average speed in a 65 mph zone	36.1 (3.5)	36.7 (3.0)	Yes
Typically drive 40 in a 35 zone	24.6%	28.0%	Yes
Scale of 5+ over limit speeding behavior	1.17 (1.13)	1.25 (1.13)	No
Scale of 10+ over limit speeding behavior	.34 (.57)	.35 (.58)	No
To avoid getting a speeding ticket, do you, (1=All of Time...4=Never)			
Use cruise control?	3.28 (.96)	2.95 (1.04)	Yes
Use a radar detector?	3.91 (.44)	3.90 (.49)	No
Listen to a CB?	3.84 (.60)	3.91 (.45)	Yes
Watch and follow trucks?	3.60 (.84)	3.72 (.67)	Yes
Frequency of methods used to avoid speeding tickets	3.66 (.42)	3.62 (.38)	Yes
Do you always use seatbelts?	92.3%	89.2%	Yes
Do you always use turn signal?	85.4%	71.9%	Yes
Do you change lanes in order to get somewhere more quickly?	55.8%	55.1%	No
Do you pass slow cars on a two-lane highway?	74.6%	77.1%	No
Do you ever roll through a stop sign?	23.9%	34.4%	Yes
Do you ever speed up to get through a yellow light?	51.6%	61.8%	Yes
Scale of risky driving behaviors	2.18	2.57	Yes

	(1.31)	(1.34)	
DEMOGRAPHIC BACKGROUND			
New Driver (four or fewer years driving)	5.2%	1.7%	Yes
Age	41.00 (15.3)	44.8 (15.8)	Yes
Male	47.7%	49.6%	No
Education Scale (1=No High School...7=Grad School Degree)	3.68 (1.85)	4.14 (1.94)	Yes
Own Home	67.8%	84.6%	Yes
Urban/Rural (1=City....4=Country)	2.0 (1.28)	2.5 (1.23)	Yes
Model Year of Car Typically Driven	93.02 (5.35)	94.01 (5.01)	Yes

Rather they are functional alternatives. None are used frequently, although many people use one. Only 37.4 percent of drivers report never using any of these methods.

Finally, we asked about a series of risky driving behaviors. African Americans reported significantly higher seat belt and turn signal use than did whites. They also reported lower likelihood of rolling through a stop sign or speeding up to get through a red light. There were no significant racial differences in lane-changing or passing on a two-lane highway.⁴¹ For the multivariate analyses we created a scale of “Risky Driving Behavior” by summing these six items. Inter-item reliability is only moderate (Alpha=.45).

African Americans report driving fewer miles, are less risky drivers in terms of reported driving behavior, and report driving slightly slower on average than do white North Carolina drivers. White drivers, on the other hand, use more methods to avoid being pulled over by the police. These descriptive statistics suggest that the reported driving behavior is unlikely to provide powerful explanations for the observed racial disparity in police stops. Because whites

⁴¹ In our analysis of the record check survey (reported in Appendix E) we checked to see if either whites or African Americans who failed to report a stop also reported fewer risky behaviors or slower driving speeds. They did-, so we repeat the multivariate analyses using record check weights to see if reluctance to report stops or bad driving behavior influences results.

report driving slightly worse than African Americans, controlling for driving behavior is likely to *increase* the estimate of the size of the racial gap in stops in the multivariate models that follow.

Racial Differences in Demographic Background

The bottom panel of Table 5.5 compares African American and white drivers' demographic backgrounds. Here there are substantial racial differences. African Americans are significantly more likely than whites to be inexperienced drivers. Compared to white drivers, African Americans are also on average younger, slightly less educated, less likely to own their own home, live in more rural areas, and drive slightly older cars.

These demographic differences are potentially important explanations of the racial disparity in police stops. The public concern with racial bias in police stops implies that police discretion coupled with police reaction to driver's status attributes combine to produce racial disparity in the pattern of stops. If this is a reasonable model of the causal process, then we would expect that other status attributes that are associated with police perceptions of driver risk or dangerousness will also be associated with the decision to stop. In particular, we would expect males, younger drivers, and economically disadvantaged drivers to have higher probabilities of stops. African American drivers tend to be slightly less likely to be male than white drivers, but to be younger, slightly less educated, less likely to own a home, and drive slightly older cars. If police stop decisions are influenced by status characteristics such as these they may explain some of the racial disparity in stops.

In addition, in multivariate models we can see the extent to which driving behaviors explain status linked disparity in stops. To the extent that they do, police may be reacting to driving behavior rather than status attributes. Strong status attribute associations with stops after

controlling for driving behavior suggests that police perceptions of status attributes such as race, gender, or age may be important direct influences on the decision to stop a car.

Modeling Police Stops in a Multivariate Context

The vast majority of drivers who experienced a stop were stopped only once in the previous year. For this reason we model the likelihood of a stop in a logistic regression multivariate framework. In this statistical framework a dependent variable, coded 1 to indicate the presence of the outcome (in this case a stop in the last year) and 0 to indicate its absence (no stop), is regressed on a series of explanatory variables. The statistical model is estimated using maximum likelihood methods, and it predicts the log of the odds of the outcome occurring (1) versus not occurring (0) as the dependent variable. In the tables that follow we display the exponent of the log-odds coefficient which can be interpreted as the multiplicative change in the odds associated with a one unit change in the independent variable. So, for example, in Table 5.6, column 1, the reported odds coefficient associated with race is 1.63. This means that for African American drivers, their odds of being stopped by the police in the last year are 1.63 times higher than that of white drivers.⁴² Odds below one mean that the outcome is less likely. Odds above one mean that the outcome is more likely as such explanatory variables as race, age, or risky driving behaviors increase. In addition, these multivariate models are all estimated twice, once

⁴² These models were estimated using only cases with no missing values. In addition, four cases were excluded because the respondent claimed not to have driven in the last year. Models include the key explanatory variables that we have available. Alternative operationalizations of miles driven and speeding were explored but those selected were the most strongly associated with the probability of stops. In addition, interactions between miles driven and risky behavior, methods to avoid tickets, and interstate frequency were explored for all three stop measures. In no case were these interactions statistically significant. Finally, including a dummy variable for inexperienced driver in no cases increased the probability of being stopped in the last year beyond the effect of simple age.

with data weighted to conform to the 2000 North Carolina DMV records of race-age-gender distributions of licensed drivers and then with a second weight based on the record check survey to correct for racial differences in the under-reporting of stops.

The logic of the analysis is to first establish the size of the racial disparity in stops. We then enter a series of demographic control variables to see if they help explain the observed racial disparity in the first model. To the extent that these demographic variables are also associated with the probability of stops they, like race, are associated either with driving behaviors or with police discretion in stops decisions. The third model introduces the series of self-reported driving behaviors. To the extent that these behaviors encourage police stop decisions they should be significantly associated with stop outcomes. In addition, if status characteristics are associated with different driving behaviors, adding driving behaviors to the models should erode the coefficients associated with demographic status characteristics. If the coefficients for demographic background are not eroded then this suggests that police are often reacting to drivers' status characteristics rather than their typical driving behavior. The final set of models separates the analysis by race and allows us to see if police react to both demographic attributes and driving behavior similarly for white and African American drivers. All analyses are repeated for any stop in the past year, and for stops by the local police and the NCSHP.

Table 5.6 displays the analyses of any stop in the last year. In the first model we see that the odds of an average African American driver in North Carolina being stopped at least once in the last year is 1.63 times higher than it is for an average white North Carolina driver. The estimate (weighted to account for non-responses from the record check survey) indicates that the African American odds of being stopped are 2.03 times greater than whites. Because the record

check weights correct for under-reporting, but not for telescoping of the recall period, the latter racial gap refers to the odds of being stopped in the last year and half.

Adding the demographic variables to the model, substantially increases the model Chi-Square suggesting that the odds of being stopped are strongly influenced by these non-racial status characteristics. The coefficient associated with race is reduced by about 10 percent using both sets of weights. The racial disparity in stops is still statistically significant. We also see that women are .70 times less likely than men to have been stopped in the last year; that increased education is associated with increased incidence of stops; and that newer cars are stopped less often, as are older drivers. Rural areas, urban areas, and home ownership are not associated with the likelihood of a stop.

Table 5.6. Logistic Regression of **Any Stop Last Year** Upon Race, Demographic Background, and Driving Behavior; multiplicative odds coefficient and significance level reported.

<i>Analyses weighted to 2000 DMV population count (N=2570)</i>	Model 1	Model 2	Model 3	African American	White
Race (1=AA)	1.63***	1.46***	1.71***	N=1,209	N=1,361
Gender (1 =Female)		.70***	.84	.77	.96
Education		1.06*	1.02	1.01	1.04
Home Owner		.84	.83	.84	.81
Model Year of Car		.97***	.97***	.97***	.96**
Age		.97***	.97***	.97***	.97***
Rural		1.03	1.03	1.05	1.01
Scale Speed 5+			1.07	1.06	1.07
Miles Driven Year (LN)			1.14*	1.06	1.39***
Risky Driving Scale			1.13**	1.12*	1.13
Fewer Methods to Avoid Ticket Scale			.79	.85	.78
Interstate Frequency (low score=high usage)			.97	.93	1.04
Degrees of Freedom	1	7	12	11	11
Model Chi-Square	26.47	166.85	206.83	84.20	108.29
Model Probability	***	***	***	***	***
<i>Analyses weighted to record check estimate of non-response bias (N=2588)</i>					
	Model 1	Model 2	Model 3	African American	White
Race (1=AA)	2.03***	1.83***	2.15***	N=1,215	N=1,373
Gender (1 =Female)		.70***	.85	.78	.96
Education		1.06*	1.02	1.01	1.04
Home Owner		.84	.82	.83	.80
Model Year of Car		.98**	.97***	.97*	.96**
Age		.97***	.97***	.97***	.97***
Rural		1.03	1.03	1.05	1.01
Scale Speed 5+			1.07	1.06	1.07
Miles Driven Year (LN)			1.15***	1.07	1.39***
Risky Driving Scale			1.13***	1.13*	1.12*
Fewer Methods to Avoid Ticket Scale			.79*	.84	.79
Interstate Frequency (low score=high usage)			.98	.93	1.04
Degrees of Freedom	1	7	12	11	11
Model Chi-Square	70.15	243.43	293.50	104.17	133.31
Model Probability	***	***	***	***	***
Probability levels of * .05; ** .01; ***.001 or below					

Model three introduces the measures for reported driving behaviors. Drivers who drive more miles in a year, those who typically drive using more risky driving behaviors and those who use more methods to avoid getting speeding tickets all are stopped more often. Self-reported speeding behavior is not significantly associated with increased stops, although the coefficient is

in the correct direction. The result for methods to avoid getting tickets is at first surprising. Drivers who consciously try to avoid getting tickets are stopped more often than other drivers. Evidently these drivers know they are breaking the law, and their increased scanning behavior to reduce police contacts is not sufficient to offset their increased speeding driving behavior.

Because African Americans report driving fewer miles per year, less risky driving behavior, and are less likely to use methods to avoid speeding tickets than whites, the race coefficient actually gets larger in Model 3. In the DMV-weighted models, after accounting for demographic and driving behavior differences, the odds that an African American was stopped in the last year are 1.71 times higher than they are for whites. When the data are weighted to account for potential non-response to stop questions the level of racial disparity rises to 2.15 times higher odds of a stop if you are African American. Driving behavior coefficients are substantively identical using both weighting systems, suggesting that social desirability effects on reports of stops and driving behavior are not large or systematic-enough to influence results.

Introducing driving behavior reduces both the gender coefficient and the education coefficient to non-significance. This suggests that the higher likelihood of being stopped among men and among more highly educated drivers is a function of their driving behavior. The effects of driver age and year of vehicle are not similarly reduced, suggesting that police tend to react to older drivers and newer cars in a more forgiving manner than they do to younger drivers and older cars. These results also help highlight that police discretion in stop decisions are not limited to race. There may also tend to be age and class biases in the stop decision of some police officers.

In the last two columns the models are split by race to see if any of the factors that encourage or discourage police stops operate differently for African American and white drivers.

The only variable that affects the odds of being stopped differently for African American and white drivers is “miles driven.” Driving more miles substantially increases the odds of a stop for whites. Miles driven for African Americans seems to be unrelated to the likelihood of a stop. Since officers cannot observe the number of miles driven, this result suggests that for whites a key risk factor increasing the likelihood of a stop is miles of potential driving exposure to police surveillance. For African Americans, their race seems to be a risk factor in its own right, a factor more powerful than miles driven.

After accounting for other demographic factors which might attract police attention and driving behavior these models suggest that the degree of unexplained racial disparity, as measured as the relative odds-ratio of African American to white police stops is somewhere between 1.71 and 2.15. This is a substantial level of unexplained racial disparity and so potential racial bias in police stops.

Table 5.7 presents an identical analysis except that it is limited to stops by local police officers. In Model 1—under both weighting protocols, we find larger racial disparities in stops than were observed in Table 5.6 for all stops. Again, controlling for demographic background somewhat reduces the degree of racial disparity in stops, and gender, car age and driver age influence the likelihood of a stop by a local police officer. Driving behaviors are not particularly important determinants of stops by local police. The one exception is that drivers who use the interstate less often are also stopped less frequently by local police. This seems to be particularly the case for African American drivers.

Table 5.7. Logistic Regression of Local Officer Stop Last Year Upon Race, Demographic Background, and Driving Behavior; multiplicative odds coefficient and significance level reported.					
<i>Analyses weighted to 2000 DMV population count (N=2581)</i>	Model 1	Model 2	Model 3	African American	White
Race (1=African American)	1.84***	1.64***	1.77***	N=1,210	N=1,360
Gender (1=Female)		.66***	.71***	.69*	.77
Education		1.06	1.04	1.00	1.09
Home Owner		1.06	1.05	1.04	1.11
Model Year of Car		.97***	.97**	.97*	.97*
Age		.97***	.97***	.97***	.97***
Rural		.95	.96	.95	.98
Scale Speed 5+			1.01	1.00	1.03
Miles Driven Year (LN)			1.03	1.00	1.15
Risky Driving Scale			1.09	1.04	1.16*
Fewer Methods to Avoid Ticket Scale			1.03	1.09	1.01
Interstate Frequency (low score=high usage)			.94	.91*	.99
Degrees of Freedom	1	7	12	11	11
Model Chi-Square	30.57	110.58	151.08	64.33	64.15
Model Probability	***	***	***	***	***
<i>Analyses weighted to record check estimate of non-response bias (N=2588)</i>					
	Model 1	Model 2	Model 3	African American	White
Race (1=AA)	2.22***	1.97***	2.10***	N=1,215	N=1,373
Gender (1 =Female)		.67***	.71***	.69***	.76
Education		1.06*	1.04	.99	1.09*
Home Owner		1.08	1.07	1.07	1.13
Model Year of Car		.97***	.97***	.96***	.97***
Age		.97***	.97***	.97***	.97***
Rural		.93	.95	.93	.98
Scale Speed 5+			1.00	.99	1.03
Miles Driven Year (LN)			1.02	.99	1.13
Risky Driving Scale			1.08	1.03	1.15*
Fewer Methods to Avoid Ticket Scale			1.07	1.13	1.05
Interstate Frequency (low score=high usage)			.94*	.90**	.98
Degrees of Freedom	1	7	12	11	11
Model Chi-Square	68.21	204.10	215.31	80.36	77.77
Model Probability	***	***	***	***	***

Probability levels of * .05; ** .01; ***.001 or below

In general the determinants of local police stops are broadly similar for white and African American drivers. Still, there are some exceptions. Controlling for driving behavior, local police stop African American males, but not white males, at higher rates. The finding from Table 5.5—that miles driven increases white stops but not African American stops—is repeated here. It would seem that local police do tend to react to the race of drivers and that African American

male drivers are at particular risk of being stopped. White drivers increase their risk of a stop when they drive more, but African American drivers' risk of a stop by local police is increased by their race and being male. For both the African American and white populations, being older and driving a newer car reduces the probability of a stop by the local police. Table 5.8 reports our analyses of stops by the NCSHP. While there is significant racial disparity in stops by the NCSHP it is less than half as large as the disparity produced by local police stops. In fact, after controlling for demographic variables the DMV-weighted racial gap is not even statistically significant. The record check weighted racial gap in Model 2 is just barely significant and suggests that African American drivers have 1.38 times the white odds of being stopped. After introducing self-reported driving behavior in Model 3 the racial gap in NCSHP stops increases for both weighting schemes. These analyses suggest that there is some unexplained racial disparity and so there may be some racial bias in driver stops by the NCSHP, but the disparity is substantially smaller than the potential racial bias present in local police stops.

Table 5.8. Logistic Regression of **NCSHP Stop Last Year** Upon Race, Demographic Background, and Driving Behavior; multiplicative odds coefficient and significance level reported.

<i>Analyses weighted to 2000 DMV population count (N=2581)</i>	Model 1	Model 2	Model 3	African	
				American	White
Race (1=AA)	1.30*	1.21	1.53*	N=1,210	N=1,370
Gender (1 =Female)		.66*	.90	.75	1.09
Education		1.05	1.00	1.06	.95
Home Owner		.78	.78	.80	.71
Model Year of Car		.99	.98	.99	.96*
Age		.97***	.98***	.99	.98**
Rural		1.10	1.09	1.14	1.01
Scale Speeding 5+			1.23***	1.25***	1.20
Miles Driven Year (LN)			1.32***	1.19*	1.64***
Risky Driving Scale			1.17**	1.25**	1.08
Fewer Methods to Avoid Ticket Scale			.64**	.74	.57*
Interstate Frequency (low score=high usage)			1.06	1.03	1.12
Degrees of Freedom	1	7	12	11	11
Model Chi-Square	3.85	65.48	131.03	62.48	77.88
Model Probability	*	***	***	***	***

<i>Analyses weighted to record check estimate of non-response bias (N=2788)</i>	Model 1	Model 2	Model 3	African	
				American	White
Race (1=AA)	1.49***	1.38**	1.78***	N=1,215	N=1,374
Gender (1 =Female)		.70*	.92	.77	1.10
Education		1.05	1.00	1.06	.94
Home Owner		.78	.77	.79	.70
Model Year of Car		.99	.98	.99	.96*
Age		.97***	.98***	.99	.98***
Rural		1.10*	1.09	1.14	1.01
Scale Speed 5+			1.24***	1.25**	1.21*
Miles Driven Year (LN)			1.33***	1.21**	1.62***
Risky Driving Scale			1.17**	1.26***	1.06
Fewer Methods to Avoid Ticket Scale			.64**	.74	.57**
Interstate Frequency (low score=high usage)			1.07	1.04	1.13*
Degrees of Freedom	1	7	12	11	11
Model Chi-Square	11.91	84.27	173.60	83.03	96.05
Model Probability	***	***	***	***	***

Probability levels of * .05; ** .01; *** .001 or below

Supporting the notion that status bias processes are lower among the NCSHP, neither gender nor age of vehicle is significantly associated with being stopped in Model 3. Age continues to be associated with the likelihood of a stop but the coefficient is closer to one than in previous models. So, while the NCSHP, like local police forces, seems more likely to give older

drivers a break they do not do so as dramatically as local police do. Also consistent with a more professional orientation, stops by the NCSHP are very strongly related to all measures of driving behavior. People who report more miles driven, driving faster, risky driving behavior, and trying to avoid speeding tickets are all stopped at higher rates by the NCSHP.

When the models are split by race there are some suggestions that NCSHP troopers are not completely color blind. Being older tends to protect white drivers but not African American drivers from NCSHP stops. Similarly, owning a newer car seems to protect white drivers from stops but not African American drivers. On the other hand, risky driving behaviors such as passing, changing lanes without signaling, and driving without a seatbelt tend to encourage NCSHP stops of African American drivers but not of white drivers. These findings suggest that bias processes, to the extent they exist among NCSHP troopers, are subtle. White drivers are “protected” by their age and class, but African American drivers are not. Conversely, African American drivers are penalized for risky driving behaviors but white drivers are not. Another way to say this is that, when the NCSHP troopers react to the race of the driver, it tends to be in conjunction with other status or driving behavior characteristics. These interactions suggests a bias process, in which attributions about characteristics other than race are modified by the race of the driver.

As in the previous analysis miles driven is a risk factor for white drivers but not for African American drivers. White drivers who use the interstate less often than other white drivers are stopped more often by the NCSHP. White drivers who use fewer methods to avoid being stopped are stopped less often.

Discussion of Multivariate Results

These logistic regressions of stop experiences upon race, demographic background characteristics and driving behavior provide evidence that there may be some racial disparity in police stops of North Carolina drivers. For all stops, as well as for stops by local police and the NCSHP, African Americans are more likely than whites to be stopped in North Carolina. The degree of racial disparity, and so potential bias, in police stops appears to be substantially greater among local police than that within the NCSHP. Within the NCSHP the pattern of stop decisions associated with race suggests that while everyone is stopped for speeding, risky driving behavior can be an aggravating factor for African Americans, while class advantage—in the form of a new car or age—is a status shield for whites but not for African Americans.

Racial Differences in Stop Experiences

We also asked all drivers who reported a stop what reason they were given by the officer for the stop. We coded the reasons into “speeding,” “other moving violations,” “non-moving violations,” “general suspicion”-based stops, “no reason given by officer,” “do not recall,” and “other.” Speeding probably represents the least discretionary reason, since an officer must document the speed of the driver. “Other moving violations” (for example, rolling through a stop sign) and “non-moving violations” (for example, a broken taillight) represent clear violations of the driving laws but also enhanced police discretion in making a stop decision. “General suspicion” includes various stops to investigate the license, registration, vehicle, and driver. These could range from running a plate and discovering that the owner’s license had expired, to stopping a car on suspicion of carrying drugs. In all cases they require the officer to proactively investigate the driver or the car in the absence of a driving violation.

Table 5.9 displays the racial differences in reasons given for the stop. African Americans are somewhat (3 percent) more likely than whites to be stopped for other moving violations. They are also 4 percent more likely to be stopped for some form of general suspicion. They are less likely to be stopped for speeding. The table also suggests that African Americans are only slightly more likely to be informed by the officer that they were stopped for potentially discretionary reasons (53% versus 51% for other moving, non-moving, and general suspicion, combined). In the recent study by the National Institute of Justice on police-citizen contacts African Americans were 1 percent more likely than whites to be told they were stopped for generalized suspicion and 2 percent more likely for non-moving violations. Similar to the previous comparison of national figures to North Carolina ones, racial disparity in discretionary stops seems to be somewhat higher in North Carolina than in the nation overall.

	African American		White	
Speeding	217	0.42	159	0.47
Other Moving Violations	93	0.18	71	0.21
Non-Moving Violations	48	0.09	26	0.08
General Suspicion	136	0.26	74	0.22
No Reason Given by Officer	8	0.02	2	0.01
No Reason Given or Recalled by Respondent	12	0.02	4	0.01
Other	2	0.00	3	0.01
Total Stops	516	100%	339	100%
Chi-Square=69.63, 6df, p=.000				

We repeated the analyses of stops reported in Tables 5.7 (local police) and 5.8 (NCSHP) separately for speeding and other stops. We reasoned that, at least for the NCSHP, there is less discretion in speeding stops than in other types of stops. Among NCSHP stops, 61 percent were for speeding. Among local police stops, only 44 percent were for speeding. Although there still were significant racial disparities in stops, the racial gap in speeding stops by the NCSHP after

controlling for driver demographics and behavior was lower (odds of 1.44 with DMV weights) than the racial gap for non-speeding stops (odds of 1.54 with DMV weights). Among local police the racial coefficients were virtually identical for speeding and non-speeding stops. These analyses are consistent with the conclusion that racial disparity may be more widespread among local police than among the NCSHP and that the NCSHP speeding stops display a lower level of unexplained racial disparity.

Table 5.10 reports the outcome of police stops. The top panel of the table focuses on the distribution of citations, written warnings, and verbal warnings. There are no statistically significant racial differences in the distributions of citations, written warnings, and verbal warnings for all stops, local police stops, or NCSHP stops. Local police are more likely to give verbal warnings and NCSHP troopers are more likely to issue citations, but neither display significant racial differences in these outcomes.

We also asked those who had been stopped if they felt they had been treated with respect by the officer who stopped them. While most people felt they had been treated with respect, African Americans were 5.6 percent less likely to say they were treated with respect than were white drivers. The racial gaps in reported respect were nearly identical for local police and for the NCSHP. These later comparisons were not statistically significant, because of the small sample size of stops within each category of police. The general pattern is clear, however. Most drivers report respectful treatment, but African Americans are slightly more likely to report a lack of respect in the officer's treatment of them after a stop.

Table 5.10. Frequency Distribution of Stop Outcomes for First Stops, North Carolina Driver Survey						
	All First Reported Stops		Local Police Stops		NCSHP Stops	
	African American	White	African American	White	African American	White
<i>Did you get a traffic ticket, a warning ticket, or just a verbal warning?</i>						
Citations	52.0%	50.0%	48.1%	42.2%	59.3%	69.2%
Written Warnings	13.1%	18.1%	12.5%	15.6%	14.2%	21.4%
Verbal Warnings	35.0%	31.9%	39.4%	42.2%	26.5%	18.4%
Significant Racial Difference?	No		No		No	
<i>Do you think that you were treated by the officer with respect during this stop?</i>						
Treat With Respect	79.0%	84.6%	79.7%	84.1%	77.6%	85.5%
Significant Racial Difference?	Yes		No		No	
Total	329	238	216	135	113	103

Conclusions

According to the analysis of the survey data, African American drivers are significantly more likely than white drivers to have been stopped by the police in North Carolina. Even after controlling for other demographic statuses and driving behavior the odds of a stop by local police may be twice as high for African American as they are for white drivers. Local police are also significantly more likely to stop African American males relative to African American females, while among whites there is no gender disparity in stops after controlling for driving behavior.

The estimated racial disparity in stops by the NCSHP is much smaller, but still statistically significant after controls for driver characteristics and reported driving behavior. The NCSHP does not stop African American males at higher rates than African American females net of driving behavior. Among the NCSHP, race is linked to other attributes in the stop decision. Older whites and whites driving late-model cars are less likely to be stopped than are other whites. African Americans who report more risky driving behaviors are more likely to be

stopped. This suggests that the NCSHP troopers are reacting not simply to the race of the driver, but to the combination of race and other status attributes for whites and race and driving behavior for African Americans.

After the stop, differences in white and African American experience are less dramatic. African Americans are slightly more likely to have been informed that the stop was for a more discretionary reason. African Americans are also slightly more likely to report that they were treated disrespectfully after the stop. There are no racial differences in the distribution of citations, written warnings, and verbal warnings. Although there are some racial differences in experiences after the stop, they are small.

References for Chapter Five

Bradburn, Norman. 1983. "Response Effects." Pages 289-319 in Peter Rossi, James Wright, and Andy Anderson (eds.) *Handbook of Survey Research*. New York: Academic Press.

Chapter 6 Racial Differences in Trust in the Police

This chapter examines the processes that produce racial differences in trust in the police. There are substantial racial differences in trust in the police in general and of both local officers and the NCSHP more specifically. Race differences in trust in the police result not only from racial differences in stop experiences, but also from the reported stop experiences of friends and family. Disrespectful interactions between police and citizens can become affronts against the local community. In addition, African Americans are more likely to believe that the police target minorities and are less trustful of government in general. These cultural perceptions, which reflect both historical and personal knowledge of racial bias, are powerful sources of minority distrust in the police. To rebuild legitimacy in the minority community police forces would need not only to control racial bias in policing, but also to change the cultural distrust that arises from past experiences and group history.

Introduction

While racial disparity in police stops is clearly a problem in its own right, both the experience and perception of police bias may contribute to minority distrust of law enforcement. Police forces with no or little racial bias in their ranks might still be perceived as untrustworthy by minority citizens. Even majority citizens who believe that police discretion is sometimes linked to status characteristics, rather than to driving behavior, may have reduced trust in the police. The “driving while black” phenomenon has a powerful perceptual dimension, which can be expected to threaten police legitimacy independently of actual officer behavior. In this chapter

we examine citizens' level of trust in the police. As in the previous chapter, we focus on the police overall, local police, and the NCSHP (relatedly, Appendix F discusses some of the findings of our focus groups with citizens about various issues of racial profiling).

Surveys have a long history of use in measuring citizens' trust in the police and other government institutions. It is possible to use a survey to establish the degree to which the experiences of interaction with the police and the more generic belief in racial profiling might influence the legitimacy of the police force in its citizens' eyes. Trust in government institutions in general, and any specific police force, will be influenced by the history of relationships between groups and those institutions as well as more direct experiences and current events. Surveys are potentially useful for establishing the degree to which current police-citizen encounters, beliefs or exposure to current controversies such as "driving while black," or more deep seated dispositions are in play.

Background

In the focus groups that we conducted with African American drivers, it was clear that African Americans' perceptions of racial bias in policing were influenced not only by their own stop experiences, but also by the stop experiences of family and friends. In addition, the focus groups revealed that a generic perception of racial bias in policing – a perception we believe was produced by a culture in which many African American citizens expect racial bias by the police because they expect racial bias in many or all institutions in the United States. Focus group participants would also point toward accounts of racial profiling in New Jersey or the Rodney King beating as evidence that racism is widespread among police in general. This suggests to us that trust in the police is a function, not only of personal experiences with the police, but also the

experiences of family and friends and more general cultural perceptions of bias by the police and the integrity of institutions.

Previous research has demonstrated repeatedly that trust in the police is lower among minorities than among white citizens (Decker 1981; Flanagan and Vaughn 1996; Weitzer and Tuck 1999). This research, however, has rarely had direct measures of police encounters and has never included measures of police encounters in the respondent's network of family and friends.⁴³ In addition, previous research shows that white and minority citizens believe that racial targeting is widespread, but minorities are more likely to hold this belief (Gallup 1999). No previous study has examined perceptions of other forms of targeting by police. Some previous research has shown that well publicized controversial police incidents (for example, the Rodney King beating) tend to reduce trust in the police generally, particularly among minorities (Tuch and Weitzer, 1997; Lasley 1994). We reason that belief in racial profiling may have a similar effect. A recent study has shown that middle class minorities are more likely to distrust the police than lower class minorities (Weitzer and Tuck 1999). Finally, many studies have shown that minorities have lower levels of trust in government institutions than do white citizens (Feagin and Sikes 1994). We use a general trust scale in the analyses below to control for general cultural influences on trust in social institutions that may influence specific trust in police officers.

In this chapter we first describe racial differences in trust of the police, perceptions of police stop discretion tied to status characteristics, and more general trust in government institutions. We then model trust in the police as a function of race, stop experiences, the stop

⁴³ Warr and Ellison (2000) have recently pointed out that research on fear of crime has also neglected personal network influences on fear. They find that a prime source of fear of crime is fear for the safety of others.

experiences of friends and family, general trust in government institutions, and perceptions of both racial and other types of bias in policing.

Race Differences in Trust of the Police

The first panel of Table 6.1 reports racial differences in trust in the police. The first question was “On a scale from 1 to 5, where 1 is always fair and 5 is never fair, do you trust the police to treat you fairly?” In general, whites report trust levels that average close to 2—“sometimes fair.” African Americans are on average closer to 3—“neutral.” These differences are quite similar for all three items, although both African Americans and whites report slightly more trust in the NCSHP than in their local police forces. These results look quite similar to results reported by Weitzer and Tuck (1999). In the analyses that follow we examine the processes that generate distrust of the police in general using a Police Distrust Scale made up of the three items listed in Table 6.. We also analyze the processes separately for the items indicating distrust in the respondent’s local police and in the NCSHP.

Race Differences in Personal and Network Stop Experiences

The second panel of Table 6.1 shows that African Americans report significantly more stops per year of driving experience, more stops in the last year, and being treated with less respect during stops in the last year than do whites drivers.

We also asked respondents to report stops they had heard about that had been experienced by members of their household and by friends and acquaintances. We do not think that these responses represent an accurate count of the stop experiences of friends and family. Rather, we

see these as indicators of the respondent's stock of stories about police behavior in his or her immediate network. We do not know the racial composition of household members or friends and acquaintances, but assume that the networks of white respondents are predominantly white and the networks of African American respondents are primarily African American.

In the third panel of the table, we see that African American and white citizens report comparable numbers of stops of household members and of friends and acquaintances. Although few people report acts of disrespect by the police, African Americans report significantly higher levels of disrespect in police behavior during stops of both household members and of friends. In general the levels of reported disrespect are lower in these reports of network experience than they are in reports of self-experience. Thus, while African Americans hear more stories of disrespectful treatment than do whites, these stories are not widespread in either community.

These results suggest to us that the damage to police reputation that is produced by possible racial disparity in stop behavior is not necessarily limited to the person who is stopped, but to some extent becomes part of the community context in which citizens live. The results also suggest that the damage done by possible racial disparity in stops is likely to be magnified in the retelling by friends and family. Since the reported levels of disrespect in the experiences of friends and family are lower for African Americans than in their own reported stops, it may be the case that the consequences of racial disparity in police stops are somewhat attenuated in the retelling. On the other hand, individuals who have never been treated with disrespect by the police may hear stories about disrespectful behavior, which are then interpreted as evidence of racial bias in policing.

Distrust of Government and Belief in Profiling

The items that are used in the general trust in government scale are, again, coded from 1—“always fair,” to 5—“never fair.” For all of the items, except distrust of county commissioners, African Americans have significantly higher mean scores than whites, signifying higher levels of distrust in these institutions. Distrust of county commissioners is particularly high among all citizens, regardless of race. In general, distrust of the police is not markedly higher in the African American community than is distrust of other government officials. Among whites, however, distrust in the police is quite a bit lower than distrust of other government officials.

We also asked these citizens about their perceptions that the police profile drivers. We asked: Since many drivers speed or otherwise break the traffic laws, it is sometimes hard to tell why any one person gets pulled over by the police. Do you think that the following kinds of drivers are more likely to be pulled over by police than other drivers: young drivers? men? African Americans? Latinos? people driving run-down cars? people driving flashy cars?

Perceptions of racial disparity are widespread among African Americans. Eighty-one percent of African Americans believe that the police are more likely to pull over African American drivers. Just under 70 percent of African Americans perceive similar bias against Latino drivers. In contrast, less than a third of white drivers believe that there is racial bias against African Americans. Slightly more whites perceive bias against Latinos by the police. Looking at the racial bias scale (which sums the previous two items and divides by two), we see that whites are 40 percent less likely than African Americans to perceive racial bias in police stop decisions.

The last panel of Table 6.1 reports racial differences in other possible types of police profiling. In all cases, African Americans are more likely than whites to suspect bias in police

stops, although belief in the profiling of run down cars is not significantly different by race. The racial gap in beliefs in other forms of police bias averages 20 percent across the scale items, half as large as the racial gap in beliefs in racial bias in policing. One of the most interesting contrasts in the table is that a higher proportion (13.2 percent) of white respondents believe that the police target for non-racial reasons than believe that police are racially biased. Among African Americans the pattern is reversed, racial profiling is perceived as more extensive than other forms of profiling by 8.4 percent. The racial gap of the belief that police profile men is almost as large as the racial gap in the belief in racial profiling.⁴⁴

The racial comparisons in Table 6.1 suggest that African Americans' higher levels of distrust in the police could arise from racial differences in stop experiences and in the stop experiences discussed in their personal networks. It also seems plausible to expect that racial differences in perceptions of racial targeting and distrust in government may also contribute to the higher levels of African American distrust of the police. Whites report higher levels of belief in non-racial targeting by the police. It is not clear how this might influence racial differences in trust in police. The differences in white and African American responses to the two profiling scales suggests that splitting the models by race might be particularly revealing.

⁴⁴ In Chapter 5, we saw no gender differences in stops by the NCSHP after controlling for driving behavior. This was also the case among whites stopped by the local police. Among African American stops by the local police, males were at significantly higher risk than females, even after controlling for driving behavior. The higher perception of male profiling among African Americans than whites may be produced by this pattern of local police stops. It, of course, could also be produced by the general perception in the African American community that African American men are particularly vulnerable in contemporary American society.

Modeling Distrust of the Police in a Multivariate Context

We model the process that gives rise to distrust in the police in a multivariate statistical context. We use two types of statistical models—ordinary least squares and ordered logistic regression. OLS models are appropriate for continuous dependent variables. The general distrust in police scale clearly meets the requirement of a continuous variable as does the general trust in government scale. The analyses of trust in the local police and the NCSHP are five category

	African American	White	Significant Difference?
Distrust of Police in General	2.63	1.95	Yes
Distrust of Local Police	2.69	2.04	Yes
Distrust of NCSHP	2.53	1.89	Yes
Police Distrust Scale (Alpha=.809)	2.63	1.96	Yes
Lifetime Stops/Years of Driving	.28	.24	Yes
Number of Stops in Last Year	.44	.25	Yes
Treated with Disrespect During Stop Last Year	6.7%	3.0%	Yes
Number of Household Stops in Last Year	.25	.21	No
Household Members Report Disrespect in Stops	2.5%	1.6%	Yes
Number of Friends Stops in Last Year	.76	.85	No
Friends Report Disrespect in Stops	5.7%	1.8%	Yes
Distrust in Teachers	2.29	2.03	Yes
Distrust in County Commissioners	2.81	2.77	No
Distrust in Judges	2.62	2.24	Yes
Distrust in Congress	2.68	2.53	Yes
General Distrust of Government Scale (Alpha=.746)	2.61	2.40	Yes
African Americans are Profiled	80.8%	32.7%	Yes
Latinos are profiled	69.3%	36.8%	Yes
Belief in Racial Profiling Scale (Alpha=.815)	75.8%	35.5%	Yes
Run down cars are profiled	43.7%	41.2%	No
Flashy cars are profiled	78.3%	60.6%	Yes
Men are Profiled	66.3%	29.3%	Yes
Young are Profiled	81.9%	66.9%	Yes
Belief in Other Forms of Profiling Scale (Alpha=.458)	67.4%	48.7%	Yes

ordinal scales. We use ordered logistic regression for these two items, as well as for our analyses of racial and general profiling beliefs. In both types of models a positive coefficient indicates that increases in the explanatory variable (such as a belief in racial profiling) leads to an increase in distrust of the police. A negative coefficient indicates that an increase in the explanatory variable

leads to increased trust in the police (for example, age). For all explanatory variables mean values are imputed for missing values. We include indicator variables for mean substitution in regression equations but do not report their values in the tables. These models are weighted to correspond to the 2000 N.C. Department of Motor Vehicles age-gender distributions within race.⁴⁵

The logic of the analyses is to first establish the size of the racial gap in trust of the police, net of the set of driver demographic and behavior variables employed in the previous chapter (racial differences in these variables were discussed in the previous chapter and reported in Table 4 of that chapter). The demographic variables (gender, education, home ownership, car age, individual age, and rural residence) allow us to statistically adjust racial differences in trust of the police for class, gender, and regional influences on distrust in the police that may be correlated with race. They also are indicators in their own right of other aspects of a citizen's identity that may influence distrust of the police. Driving behaviors (speeding, miles driven, risky driving, methods to avoid tickets, and interstate frequency) are included in these initial models to adjust for risk behaviors that may influence trust in the police. We reason that people who break the driving laws may be less trustful of police because they fear police stops.

The second model in the following analyses introduces the respondent's reports of his or her and his or her friends' and family members' stop experiences regarding distrust in the police. We measure the number of stops per driving year and in the most recent year for respondents, but only stops in the last year for their family and friends. We also include three measures of disrespectful treatment during stops corresponding to personal, family, and friends' stops. If

⁴⁵ The non-response weights used in the last chapter are not appropriate for these analyses. They are appropriate only for weighting reports of stops.

coefficients in Model 1 are reduced in magnitude in Model 2 this indicates that the Model 1 estimates partly reflect and are caused by differences in stop experiences.

The third model for our analyses of trust in the police introduces the measure of trust in general government institutions as well as beliefs in racial and non-racial targeting by the police. If coefficients in Model 2 are reduced in magnitude in Model 3, this indicates that the Model 2 estimates partly reflect and are caused by differences in general trust and beliefs in police bias. Finally, Model 3 is estimated separately for African American and white citizens to see if the processes that lead to distrust in the police are the same for African American and white citizens.

Because general distrust of government institutions and belief in racial and other forms of police profiling are such strong influences on distrust of the police, we also model them as outcomes of demographic and driving variables. We begin with an analysis of racial differences in trust of government officials other than the police and belief in racial and non-racial profiling.

Distrust of Government Officials Other than the Police

The variable indicating distrust in government officials is an additive scale based on level of distrust in teachers, county commissioners, judges and congressional representatives. In the analyses of distrust of the police that follows we introduce this variable to account for differences in generic trust in government that might also spill over into distrust of the police in particular. We first regress the distrust in government scale upon the characteristics of drivers and stop experiences to see if these characteristics influence generic distrust in government.

Table 6.2 reports the models of generic government distrust. African Americans have significantly higher distrust of government officials. The racial gap of .25 is slightly higher than the racial gap reported in Table 6.1. In addition, female and young African American drivers

distrust government officials at higher rates (Model 3). African American and white risky drivers distrust government officials at higher rates.

Being personally treated with disrespect by police officers during a stop in the past year strongly increases generic distrust of government officials. When the models are split by race, we find that both African Americans' and whites' trust in government is undermined by disrespectful treatment by the police during a stop. In addition, African Americans who hear of disrespectful police behavior toward friends and acquaintances trust government officials less.

In general, the table suggests that there are relatively large racial gaps in trust of government institutions that are not a function of other driver statuses or of driving behavior. Rather, distrust of government in general is increased when police officers interact with citizens in ways that are interpreted as disrespectful. Hearing stories of disrespectful police behavior from friends and acquaintances increases distrust of government for African Americans.

Belief in Racial Profiling

The variable indicating belief in racial profiling is an additive scale based on belief in racial profiling of African American and Latino drivers. Models are estimated using ordinal logistic regression because this variable only has three categories. The first model of Table 6.3

Table 6.2 Regression of Trust in Government Scale on Race, Driver Characteristics, and Stop Experiences, N=2,284

	Model 1	Model 2	Model 3	Model 4
Race (1=African American)	.249***		African American	White
Gender (1=Female)	.072*	.093*	.215***	-.003
Education	-.003	.001	.012	-.011
Home Owner	-.007	-.013	.020	-.063
Model Year of Car	-.002	.000	-.000	-.001
Age	-.004**	-.000	-.006**	.000
Rural	-.017	-.015	-.022	-.011
Scale Speeding 5+	.012	.011	.004	.023
Miles Driven Last Year (LN)	.039**	.032*	.023	.051*
Risky Driving Scale	.068***	.065***	.070**	.054**
Fewer Methods to Avoid Ticket Scale	-.027	-.024	-.048	.005
Interstate Frequency (low score=high usage)	-.005	-.005	.008	-.017
Number of Stops per Driver Year		.021	.040	.018
Number of Stops in Last Year		.015	.007	.026
Treated with Disrespect During Stop Last Year		.455***	.482***	.427**
Number of Family Stops in Last Year		.014	.028	.005
Family Members Treated with Disrespect		.142	.155	.096
Number of Friend Stops in Last Year		.006	-.012	.019
Friends Treated with Disrespect		.176	.268*	-.003
Adjusted R ²	.052	.075	.090	.051

* .05; ** .01; *** .001

regresses belief in racial profiling by the police upon the respondent's race and other driver characteristics. African Americans are much more likely than whites to believe that the police profile on the basis of race even after controls for other status characteristics and driving behaviors. The much higher African American belief in racial profiling is only minimally a function of class, gender, rurality, or reported driving behavior differences between African Americans and whites.

Higher education is associated with higher belief that the police profile on the basis of race among both African American and white citizens, although the effect is much stronger

among African Americans. Urban residents of both races are significantly more likely than rural residents to believe in racial profiling. Whites who report typically speeding more than five miles an hour above the speed limit are more likely to believe in racial profiling than more law abiding white drivers. African Americans who actively try to avoid tickets more are less likely than other African Americans to believe in racial profiling by the police.

Introducing the measures of stop experiences provides some interesting results. Both African American and white drivers who were treated with disrespect during a police stop are more likely to believe the police profile on the basis of race. In addition, African Americans whose family members experience more stops and whose friends report being treated with disrespect are more likely than other African Americans to believe that the police profile on the basis of race. Thus for African Americans, but not whites, belief that the police profile on the basis of race is related to the recent experiences of members of their community with the police. An inspection of the R^2 across models makes clear that race is by far the strongest predictor of belief in racial profiling.

Belief in Other Forms of Profiling

The variable indicating belief in other forms of profiling is an additive scale based on belief in police profiling of young and male drivers as well as profiling of run-down vehicles and flashy cars. We use ordinal logistic regression to estimate the models presented in Table 6.4 because belief in other forms of profiling is a five category ordinal scale. Controlling for

Table 6.3 Ordinal Logistic Regression of Belief in Racial Profiling on Race, Driver Characteristics, and Stop Experiences (N=2,830)

	Model 1	Model 2	Model 3 African American	Model 4 White
Race (1=African American)	1.83***	1.789***		
Gender (1=Female)	-.052	-.022	.023	.086
Education	.112***	.119***	.164***	.062**
Home Owner	.001	-.013	.004	.075
Model Year of Car	-.016	-.012	.009	.013
Age	-.002	.002	.005	.001
Rural	-.121***	-.121***	.009	-.150***
Scale Speeding 5+	.113**	.095*	.024	.128*
Miles Driven Last Year (LN)	-.022	-.035	.006	.097
Risky Driving Scale	.065	.006	.078	.066
Fewer Methods to Avoid Ticket Scale	-.171	-.184	-.447**	.007
Interstate Frequency (low score=high usage)	.004	-.005	.004	.000
Number of Stops per Driver Year		.097	.151	.084
Number of Stops in Last Year		-.039	.061	.002
Treated with Disrespect During Stop Last Year		.744**	.850*	.722*
Number of Family Stops in Last Year		.160*	.129	.151
Family Members Treated with Disrespect		.457	.588	.297
Number of Friend Stops in Last Year		.024	.052	.063
Friends Treated with Disrespect		.791**	1.04**	.320
Pseudo R ²	.241	.257	.096	.064

* .05; ** .01; *** .001

demographic and driving characteristics, African Americans are more likely to believe in these non-racial forms of profiling as well. African American women are less likely than African American men to believe that the police profile on these other driver characteristics. African American homeowners are less likely to believe in these forms of profiling, while older African Americans are more likely to think that profiling is about characteristics other than race. Whites who report typically speeding while driving are more likely to report that police profile on non-racial dimensions.

The only statistically significant stop experience reported in the model for whites indicates that respondents whose family members were stopped more in the last year are more likely than other whites to believe that the police profile on these non-racial characteristics. This latter finding is particularly interesting in light of the previous analysis. African Americans' belief in

	Model 1	Model 2	Model 3 African American	Model 4 White
Race (1=African American)	1.16***	1.126***		
Gender (1=Female)	-.228**	-.202*	-.409***	-.078
Education	-.035	-.032	-.018	-.041
Home Owner	-.163	-.175	-.248*	-.149
Model Year of Car	-.009	-.007	.002	-.011
Age	-.001	.001	.014**	-.008
Rural	-.049	-.005	-.056	-.052
Scale Speeding 5+	.094*	.080*	.000	.134*
Miles Driven Last Year (LN)	-.030	-.044	-.048	-.040
Risky Driving Scale	.027	.016	.028	-.028
Fewer Methods to Avoid Ticket Scale	-.062	-.045	-.004	-.090
Interstate Frequency (low score=high usage)	-.002	.003	-.016	-.056
Number of Stops per Driver Year				
		.131	.105	.147
Number of Stops in Last Year		.021	.049	.051
Treated with Disrespect During Stop Last Year		.133	-.094	.624
Number of Family Stops in Last Year		.116	-.010	.261**
Family Members Treated with Disrespect		.029	-.001	.107
Number of Friend Stops in Last Year		.037	.101*	-.018
Friends Treated with Disrespect		.285	.409	-.294
Adjusted R ²	.126	.134	.050	.053

* .05; **.01; ***.001

racial profiling is increased dramatically by the experiences of their friends and family. While not as dramatic, white belief in other forms of profiling is enhanced by family stop experiences.

General Distrust of the Police

Table 6.5 reports the multivariate analysis of the general distrust in police scale. This scale was created by summing the three police trust items described in Table 6.1 and dividing by three to preserve the original metric. We estimate the models using ordinary least squares regression as the untransformed scale varies from 3 to 15. Model 1 shows that African Americans are .63 units more distrustful of police than are whites, even after controlling for other demographic statuses and driving behaviors. This result is very similar to the bivariate relationship reported in Table 6.1, and it indicates that African Americans are significantly more likely to distrust the police than are whites. This result holds up across all three models, although the total racial differences in distrust declines in Models 2 and 3. On balance, this suggests that the racial gap in trust in the police is partly created by police encounters with the respondent and his or her family and friends, and partly by the more generalized beliefs in police profiling and distrust of government examined in the previous two tables.

There are three additional statistically significant effects in Model 1. Older drivers show lower distrust of the police. It is also the case that faster drivers and those reporting more risky behaviors are more distrustful of the police. As we expected, drivers who break the law routinely are less trustful of police than their more law abiding counterparts, suggesting perhaps an understanding of their increased vulnerability. For speeding, this effect is limited to whites.

Model 2 introduces the indicators of personal and network stop experiences. The racial gap in stops declines slightly relative to Model 1, suggesting that personal and network stop experiences produce about 8 percent ($.58/.63$) of the heightened minority distrust of the police. Stop experiences also reduce to non-significance the effects of speeding behavior on police distrust. Speeding increases the probability of a stop, which in turn influences distrust in the

police. In the split models, however, whites who speed remain particularly suspicious of the police.

The more lifetime stops per year of driving, the less the trust in the police, but this effect is particularly present among white drivers. Stops in the past year, either of the respondent or of his or her family and friends, have no effect on trust in the police—with one exception. Surprisingly, whites who have been personally stopped in the last year show increased trust in the police. Being treated with disrespect by an officer during a stop in the last year, however, dramatically increases distrust of the police for both African Americans and whites. Similarly, and only somewhat less dramatically, disrespectful treatment during stops to family or friends increases the level of distrust in the police.

One way to interpret these results is that it takes multiple stops over a lifetime to damage the legitimacy of the police, but it takes only a single act of disrespect during a citizen encounter to lower citizen trust. Acts of disrespect are likely to be shared with family and friends and also to lower trust in the police across the whole acquaintance network of the motorist who has been treated poorly.

Model 3 introduces the measures of trust in other government institutions and the two measures of perceptions of police targeting of drivers. The racial gap in trust is still statistically significant but drops to .35 in this model. Thirty-nine percent of the observed racial gap in trust in the police is directly tied to these three measures. Perceptions of racial and non-racial targeting by the police, as well as historically produced distrust of government institutions are powerful sources of distrust of the police. The effect of general distrust of government officials is three times larger than the effects of belief in either profiling scale. Ending racial disparity in stops,

even ending perceptions of racial bias, can only be a first step in building community trust in the police.

The risky driving scale is no longer significant. As we saw in Table 6.2, risky drivers are less trustful of government in general. The effects of the three indicators of police disrespect are all reduced in Model 3. Again, as we saw in Tables 6.2 and 6.3, disrespectful treatment by the police increases distrust of government in general and the belief that police are targeting minority drivers.

Models 4 and 5 split the sample by race and estimate Model 3 again. The processes which generated distrust in the police are not dramatically different by race. As noted previously, even after controlling for general trust in government, whites who routinely speed are less trustful of the police. African American drivers who are more active in scanning the road for speed traps are less trusting in the police than other African Americans.

Disrespectful treatment of drivers, or their friends and family, increases distrust of the police among both African Americans and whites. Belief systems influence distrust in the police similarly for African American and white citizens. Among African Americans distrust of the police is strongly influenced by their general distrust of government officials and belief in racial profiling, but not by their belief in non-racial police profiling. Whites' distrust of the police, on the other hand, is also influenced by belief in non-racial profiling.

Table 6.5. Ordinary Least Squares Regression of Distrust in Police Scale on Race, Driver Characteristics, Stop Experiences, Trust in Institutions, and Perceptions of Police Bias

	Model 1	Model 2	Model 3	Model 4 African American	Model 5 White
Race (1=African American)	.632***	.580***	.354***		
Gender (1=Female)	-.007	.043	.006	.013	-.012
Education	-.003	.046	-.001	.010	-.011
Home Owner	-.038	-.046	-.034	-.049	-.018
Model Year of Car	-.007	-.003	-.024	.000	-.004
Age	-.009***	-.006***	-.005***	-.005**	-.004**
Rural	-.018	-.013	.001	.020	-.015
Scale Speeding 5+	.048**	.031	.020	-.008	.041*
Miles Driven Last Year (LN)	.002	-.011	-.021*	-.025	-.004
Risky Driving Scale	.063***	.057***	.021	.022	.022
Fewer Methods to Avoid Ticket Scale	-.046				
		-.061	-.039	-.110*	.031
Interstate Frequency (low score=high usage)	.005	.003	.007	.011	.002
Number of Stops per Driver per Year		.169***	.137***	.051	.206***
Number of Stops in Last Year		-.036	-.040*	.005	-.112***
Treated with Disrespect During Stop Last Year		.718***	.463***	.355***	.658**
Number of Family Stops in Last Year		.016	-.007	-.027	.016
Family Members Treated with Disrespect		.550***	.456***	.415**	.565***
Number of Friend Stops in Last Year		-.007	-.014	.000	-.019
Friends Treated with Disrespect		.572***	.450***	.272***	.691***
Generalized Distrust of Government			.617***	.676***	.553***
Belief in Racial Profiling Scale			.210***	.231***	.181***
Belief in Other Forms of Profiling Scale			.174**	.133	.217**
Adjusted R ²	.177	.231	.479	.452	.380

* .05; ** .01; *** .001

Distrust of the Local Police

Table 6.6 repeats the analyses presented in Table 6.5, but the trust refers specifically to the respondent's local police force, rather than police in general. The results in Model 1 are nearly identical to those in the previous table. African Americans, the young, risky drivers, and self-

reported speeders are all more distrustful of the police as in the previous table. Adding stop experiences in Model 2 also produces nearly identical results to those in the previous table. More lifetime stops increase distrust of the local police, as do disrespectful interactions with the police, either with the respondent or with his or her family and friends. Again the effect of speeding behavior on distrust is mediated by stop experiences. Model 3 introduces the measure of general distrust and the two measures of belief in police profiling. Again these are powerful predictors of distrust in the local police. They also mediate a large share of the racial coefficient, suggesting that these more general cultural perceptions which are strongly linked to race are powerful sources of minority distrust in their local police. Again, being a risky driver is no longer a significant predictor, suggesting that they tend to distrust government in general (refer back to Table 6.2).

Models 4 and 5 compare the processes generating distrust of the local police for African American and white drivers. The coefficients for demographic and driving behavior variables are quite similar with one exception. Risky white drivers are particularly distrustful of the police, even net of controls for general distrust of government. More dramatically, stop experiences produce much larger coefficients for whites than for African Americans. While both African American and white drivers' trust in their local police is diminished by disrespectful treatment, this is much more so the case for white drivers. Similarly, lifetime stops per year reduces white, but not African American, trust in the police significantly and dramatically. Finally, both African American and white distrust of local police is tied to their distrust of government in general, but belief in racial profiling is a more important source of distrust among African American drivers, while belief in other forms of profiling by the police undermines white trust.

Table 6.6 Ordinal Logistic Regression of Distrust of Local Police on Race, Driver Characteristics, Stop Experiences, Trust in Institutions, and Perceptions of Police Bias (N=2,830)					
	Model 1	Model 2	Model 3	Model 4	Model 5
Race (1=African American)	1.11***	1.036**	.732***	African American	White
Gender (1=Female)	.036	.117	.064	.052	.048
Education	-.012	-.024	-.008	.006	-.020
Home Owner	-.132	-.151	-.135	-.197	-.039
Model Year of Car	-.009	-.003	-.001	.002	-.004
Age	-.018***	-.013***	-.012***	-.014***	-.009*
Rural	-.021	-.014	.003	.027	-.025
Scale Speeding 5+	.099**	.073	.058	.024	.077
Miles Driven Last Year (LN)	.053	.029	.002	-.006	.035
Risky Driving Scale	.152***	.143***	.071*	.027	.130**
Fewer Methods to Avoid Ticket Scale	-.067	-.071	-.031	-.226	.149
Interstate Frequency (low score=high usage)	.015	.015	.021	.011	.030
Number of Stops per Driver per Year		.329***	.361***	.207	.528***
Number of Stops in Last Year		-.041	-.060	.007	-.270**
Treated with Disrespect During Stop Last Year		1.221***	.819***	.659**	1.095***
Number of Family Stops in Last Year		-.016	-.031	-.060	.018
Family Members Treated with Disrespect		1.006***	1.099***	1.002**	1.288***
Number of Friend Stops in Last Year		-.003	-.019	.007	-.018
Friends Treated with Disrespect		1.103***	.960***	.627*	1.810***
Generalized Distrust of Government			1.671***	1.762***	1.620***
Belief in Racial Profiling Scale			.373***	.413*	.302*
Belief in Other Forms of Profiling Scale			.359*	.096	.673**
Pseudo R ²	.149	.202	.455	.453	.382
* .05; ** .01; *** .001					

Distrust of the North Carolina State Highway Patrol

Table 6.7 repeats the previous analyses, but now trust refers specifically to distrust of the NCSHP. Models 1 through 3 reprise the results we have already encountered in the general distrust and local police analyses. Distrust of the NCSHP seems to be governed by the same

generic process that leads to distrust in the police in general. The one interesting new finding is that African Americans who drive more miles per year tend to have increased trust in the NCSHP. Models 4 and 5 reveal some interesting differences in the process that generates distrust in the NCSHP between African Americans and white drivers. African American trust in the NCSHP is not influenced by lifetime stops or personally being treated with disrespect during a recent stop. Both of these experiences increase white distrust of the NCSHP. Similarly, belief in racial and non-racial profiling increase white distrust of the NCSHP but not African Americans' distrust. These racial differences in the processes that generate distrust of the NCSHP suggest that African American's trust in the NCSHP is less vulnerable to either personal experience or the current controversy around police profiling. This is certainly consistent with the findings in the last chapter that racial disparity in stops by the NCSHP is much lower, and a much more subtle process, than racial disparity in local police stops. These findings are also consistent with our focus groups with white and African American drivers. In those focus groups white drivers did not clearly differentiate between different types of police. African American drivers, on the other hand, were quite clear that they had considerably more trust in NCSHP troopers to act professionally than they did of their local police.

Conclusions

Distrust in the police is produced by a combination of negative personal experiences with the police, negative experiences of family and friends, belief in police profiling on both racial and non-racial grounds, general distrust of government institutions, and being a minority.

Table 6.7 Ordinal Logistic Regression of Distrust of the NCSHP on Race, Driver Characteristics, Stop Experiences, Trust in Institutions, and Perceptions of Police Bias (N=2,830)					
	Model 1	Model 2	Model 3	Model 4	Model 5
Race (1=African American)	1.117***	1.071***	.764***	African American	White
Gender (1=Female)	.082	.150	.088	.138	.017
Education	.026	.040*	.043*	.067*	.013
Home Owner	.025	-.007	.019	-.050	.120
Model Year of Car	-.009	-.005	-.003	-.001	-.002
Age	-.020***	-.015***	-.017***	-.016***	-.018***
Rural	-.012	-.001	.009	.081	-.060
Scale Speeding 5+	.132**	.115**	.094*	.041	.146*
Miles Driven Last Year (LN)	-.011	-.031	-.098**	-.081*	-.066
Risky Driving Scale	.090**	.081**	.015	-.015	.045
Fewer Methods to Avoid Ticket Scale	-.041	-.057	-.008	-.130	.076
Interstate Frequency (low score=high usage)	-.001	-.003	.002	.005	-.009
Number of Stops per Driver Year		.284**	.271**	.083	.419***
Number of Stops in Last Year		-.065	-.086	.017	-.284**
Treated with Disrespect During Stop Last Year		.977***	.551**	.200	1.221***
Number of Family Stops in Last Year		.066	.012	.031	-.012
Family Members Treated with Disrespect		1.013***	.915***	.803*	1.162**
Number of Friend Stops in Last Year		-.013	-.036	-.030	-.030
Friends Treated with Disrespect		.815***	.682***	.636**	.899*
Generalized Distrust of Government			1.568***	1.667***	1.511***
Belief in Racial Profiling Scale			.307**	.225	.357**
Belief in Other Forms of Profiling Scale			.456**	.180	.698**
Pseudo R ²	.152	.187	.421	.404	.346
* .05; ** .01; *** .001					

The related problems of racial profiling and trust in the police are not simple ones. African Americans distrust the police, especially local police, because of their personal experiences and more general cultural orientations. Disrespectful interactions are particularly powerful sources of both distrust in the police and belief in racial profiling. This is not, however, simply a perception produced by direct experience. On the contrary, negative encounters with the police by family and friends generate distrust and increase the belief in racial profiling. In fact, among African Americans, disrespectful police treatment or stories of disrespectful police treatment can even undermine trust in government institutions in general. Belief in racial profiling undermines trust in the police even among whites.

African Americans are more forgiving of the NCSHP than are whites. African Americans are more likely to translate negative experiences into distrust of local police forces than the NCSHP. This may reflect their observations of lower bias or more professional carriage by NCSHP troopers. Whites, on the other hand, are less discriminating. Any perception of disrespect or profiling undermines white trust in all types of police. Whites are particularly influenced by perceptions of non-racial profiling, assumedly because these are the types of profiling for which they would be most at risk. Thus while African Americans are more distrustful of the police in general than are white citizens, white's trust in the police seems more vulnerable to recent experiences and media portrayals.

Citizen trust in police is also influenced by more general dispositions toward trust in government. This is true for white and African American citizens and for all types of police examined. This suggests that the legitimacy of the police in general, and of specific police forces,

is a nested problem. Police legitimacy is undermined by disrespectful treatment (especially among whites), and belief in racial profiling (especially among African Americans), and belief in other forms of profiling (especially among whites). Where racial disparity in treatment is lower, as in the NCSHP versus local police, African Americans do not translate negative experience into reduced trust. Police legitimacy is more vulnerable among whites. African Americans, however, have a lower level of trust in the police of all types stemming from their past experiences in, and cultural understanding of, American society. Some of this can be seen in African Americans' lower trust in government institutions in general, but most seems to be focused on a specific fear of the police. Among whites distrust of the police is more strongly tied to distrust of government institutions in general.

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Chapter 7 Discussion and Conclusions

When this project began, we had two goals in mind: to determine if NCSHP troopers engaged in “racial profiling” in making their routine traffic stops and to determine what citizens thought about racial profiling (perceptions of bias prevalence, attitudes toward the NCSHP, degree of distrust of police, and so forth). As we began collecting and analyzing the data, we realized that these general goals could be broken down into several sub-goals: 1) to determine if there were disparities in the stops and citations of African Americans after controlling statistically for the deployment of troopers (by place and time); 2) to determine if the troopers specializing in conducting searches for contraband (the CIT) stopped and conducted searches based on the race of the person stopped; 3) to develop methods to provide for information as to districts and troopers with racially disparate intervention rates (to be used in conjunction with other indicators of bias, such as citizen and/or trooper complaints); 4) to determine the extent to which surveys of the general public regarding traffic violations and stops can help shed light on the processes involved; 5) to assess the attitudes of the general public in their perception of racial bias and profiling and to see if the authority of the NCSHP was being undermined by the perceptions of widespread racial disparity in stops, citations and searches; and 6) to assess the points of view of the NCSHP troopers themselves as they discussed possible racial bias and profiling. We will discuss each of these goals in turn.

Official Record Evidence of Disparity

In assessing the prevalence of racial disparity, it is necessary to differentiate between the usual statistical information available to state police organizations (essentially statewide totals of

the number and percentage of interventions involving African Americans) and information based on, and required for, a more detailed analysis involving appropriate statistical control variables that account for the deployment of troopers relative to the racial demographic characteristics of the drivers on the highway. Data on stops, citations, and written warnings that allow for a simple statistical summary for the state as a whole show evidence of racial disparity against African Americans. For example, even though African Americans make up approximately 20 percent of the drivers in the state, they constitute almost 25 percent of the drivers cited for speeding. In general, we find overall levels of disparity of the magnitude of 5 to 10 percent in absolute terms, and up to 50 percent in relative terms (for example, a 20 percent African American driver baseline and a 30 percent rate of stops/citations of African Americans for license, registration, or insurance violations represent a 10 percent absolute difference or a 50 percent relative difference—10% absolute difference/20% African American baseline. It should be noted that we do not find evidence of larger disparity, such as that suggested by Lamberth (2001), nor do we find that the disparity increases when we control for the measures of the deployment of troopers.

Throughout the analysis of the official record data, we are limited by the fact that we have baselines that only approximate the actual volume of drivers on the highways (so called “proxy measures”) -- and those drivers are assumed to be “available” for a stop -- against which to measure the racial composition of the drivers stopped and cited by the NCSHP. That is, we do not have direct measures of the racial composition of highway drivers, and thus cannot make strong claims as to whether or not the racial disparity that we observe is due to racial bias or to deployment. (By deployment we are referring to the assignment of patrol cars to areas of highways by time of day). For example, all else being equal, the more patrolling of areas with more African American drivers, the greater the disparity in the racial composition of those

stopped, cited or warned. Deployment by time of day is also relevant since we find strong evidence that the proportion of drivers who are African American varies by time of day (with proportionately more African Americans on the highway at night).

Analyses of racial profiling are further complicated by researchers' inability to directly and precisely measure what troopers actually observe and react to as they patrol the highways. That is, while driver/vehicle behavior would likely be a crucial piece of information to use in accounting for the observed levels of disparity, such information on the actual racial distributions across different infractions are generally unknown. We were able to obtain a first measure of driver behavior but it was limited to fourteen highway segments (as discussed in Appendix A). For those limited geographic areas we found that there were differences in the vehicular speeding behavior of whites and African Americans, with more speeding above what we call "local speeding thresholds" on the part of African Americans. We do not generalize our findings from the fourteen sites to other areas, because we cannot rule out the likelihood that there are racial differences in driving behavior across locales and conditions. Our opinion, as informed by what little evidence is available on this topic, is that there are many factors involved in the determination of the racial composition of drivers involved in traffic violations in an area. Further data on such behavior are necessary for researchers to be able to make truth claims in this regard.

Besides not having direct measures of violating behavior, we as researchers do not have direct measures of the deployment of troopers. Instead, we can only assemble a "paper trail" of stops, citations, and written warnings issued by troopers. Thus, we do not know how many hours of patrolling occur on a given highway, nor at what precise times. In controlling statistically for deployment we can only compare citation rates of African Americans to our best available

baseline measure at the smallest unit of analysis feasible to study. Here we argue that the data on accidents provide a baseline measure at relatively small units of analysis (as small as what we call the “county highway area” or the stretch of a highway within about a quarter of a North Carolina county). We argue that accident data at the level of the county highway area allow us to take a look into the problem of the mismatch of where NCSHP troopers look for violators and where violations occur (the so-called “spatial heterogeneity” problem). Yet, our measures will involve an imprecise match not only because the racial composition of drivers in accidents is not necessarily an adequate substitute for the behavior of drivers violating the traffic laws, but because there still could be a substantial mismatch between where the patrols occur and where the citizens violate the traffic laws. For example, there could be variation within a given highway segment (such as what we call a county highway area) in the proportion of violating drivers who are African American because of the intersecting highways which take drivers from, and supply drivers to, specific segments of the highway. Thus, we may find that on a highway within a county, such as U.S. 64 in Nash County, that 25 percent of the drivers violating the law are African American in one segment, and 20 percent in another because the intervening intersection takes/provides African Americans to/from a town where relatively many African Americans live. The limited data we have on such variation within highways indicates that 5 percent changes in the African American composition are common.

An examination of the stops of drivers by the NCSHP on highways where there are mileposts indicates even more variation in the patrolling patterns of the NCSHP by mile of highway. Adjacent miles of a given highway can differ by a substantial degree in the number of stops that occur, and such variation cannot reasonably be attributed only to variation in driving behavior. The NCSHP may stop drivers more in one segment of a highway than another due to a

variety of very local considerations, such as turn-around points to the opposite direction, wide shoulders to pull over vehicles safely, low density of traffic (stopping vehicles in high density traffic may cause accidents), and so forth. If the choice of such high volume stop zones happens to coincide with the disproportionate presence of African American drivers, the disparity rate for an area may rise accordingly.

With all of these considerations possibly at work—and not measured in the data available to us—one can neither rule out the possibility that racial bias explains some of the variation in racial disparity, nor assert that it is unequivocally present in any geographic area or in the workings of any specific trooper. Put simply, the current level of science is inadequate to the determination of whether disparity can be explained by bias or by one or the other of the rival hypotheses just discussed.

At the same time the empirical evidence from the analysis of accident and citation data at the district level (in which data have been aggregated from the county highway area) suggests that there are some districts where there are relatively high levels of racial disparity that cannot be accounted for by the deployment of troopers (within the limits of the analysis as discussed above). Yet, it must be mentioned that there are even more districts where there is under-representation of African Americans in citations: there are fewer citations of African Americans relative to involvement in accidents. It could be that the districts with high rates of disparity in this analysis are units where there is racial bias, or it could be that there are uncontrolled variables that account for the disparity. Given the data and analysis limitations discussed above, we cannot say with certainty.

The analysis in Chapter 3 at the individual trooper level also leads us to a conclusion of ambiguity regarding whether or not there are troopers who have unduly high rates of citations of

African Americans. While we have developed mathematical models that explain about two-thirds of the variance in the number of African Americans cited, it is not possible to ascertain what proportion of the remaining variance can be explained by bias or by factors not included in the model.

Criminal Interdiction Team

As for the results of our analysis of the searches conducted by the Criminal Interdiction Team (CIT) and by regular NCSHP troopers, we were initially surprised to find that compared to the very large volume of drivers confronted by the NCSHP every year (upward to a million), there were about a thousand probable cause and consent searches a year in 1997, dropping to about 500 in 2000. The majority of the probable cause and consent searches are conducted by the CIT. In consent searches, the trooper receives the permission of the suspect prior to searching the vehicle, and in probable cause searches, the trooper has reason to suspect that contraband is hidden in the vehicle and consent is not needed (however, we were told by some CIT troopers that sometimes consent was asked for in what were clear probable cause situations). Looking at trends over time, we see that probable cause searches increased as a proportion of all probable cause and consent searches. The proportion of all searches involving African Americans declined over the four years of available data, 1997–2000. At the same time the “hit” rates (finding contraband) have increased for African Americans and are down somewhat for whites. If racial disparity in searches is any indication of racial profiling, it would seem that the NCSHP has lessened—or perhaps even eliminated—such activity over the course of the years we have examined.

At the same time, our discussions with CIT troopers indicate that the primary method used by troopers to initiate searches is the so-called “conversational method” in which the trooper asks the driver routine questions, and depending on the nature of the answers and the degree of nervousness of the driver, decides to ask the driver for permission to search the vehicle. The driver then has the option to either decline or accept the offer (we are told that most accept, even if they have contraband in their vehicle). As such, the method seems to us to open the door to a rather subjective process of decision making. When does someone appear to be “excessively nervous?” What constitutes an “inconsistent story” as to where one is going? In that the process is a subjective one, the door is left open for “cognitive bias” to influence the decision making of the trooper, possibly resulting in a disproportionate number of African American searches.

Indicators of Racial Disparity

One of the by-products of our efforts is to provide a road-map for some techniques of data analysis that others may draw upon in making a decision whether or not a particular troop or even a particular trooper is racially biased. In Chapter 2, we show how a troop district could be identified as having a relatively high rate of citations of African Americans relative to any of several base rates (licensed drivers, “drivers driving,” or drivers in accidents). By statistically controlling for variations in deployment (by time of day and location), and by collecting sufficient data at relatively small units of analysis (to limit the effects of spatial heterogeneity or the mismatch between where troopers patrol and where drivers drive), it is possible to obtain an estimate of the extent to which a district departs from an expected value. As such, districts which are found to depart excessively from other districts despite the statistical controls are candidates for further scrutiny.

It should be pointed out that our assumption is that an overt kind of racial bias is likely to manifest itself in multiple ways, and hopefully decision makers would have multiple indicators of such bias available to them. For example, there is likely to be a trail of citizen complaints about troopers who behave in racially derogatory ways toward citizens. Other troopers are likely to be aware of the expression of inappropriate racial attitudes on the part of a given trooper or even a troop. Records, including personnel files, are alternative sources of information on expressions of overt racial bias.

Racial bias of a more subtle kind, specifically “cognitive bias,” is not necessarily associated with citizen complaints, or other indicators of explicit bias. Rather, statistical evidence of disparity that cannot be accounted for by appropriate statistical controls is probably the best source of information on the presence of cognitive bias. The kinds of statistical analysis presented in Chapters 2 and 3 provide a means to ascertain where disparity could well be a sign of such cognitive bias. At the same time, we admit that the evidence is somewhat ambiguous as to whether a district has an exceptionally high rate of interventions directed at African Americans, or if a specific trooper has. The statistical analyses need be supplemented with further investigations to rule out possible rival hypotheses as to why the statistical disparity is present.

Self-Reported Traffic Violations and Stops

In addition to attempting to assess racial disparity in the official data bases of the NCSHP, we also conducted surveys of the general public to see if there was any correspondence between the two different sources of information (surveys and official records), and to determine if we could learn more about the behaviors of interest by studying the surveys. The results are

encouraging to those doing research in the area of racial disparity in that there are some broad similarities between the findings of the survey data with the findings of the official data. For example, according to the survey findings, African American drivers are significantly more likely than white drivers to have been stopped by the police in North Carolina. This finding generally corresponds to the official data findings. However, the survey also found patterns that could not be found with the limited official data available to us: local police are even more likely to have stopped an African American relative to their representation in the population of drivers and controlling statistically for self-reported driving behavior. Other intriguing survey findings include that African Americans are slightly more likely to have been informed that the stop was for a more discretionary reason, and they are also slightly more likely to report that they were treated disrespectfully after the stop. On balance, the use of surveys to study the controversial topic of racial profiling is promising.

Attitudes of the General Public

Survey data also are useful in that they tell us about people's opinions as relevant to such important considerations as whether or not trust in the police is undermined by racial profiling perceptions. One fact is undeniably true: in the focus groups that we conducted with African American drivers it was clear that African Americans' perceptions of racial bias in policing were influenced not only by their own stop experiences, but also by the stop experiences of family and friends. Some previous research has shown that well publicized and controversial police incidents, such as the Rodney King beating, tend to reduce trust in the police generally, particularly among minorities. Not only do media events affect attitudes toward the police, but so does the direct experience of citizens in their encounters with police, as well as the experiences of

friends and family. Although few people report acts of disrespect by the police, African Americans report significantly higher levels of disrespect in police behavior during stops of both household members and of friends.

The perception of racial bias on the part of police is widespread: 81 percent of African Americans believe that the police are more likely to pull over African American drivers. Just less than 70 percent of African Americans perceive similar bias against Latino drivers. In contrast, less than one third of the white drivers believe that there is racial bias against African Americans, and whites also see more bias against Latinos than African Americans. Interestingly, more white respondents believe that the police target for non-racial reasons than they believe that police profile on the basis of ethnicity or race.

We find that both African American's and white's trust in government, as well as in the police themselves, are undermined by disrespectful treatment by the police during a stop. In addition, African Americans who hear of disrespectful police behavior toward friends and acquaintances are less trustful of government officials—and the police.

Viewpoints of the NCSHP Troopers

Not surprisingly, the NCSHP troopers with whom we spoke in the focus group sessions say that they do not racially profile, although some admit that there may be “isolated instances” of it within the NCSHP. “Bad apples” exist in any large organization, the NCSHP not excepted. Moreover, they say that the nature of the work is one in which they are largely reacting to the behavior of vehicles, and that it is not easy to see the race of drivers until after the decision has been made to stop the vehicle (and often not until the trooper approaches the vehicle). At the same time, troopers talked about targeting highways where there are multiple bars (often lower

class bars), and not spending too much time around “country club” bars (“fishing those holes” were not seen to be especially fruitful). As such, and to the extent that such deployment choices are common, decisions of this sort do suggest a level of class bias. There was also some acknowledgment of what can be considered to be classic stereotyping, such as regarding Latino drivers generally being “bad drivers.” There was also discussion about enforcement practices that might increase or decrease levels of disparity in stop outcomes. While most commonly troopers suggested they have already made the decision to issue a citation (rather than issue a written or verbal warning) when the car is pulled over, some troopers said that they can sometimes be influenced by the resulting interaction with the driver: a display of “bad attitude” (inappropriate demeanor) may earn some drivers a citation rather than a warning. As such, these statements point to the discretionary aspects of NCSHP, and all law enforcement, work.

One clear message from all of the regular road troopers with whom we have spoken is that they are reluctant to become involved in vehicular searches. (The rarity of such searches by troopers in part validates this claim.) In the past, when the “war on drugs” placed more of an emphasis on drug interdiction, some troopers suggested that some racial profiling may have occurred (See Appendix B for a summary of focus groups with the NCSHP).

As for what to do about possible racial bias and profiling, the troopers mentioned record reviews, use of cameras in patrol cars, court visits to assess the quality of evidence and charges, and occasional ride alongs as possible avenues toward keeping check on troopers.

Conclusion

On the basis of all of our analysis, we conclude that although there is no conclusive evidence of widespread racial disparity exhibited in the actions of the NCSHP troopers in their routine interactions and interventions with drivers, there are some districts and some troopers whose citation rates of African Americans may warrant further investigation and possibly ongoing or intermittent monitoring. However, it is not our recommendation that such investigations necessarily occur. Presumably, the NCSHP would have more direct knowledge of several of the important factors that we have tried to measure that may help to account for the observed disparity (for example, more precise measures of deployment, especially as it related to time of day and location). Thus, the decision on their part to reexamine practice in 2000 would only be made if there were other information (information not available to us, but available to NCSHP leadership) of potentially biased behavior of any troop or individual trooper active in the force in 2000. Our purpose here is only to sensitize the NCSHP leadership to possibilities of irregular patterns that might be construed as evidence of bias. Given inadequacies in the data and in the measurement of key concepts, we cannot rule out the presence of low prevalence levels of bias, perhaps of the “cognitive bias” sort. Thus, we think that more evidence would be required than currently found with the data available to us, to proceed with further investigation of potentially biased behavior in NCSHP in 2000. At the same time we think that the methodology that we have developed here can serve NCSHP and other trooper organizations to monitor possible racial bias in their ongoing operations.

Our analysis of the actions of the CIT, which represents only a handful of the troopers in the NCSHP, leads us to conclude that even though there is less disparity than there was a few

years ago, the CIT troopers in the year 2000 are more likely to search a vehicle driven by an African American than by a white. The fact that the CIT is more likely to find contraband in an African American driven vehicle undermines, to some extent, the possible bias interpretation of their behavior. Nevertheless, the use of the “conversational method” as the primary mechanism for deciding to conduct so called “consent searches” leaves open the door to processes of possible bias (“cognitive bias”) in which the “signs” that the troopers are looking for are sought more often, or are seen to be more compelling, when the driver is African American.

Regardless of the interpretation of the empirical evidence of possible racial disparity, the perceptions of bias and of inappropriate treatment on the part of police (including the NCSHP) seems to foster distrust of the police. Aside from eliminating any vestige of possible racial disparity, the NCSHP would be wise to take actions to ameliorate the perceptions that their behaviors are unfair.

Appendix A: Baseline Observational Study

This appendix details the methodology we used to study one particular driving behavior—vehicular speeding—and correlates results from that observational study with official data on trooper stops for the NCSHP (during three months of the year 2000 – May, June, and July). It is proposed that the data collection technique pioneered by Lamberth, called the “carousel method” or “rolling survey,” can be improved upon and serve as a methodologically sound technique for collecting speeding data on demographic groups (Lamberth 2000). An analysis of fourteen highway segments in North Carolina indicates that speeding behavior is strongly correlated to NCSHP stops of citizens’ vehicles for speeding. Some evidence of possible racial disparity on the part of the NCSHP in the stops for speeding on the highways studied is found, but in general the results are not statistically significant. In general, NCSHP stop behavior of speeders seems to be determined by vehicular driving behavior.

Central to our concerns herein is the question of whether or not different races or ethnic groups have similar behavior rates. If so, then the task of determining whether or not racial disparity exists in stops, citations, and warnings of African Americans is made considerably easier. But upon what basis can we as researchers assume equivalent behaviors across demographic groups?

Studying Driver Behavior

The issue of whether or not assumptions of equivalency in behavior across demographic groups are warranted has not been adequately addressed in previous research. The omission is

primarily due to a lack of quality data, which in itself reflects several methodological difficulties of collecting data. These methodological difficulties center around the difficulties associated with identifying the race (or age and gender) of drivers traveling at high speeds on highways. A recent study by Lange and associates (2002) used video cameras and radar speed guns to determine race and speed, respectively. However, of the 38,747 images collected, only 26,334 were usable (12,413 were not; as reported in Kociewiewski 2002). That is, in almost one third of the observations, the race of the driver could not be determined or agreed upon by three researchers watching the video. This research, therefore bolsters our claim that collecting such data on race of driver is difficult.

One might think that data collection would be as easy as standing by the side of the road and observing. Armed with a radar gun and a pad of paper, researchers seemingly could record the race, as well as perhaps the gender, age, and speed of drivers as they drive by, and then would be able to compare such data with the rates of stops and citations for a given highway. Researchers might even be able to observe vehicles weaving unsafely, or vehicles with expired license plate “stickers,” or perhaps identify drivers they thought were “driving while intoxicated.” One could conceivably estimate speed of the vehicles on a video tape by measuring the time during which a known distance is traveled.

These first thoughts on how to collect data on the behavior of drivers, however, have several problems that make them impractical. One problem is that the glare on windshields and side windows of vehicles makes it difficult to see clearly the motorist’s race, as well as his or her gender or age, from a safe distance at the side of the road. The glare is in part due to the tinting done on most, if not all, windshields and to the angle of light from the sky (even on cloudy days). Video cameras suffer from the same problem. The skeptic is encouraged to simply try to stand

(from a safe distance) near a highway with fast-moving traffic, and see how often they can successfully identify the demographic characteristics of drivers. We experimented with this method and found that we were frequently unable to do so. Compounding this difficulty is the high rate of speed of the passing vehicles, as there is little time to assess demographic characteristics, much less their speed. More importantly, when we tried this technique (roadside viewing), it did not seem likely that our failure to identify demographic characteristics was random (else one could argue that misses were “random error” and could be safely ignored). Rather, some types of vehicles or conditions, such as an open side window, seem to permit greater visibility than others. In short, we suspect that it is simply too difficult to reliably ascertain demographic characteristics from the sides of busy roads (also, it is rather unsafe for the observer, unless he or she is well removed from the highway, but then it is even harder to see into passing vehicles).⁴⁶ Despite these concerns, we should note that the general substantive findings of the Lange et al. study are generally similar to what we report here regarding racial differences in speeding behavior.

As an alternative methodology, researchers might collect data on drivers by riding with NCSHP troopers. Researchers could keep track of what troopers encounter and react to on the highway. One drawback of this approach is that troopers might alter their behavior under such circumstances. Our concerns, however, were primarily with safety. We did not want to place our researchers in patrol cars during an extended period of time due to the risk of accidents (which is much higher than for the average passenger car) and of possible encounters with armed citizens (unlikely, but with potentially catastrophic consequences).

⁴⁶Further analysis of the data collected by Lange and associates, however, may prove our claims wrong.

Fortunately, there is a viable alternative to avoid the problems discussed above on the task of collecting data on drivers guilty of at least one type of law breaking: vehicular speeding. The “carousel method” of identifying speeders and their demographic characteristics is a method in which a research vehicle is driven at the speed limit, and vehicles that pass the research vehicle can be examined from the vantage point of the research vehicle, wherein the researchers can identify the race and other demographic features of the driver. Other researchers found, and we verified, that at the close range of the proximate traffic lane on a highway, one is almost always successful at identifying race and gender—and even age (see discussion below). There is little windshield or car-door window glare at close range. Moreover, in the vast majority of cases, our research teams were in agreement as to the race of the driver in the adjacent lane. By counting the race of the drivers who pass (as well as whom the research vehicle passes), we can identify the prevalence of certain demographic groups on the highway, as well as their prevalence among the speeding population. As such, the carousel method, as used by Lamberth in Maryland, was a big improvement over previous research studies which were based on the demographic characteristics of the area surrounding the highway. With the carousel method, the researcher can keep track of the racial composition of speeders and have a better denominator than, for example, Census Bureau population counts of local residents. The number of African Americans on a particular highway who are speeding could be compared with a numerator based upon, for example, the number of African Americans stopped for or issued a citation for speeding. Such a ratio could then be compared to the corresponding ratio of whites in order to determine the existence of racial disparity—and, by extension, any evidence of racial discrimination.

Modified Carousel Method

The problem with the carousel method as it has been implemented previously, is that not all speeding is equal. Passing vehicles could be driving 2 mph faster or 20 mph faster than the researchers' vehicle—and they would both be counted simply as speeders. Almost all vehicles travel above the speed limit on major highways, and it is well known that police do not routinely stop or cite vehicles simply because they are breaking the speed limit. Rather, vehicles must be exceeding a certain “threshold” of speed to actually trigger a stop and citation (or perhaps only a warning). The carousel method, as used by Lamberth, did not differentiate speeders based on the extent of their speed.

We explored the possibility of capturing the speed of other vehicles on the highway by using two radar guns (called in the business “same lane” radar, which means capturing the speed of a vehicle moving in the same direction of the patrol car or, here, research vehicle). One radar gun captures the speeds of vehicles as they approach from the rear, and the other gun captures their speed as the vehicle pulls away from the moving research vehicle. That is, the radar can capture the speed of a car in front of the research vehicle or behind. This approach of using radar has some obvious merits. First, the researcher can ascertain the speed of vehicles accurately, and at two points (back and front of the research vehicle). The average of the two is plausibly a reliable single measure of the propensity of the driver to speed. However, there is a serious drawback to using radar: it sets off radar detectors, triggers an alert on the CB, and slows down traffic. We suspect that virtually every semi-tractor trailer driver in North Carolina uses a radar detector, and so do many other drivers. By listening on a Citizen Band radio (CB), we verified that there is a high prevalence of CB use announcing the presence of radar. When we set off the

radar, there would almost always be a CB alert broadcast on the local highway, presumably heard by most truckers on the highway within a few-mile stretch. By constantly triggering the radar during the course of our study, we would be introducing bias into the study that we determined would be hard to correct. Another problem with radar equipment is that it is difficult to collect data systematically on highways while traffic is heavy—vehicles simply approach too quickly to allow you to get readings of every vehicle if you are driving at or below the speed limit. (It is generally not possible for safety reasons, of course, to have researchers drive above the speed limit.) We could have sampled the cars, but that would have meant more time on the highway and/or introduced more complexity into what was found to be quite a difficult procedure - —recording data on passing vehicles and their drivers.

We chose instead a simple, but at first glance seemingly error-prone method: using stopwatches to record how long it takes vehicles to pass our research van (of known length). The miles per hour of the passing “subject” vehicle could be measured quite accurately by two researchers, each using a stopwatch, and averaging the two speeds. The stopwatch is started by both researchers the moment the subject vehicle’s front bumper crosses an imaginary line perpendicular from the research vehicle’s (minivan) back bumper, and the watches are stopped the moment the subject vehicle’s front bumper crosses an imaginary line perpendicular from the research vehicle’s front bumper. By knowing the time it takes to pass the research vehicle as it travels at a constant speed, we can easily calculate the speed of the passing vehicle. We discuss below the fact that we systematically underestimate the speed by this method. However, we correct for this underestimation statistically to arrive at what seem like accurate measures of the speed of the passing subject vehicle.

We estimated that we could observe an adequate number of what we called “threshold speeders” by spending approximately 24 hours of one week driving a segment of highway approximately 10–15 miles in length, both ways. We were not sure when we started our research what the “threshold” of speed was that troopers use to stop motorists, but we estimated that it would be perhaps 7, or 9, or even 12 miles greater than the posted speed limit. (This was somewhat in error—see discussion elsewhere in the text.) We chose as many sites as we could afford to study. Fourteen were studied (although the prevalence of speeding on one of the segments was greatly reduced by the fact that there was some construction on part of the segment). These fourteen sites represent highway segments that meet the conditions of being four-lane highways, and highways that have frequent citation events for speeding such that we could compare the citation rates with the speeding rates (see discussion below). We omit details of where the fourteen sites are (that would constitute publishing the whereabouts of “speed traps”), but nine sites are along interstate highways, four along U.S. highways, and one on a N.C. highway. All are four-lane highways. Three of the sites are on Interstate 95, another four on Interstate 85, and two on Interstate 40.

Studying highways with more or less than four lanes was deemed unsafe. Two-lane highways would not permit vehicles to pass us frequently enough, so that it would take many weeks to accumulate data on passing vehicles. Six- or eight-lane highways (three or four lanes in each direction) were found in our preliminary tests to be very busy highways, complicating further the data collection protocols. Moreover, cars that were two lanes away could not as easily be gaged as to when they crossed the imaginary line extending from the back and front bumpers of our research van. To have chosen any less busy highways, or any with fewer speeding citations would have been cost prohibitive, as even more time would have to be spent collecting

data at each site. We found that the NCSHP concentrates patrols on particular segments of highway and that we were capturing some of these segments in our research. It is interesting to note that, although there are stops for speeding during the course of a year between almost any milepost markers on the busier highways (interstates and U.S. highways), stops are much more concentrated along certain stretches of highway that run for distances similar in length to those of our selected highway segments.

Baseline Data Collection

Beginning in January 2000, troopers of the NCSHP were required to collect data on each vehicular stop they initiated. They record the location of the stop by indicating the closest mile marker, wherever such information is available. We found that for the highway segments studied here with milepost information (nine), troopers did so more than 90 percent of the time. We were able to determine how many vehicles were stopped in the fourteen highway segments, and below we report a comparison of race-specific rates of stops for speeding to race-specific speeding behavior in the fourteen highway segments during a time period slightly longer than when we were collecting data—May through July. (We collected data in May and June, and no highway segment was studied for longer than one week during that time period).⁴⁷ We extended the time period into July to generate more observations so as to be able to compare the proportion of speeders observed who are African American to those who are stopped and/or cited who are African American.

⁴⁷ We added July to have more observations to compare to and under the logic that the traffic patterns are generally seasonal and that May, June, and July were more likely to be similar than April, May and June (April being the other month adjacent in time to May and June).

Data were collected using two research vehicles, with four researchers per vehicle, at two sites per week (one site for each vehicle). Of the four researchers in each van, one drove, one recorded time of day (to the second), race, gender, and age of driver, as well as other information (color of passing vehicle, state of the license plate, type of vehicle). The other two researchers timed the passing vehicle from the moment its front bumper crossed an imaginary line drawn in extension from the back bumper of the research vehicle to another imaginary line from the front bumper of the research vehicle.

As mentioned above, fourteen sites were selected using the following criteria. A site had to be a four-lane highway with sufficient recorded stops (which could result in citations or warnings) throughout a comparable time period (assumed to be May through July) to be able to compare rates of speeding behavior to rates of stops for speeding. It was judged unsafe to collect the data on highways that were too busy (for example, during rush hour) or with more than two lanes each way (as discussed above). Cars passing the research vehicle on both sides were distracting to the driver and the other researchers, and created “overload”—too many cars passing at once. It was all the researchers could do to record the data for each subject vehicle individually as it passed on the left of the research vehicle. We also found that it was hard for the driver not to get involved in the data collection process (see discussion below), and if he or she were on a three-lane highway, safety concerns would arise.

The fourteen segments had to have an appreciable number of African Americans driving on the segment. Sites to the far west of the state were eliminated due to this criterion. North Carolina’s African American population largely lives along counties stretching north and eastward from Charlotte to Interstate 95 where Interstate 95 enters Virginia. Many African Americans in North Carolina also live in the counties along Interstate 40 between Winston-Salem

and Raleigh. These were the areas within which we selected our fourteen sites. Each research site was between 10 and 15 miles in length (one way). The research vehicle traveled back and forth along these segments for approximately 6 hours a day, four days a week (although for some segments, data were collected for closer to 8 hours a day for three days). Thus, in general, 24 hours of observation, consisting usually from 9:30 a.m. to 11:30 a.m., and from 1:00 p.m. to 3:00 p.m., and again from 6:00 p.m. to 8:00 p.m., were completed for each of the fourteen sites. Noon hour and afternoon rush hour times were omitted because troopers frequently are attending accident scenes at these times, and thus are less likely to be patrolling for speeders then. We also preferred to avoid the heavier traffic situations as they seem less safe for driving. Also, under heavy rush hour traffic, speeding is largely limited by traffic density along these heavily-traveled commuter corridors.

Data were gathered during the second week of May until the end of June 2000. Data were subsequently entered by several graduate students into a spreadsheet and “cleaned” (corrected) for all fourteen sites.

For nine of the segments, mile post data were recorded by the NCSHP, such that we can compare the rate of African American to white stops for speeding with the rate of African American to white speeding within the same highway segment. When we started the study, we assumed that if we found that the rate was higher for stops than for behavior (within a margin of error), this might support claims of racial discrimination in these locales. However, we subsequently found that the process is much more complicated than that. More observations would be desirable (and possibly greater accuracy in assessing speed) to conduct a rigorous test (a test with a narrow margin of error) of the hypothesis at each sight to determine whether or not there is bias in the workings of the NCSHP.

As part of the study, but not presented here, we employed NCSHP troopers to drive in the vicinity of our research vans. We kept in radio contact with them and kept varying distances from them so as to best simulate an environment in which troopers identify speeders. Our thinking was that it did little good for us to collect speeding data on drivers outside the context of trooper location. Thus, it was preferable to collect data on drivers who were driving within a few miles of an NCSHP car. Troopers stop speeding vehicles that they identify visually—typically through the use of radar—and arguably, many drivers alter their behaviors depending on the speed of the vehicles on the highway around them. Thus, we wanted to capture the speeds of vehicles in these types of contexts. (In a subsequent publication we plan to discuss further the effects that NCSHP proximity had on driving behavior.)

Validating the Stopwatch Method

In order to test the idea that we could gather reasonably accurate measures of speeding vehicles using stopwatches, we conducted validation of the stopwatch method by recording the speed of a passing vehicle using a radar gun. To conduct the validation, we used the NCSHP highway “track” that is normally used for training purposes. We used three vehicles to validate the ability of each of six researchers to accurately time how long it took one of our vehicles to pass another vehicle that was traveling at a faster speed than that of the vehicle being passed. The passing vehicle will be referred to as such, and the vehicle being passed will be referred to as the stopwatch vehicle. In the stopwatch vehicle, researchers recorded the speed of the passing vehicle by timing the amount of time it took the passing vehicle to cross an imaginary line extending from the back bumper (perpendicular to the side of the vehicle) to another imaginary line extending similarly from the front bumper. The stopwatch vehicle traveled at a constant 35

mph, and included a driver and three researchers equipped with a pencil, pad of paper, and a stopwatch.⁴⁸ They were instructed to record the amount of time it took the passing vehicle's front bumper to pass from the stopwatch vehicle's back bumper to front bumper (vehicle length). They were instructed to write down the speed as they observed it and not to communicate to others in the vehicle what time they recorded.⁴⁹ The stopwatches were standard "sport" stopwatches, and the number of seconds it took for the passing vehicle to pass was recorded, not the speed in miles per hour. The passing vehicle was given a list of "passing speeds" at which the driver was to drive the passing vehicle past the stopwatch vehicle. These speeds varied from about 2 to 20 mph faster than 35 mph. Speeds faster than 20 mph above 35 mph (such as 55 mph) were thought to be too dangerous for the amount of "straight away" available on the training track. It was not thought possible to accurately record the time for speeds significantly faster than 20 mph above the speed of the stopwatch vehicle, since such stopwatch times that would be recorded were less than one second (i.e., less than one second would pass between initializing the stop watch and stopping it to read the recorded time.) CB radio was used to communicate between the passing vehicle and a stationary vehicle, which was parked by the side of the road. The researcher in the stationary unit used a radar gun to estimate the speed of the passing vehicle (as well as to estimate

⁴⁸ The training track was configured such that a speed higher than 35 mph was not possible without risk to the vehicle and passengers.

⁴⁹ The researchers were graduate students, and it is believed that they followed instructions and did not share time information (in part because the times were different from one another). We believe the researchers had no motivation to communicate the recorded times, since they did not perceive themselves as being tested, but rather the method was being tested. They were told it was acceptable to miss a speed measurement and to record whatever time was on their stopwatch. The variation in stop times across researchers was somewhat high, relative to our initial expectations for the accuracy of the stopwatch method, so it is doubtful that the researchers "cheated" and said their times out loud. Additionally, later tests conducted on actual highways (so called "same-lane" tests) further validated the stopwatch method, and for those tests the stopwatch researchers did not know the speed of the passing cars (which were gaged by the "same-lane" radar gun).

the speed of the stopwatch vehicle, to verify that it was traveling at the designated speed of 35 mph).⁵⁰ Also, the passing vehicle's driver had an assistant to record the speed of the passing vehicle. CB radio was also used to communicate between the researcher in the stationary vehicle and the stopwatch vehicle, but using a different channel from that which was used with the passing vehicle. The primary purpose of communicating with the stopwatch vehicle was to coordinate the efforts of both the moving vehicles.

The driver of the passing vehicle did his best to maintain a constant speed while passing the stopwatch vehicle, and the radar gun reading confirmed that this was the case. We maintained constant speed in order to more accurately measure the speeds of the passing vehicle, and thereby help us verify that the stopwatch method worked. Differences between the radar reading and the recorded speedometer reading were small (or at least seemed small to us at the time – see discussion below), and in only one test run, varied as much as 3 mph. In most cases the readings were the same, but in some cases the readings from the radar gun differed by 1 or 2 mph from that of the driver's speedometer. The training track is a closed circle such that the vehicles (two vans) could drive continuously, but they had to slow down for the curves. The speeds were recorded only when the passing vehicle was able to pass on the straightaway section of the track.

Figure A-1 shows the relationship between the actual speed of the passing vehicle (speedometer speed) and the stopwatch measured speeds for what we call the “first road test,” in which the first three researchers (out of six total) recorded the times on stopwatches. Because we only had one stopwatch van, we divided the validation tests into two sessions. In this figure the measured speed is calculated by miles per hour above the 35 mph of the stopwatch vehicle.

⁵⁰Actually, two radar guns were used. One was supplied by the NCSHP and the other was purchased by the research team. The two were almost identical in the readings made for the passing vehicle and for the stopwatch vehicle.

Figure A-2 shows the same information, but for the second road test (the next three researchers). Separate tests were done for each of the individual researcher's recorded speeds, and correlations among the researcher's speeds were found to be between .88 and .99, indicating quite a high level of reliability by customary standards. That is, the six researchers tested that day tended to vary little among themselves in their recorded speeds.

In the two figures, the horizontal axis represents the speed of the passing vehicle as recorded from the speedometer. Although there is some error in this measurement of the vehicle's speed, it is probably more accurate than the radar estimate of the speed, which is also shown in the graph as the hash-marked line (the uppermost line on the graph). Note that the speed values are sometimes repeated (for example, 44 mph occurs more than once in the first time trial. Thus, we should not expect any straight lines across this graph).

Note that as the actual speed of the passing vehicle increased across trials, the researchers were underestimating to an increasing extent the speeds of the passing vehicle. For example, at 40 mph actual speed the estimated speed is just a little less than 40 mph, but at 52 mph actual speed, the researchers were estimating about 42 mph—roughly 10 mph under the speedometer speed. Essentially, as the vehicle passed, it took relatively longer for the researchers to stop their stopwatches. We noticed that researchers virtually never stopped the watches prematurely, meaning before the passing vehicle's front bumper actually passed the front bumper of the turtle vehicle. But the faster the passing vehicle's speed, the more distance traveled by the passing vehicle past the stopwatch vehicle's front bumper before the researchers stopped their watches. Nevertheless, the researcher's were systematic in their error. As discussed in the next section, we can correct for the systematic underestimation of speed and generate an estimate of the speed of the passing vehicle within a known error range.

Correcting for Under-Estimation of Speed

Predicted values from the equation in the above paragraph were found to fit the speedometer speed measure quite well (see Figure A-3 and A-4). Here we took the average of two estimates, one from each of two research “timers” (we use two stopwatch timers in the observational baseline study of the fourteen sites). Thus, the speeds of Timer 1 and Timer 2 were averaged for one estimate for each passing vehicle’s speed, and the estimates of Timer 2 and Timer 3 were averaged for the other pair’s estimates. Comparisons can be made here with the radar estimate (the dotted line on the graph) or with the horizontal axis (vehicle speed), as they are very similar. Here one can see that the “corrected” estimated speeds of 40 and 52 mph, for example, correspond with varying degrees of accuracy with the observed speeds. Thus, the systematic nature of the under-reporting of estimated speed can be capitalized on, in order to correct for the speed underestimation.

Figure A-1 Stopwatch Validity Test for First Three Researchers

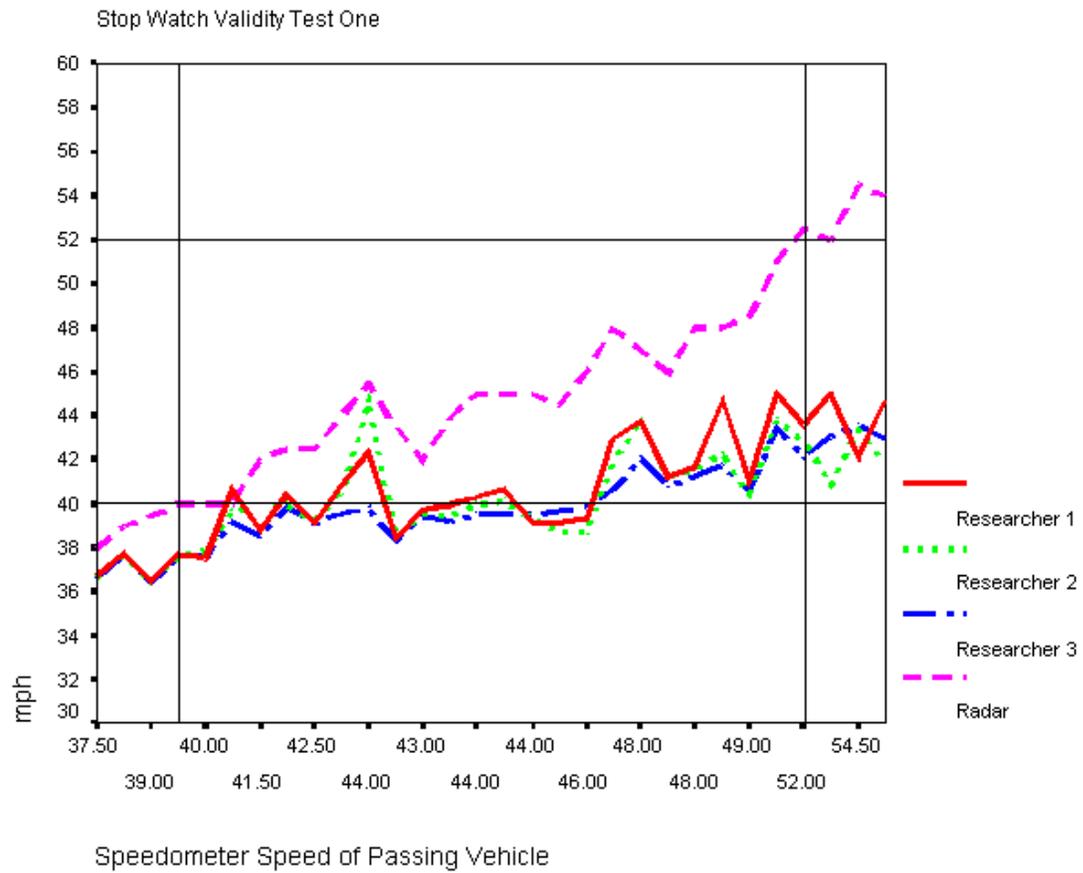
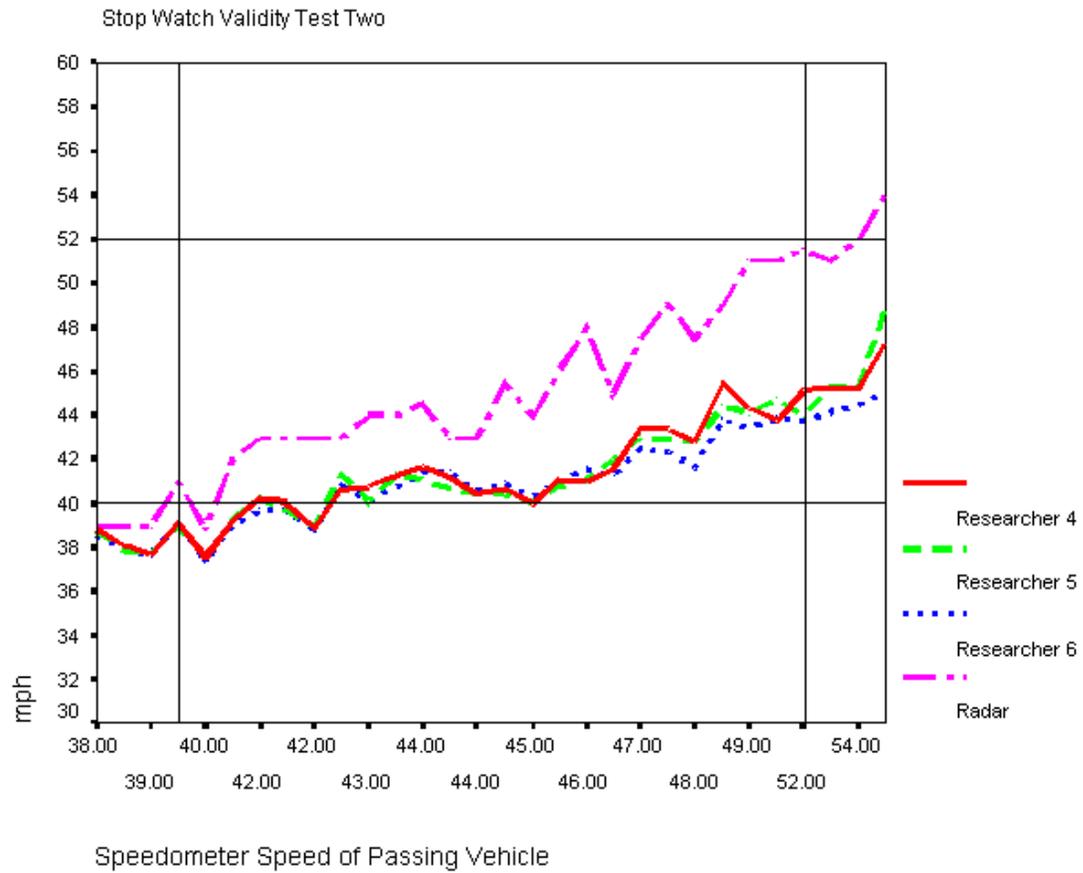


Figure A-2. Stopwatch Validity Test of Second Set of Three Researchers



If one looks closely at the differences between the actual and estimated speeds across values of actual speed, it is arguable that these researchers were somewhat overestimating speeds in the 40–48 mph range, and somewhat underestimating the speed greater than 48 mph. However, in Figure A-4, the three researchers there show no such pattern.

Note that on the actual highways of our observational study, our stopwatch van would not be traveling 35 mph, but between 55 and 70 mph. We are assuming that our ability to record speeds above the posted speeds on the highway is the same as on the training track. Thus, if a passing vehicle is traveling 50 mph on the training track or 15 mph above 35 mph, the speed of the stopwatch van, it is equivalent to a vehicle traveling 80 mph while passing our stopwatch van traveling 65 mph on an observational study highway segment.

We presumed that the risk of being stopped and cited at 15 mph above posted speed was substantial for most highways. At the time we collected the data at the training track, we did not think it likely that speeds of greater than 15 mph above posted speed would be common. As it turns out, we subsequently learned that 15 mph above posted speed is quite common, and is often the median speed for which drivers are stopped and cited for speeding on the fourteen highway segments of our observational study. Thus, it would have been desirable to measure speeds more accurately at between 20 and 25 mph above the posted speeds. We assume that the risk of being stopped and cited may go up substantially as a vehicle's speed rises above the 15 mph speeding threshold.

The implication of these figures is that the researchers' stopwatch times (from recording the time it took for vehicles on the highway to pass them) could be mathematically transformed into a reasonably accurate estimation of the speed of the passing vehicle by using the regression equation, discussed in the section above. That is, the estimated speed (raw score speed) could be

multiplied by 1.73 (subtracting .353), to arrive at an estimated speed of the vehicles. Note that we corrected the average speed of the two stopwatch times in the collection of the speeds of the passing vehicles in the road trials across fourteen sites chosen for the baseline observational study described below (although occasionally only one stopwatch time was available).

Figure A-3 Corrected Average Speeds, Time Trial One

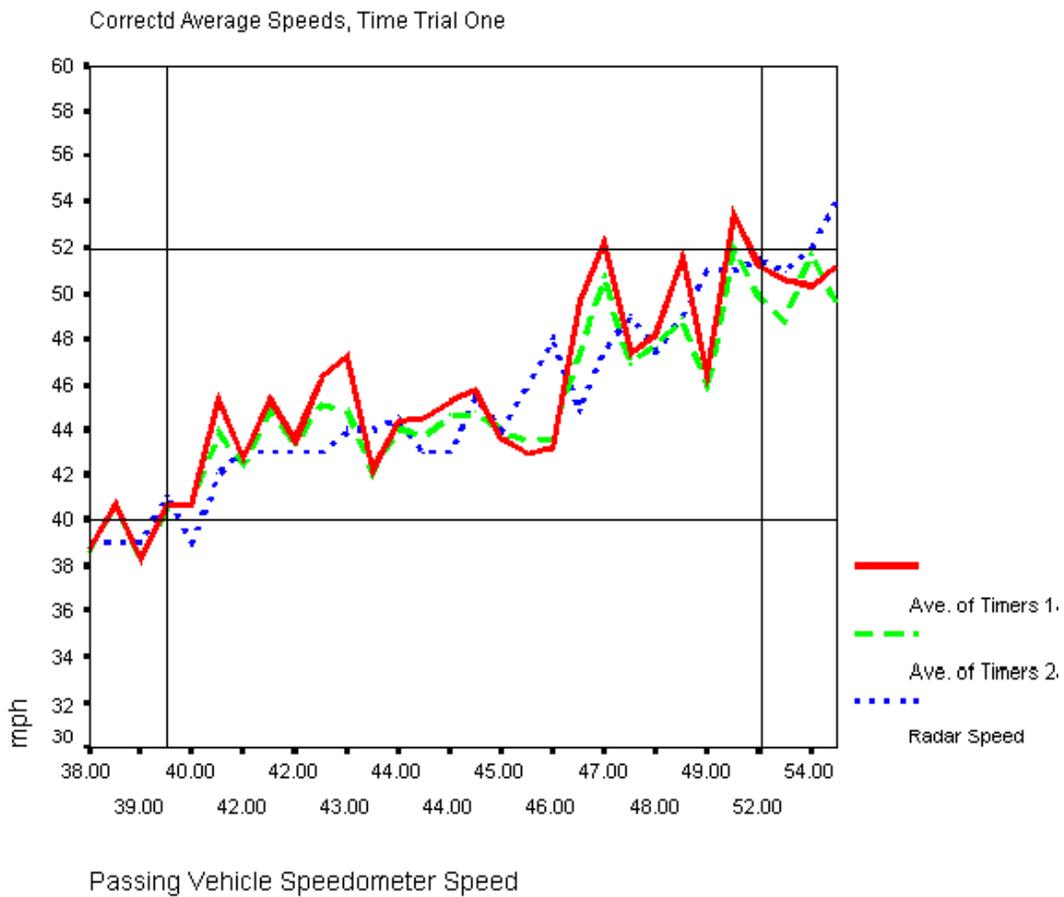
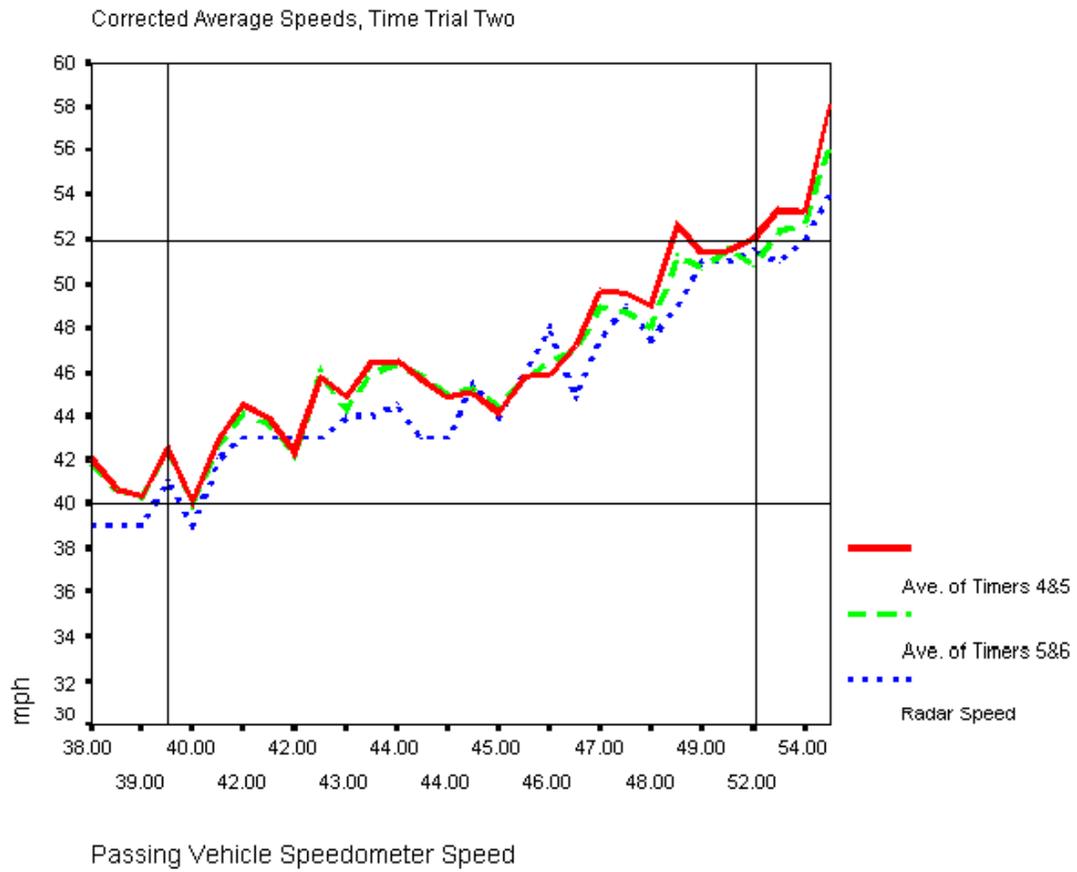


Figure A-4 Corrected Average Speeds, Time Trial Two



Note that, unlike the case with the road testing at the training track described above, passing vehicles on a real highway might accelerate or even decelerate as they passed the research vehicle. Thus, there might be greater error in recording the actual speed of the passing vehicle than we observed in the artificial conditions of the training track. Nevertheless, the stopwatch timers are measuring an “average speed” of the vehicle as it passes the research vehicle, and as such, the estimate should be sufficient to test hypotheses as to differences in speeding behavior across demographic groups, within a margin of error of a few miles per hour. Moreover, we conducted other tests involving the use of a “same-lane” radar gun, which allowed us to assess the speed of actual vehicles passing a second research vehicle (from a first research vehicle following roughly 100 yards or so behind the research vehicle). We found that, in general, the recorded speed did correspond quite accurately to the speed of the passing vehicle measured with the radar gun, and that the vast majority of cars passing did so at a reasonably constant speed.

It should also be noted, however, that there is a substantial margin of error associated with the estimate of the speed of the passing vehicles. One can get an intuitive sense of the range of estimated stopwatch speeds by looking carefully at the variation in the graphs in A-3 and A-4. In part, the fact that the lines are not smooth speaks to the variation from the speedometer speed (also recall that some speeds are repeated on the horizontal axis as some time trial speeds appear more than once). In the graph, one can more easily see the differences between the corrected estimate and the radar speed than one can see the differences between the corrected estimate and the speedometer speed. Therefore, the average of the pairs of timed estimates can be quite different from the radar speed. Also, at the reference lines, one can see that the stopwatch speed varies by a few miles per hour from the speedometer speed. We ran some simple regression equations in which the speedometer speed of the passing vehicle was the dependent variable and

the average estimated speed (stopwatch speed) for pairs of observers was the independent variable. The range of standard errors associated with these regression equations varied from 2.61 to 5.24, indicating that, on average, an estimate could be wrong by plus or minus 2.6 to 5.2. As it turns out, this is a rather large degree of variation relative to the variation associated with the measure of speed on the highway by the NCSHP troopers (who generally measure speed within 1–2 mph of the best estimate of actual speed). Also, we were surprised when we later discovered that there is a big difference between traveling just a few miles per hour faster or slower than the speeding thresholds on a stretch of highway. When speeding thresholds are exceeded, they trigger a stop and often a citation. A 1 mph difference in speed can make the difference between whether or not someone receives a citation. The inaccuracies of the stopwatch method will attenuate slightly any correlation between the estimated speed and the actual speed (here we are assuming that is the speedometer speed). As we subsequently learned, the range of likely speed threshold values (speeds that are likely to result in a stop or citation) is quite narrow relative to the accuracy with which we can reliably measure speed, thereby limiting our ability to test hypotheses for each of the fourteen highway segments. (Results of the observational study can be seen in Appendix G.)

References for Appendix A

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Appendix B: North Carolina State Highway Patrol Focus Groups

Topic: Racial Profiling

Selected Findings

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North Carolina State Highway Patrol Focus Groups

Focus groups were conducted to gauge NCSHP trooper perspectives about the nature and extent of racial profiling in North Carolina. Six focus groups were conducted in early June 2001. Due to the racially sensitive nature of the topic, four of the six focus groups were race specific (two were African American and two were white). This was deemed appropriate in order to best provide a forum where respondents would feel less restricted, although each group noted that they would feel comfortable speaking in the presence of fellow troopers. The management and command groups were racially diverse. Our random selection process did not capture any female troopers (there are few women in the NCSHP relative to men). Focus groups numbered from six to nine troopers and each lasted approximately 2 hours. The sessions were facilitated by two members of the research team. Three other members of the research team observed the sessions shielded by a one-way glass window.

It should be understood that findings from the trooper focus groups cannot be generalized to all NCSHP troopers. They were not selected, nor were they expected, to be spokespersons for their peers. We desired insights into the activities of individuals on patrol in order to better understand what they do and why. Further, the specifics of what we learned from these individuals cannot be generalized to other law enforcement agencies (such as local city police) due to the differences in scope of their duties and restricted jurisdiction.

For both the general public and the troopers, there appears to be both confusion and disjuncture between what is thought to be “good” law enforcement, on the one hand, and “race-based” law enforcement on the other. Law enforcement practice, both organizationally and individually, is influenced in part by a multitude of experiences brought to the table. These “experiences” result from an accumulation of identifiable and individual encounters with the

public. They also include generalized expectations of behavior that may or may not be the result of specific or accurate information. The latter generalizing process is best considered to be a process of stereotyping. Certain stereotypes associated with minority members of the community may subject them to more intensive scrutiny, may result in defining their behavior as more serious than it is and the alleged party as more culpable, and may lead one to believe that the transgression is more deserving of official intervention than would occur in the absence of the stereotype. Whether conscious or unconscious, stereotypes held related to degree of suspicion, prevalence of crime, or dangerousness can lead to more frequent stops of African Americans and other minorities, as well as influence decisions to release, warn, cite, or search vehicles and occupants.

Race as a profile serves as both a predictor and an explanation for citizen's behavior. It is all too common for individual's to answer the questions of "why do you think African Americans are stopped more often than whites?" or "why do you police make so many stops in a certain area" with simple answers like "well look at the crime rate, look at the prison population." It would seem that in most cases, police and citizens do not recognize the disconnect in their answers associating the assumed (but often unknown) robbery rate in a particular part of town and a routine traffic stop in that or another area. Still, the answers are given without blinking an eye and the practices are considered both good policing and a service to the community.

Leaving the question of efficiency aside for another time, such proactive policing strategies might be viewed as uninformed, but more benign, if racial stereotypes were absent. However, given past and current American race relations, it is quite unlikely that we separate race totally from our thinking about crime. It is precisely this type of discrimination that can be so harmful

and which has been highlighted in the public controversy over race-based policing. Indeed, the ACLU (1999) notes that “... skin color has now become a proxy for criminality.”

For our purposes here, acting out one’s racial prejudice through law enforcement, individually or organizationally, is “racial profiling.” Such prejudice can be in the form of presumed criminality, fear of social integration, or a sense of white privilege. Complicating the issue of racial profiling is the extent to which law enforcement decisions are influenced by socioeconomic standing. To the extent that this is true, the demarcation of what is, and what is not racial profiling becomes blurred.

Below are selected findings from the focus groups. Consistent with the structure of the focus groups, proper summary findings are presented in the following areas: 1) Decision to Stop, 2) Enforcement Decision, 3) Decision to Search, 4) Minorities and Traffic Stops, and 5) Steps to Stop Racial Profiling.

Decision to Stop

It appears that troopers, for the most part, engage in enforcement patterns they believe yield the greatest number of enforcement opportunities. A major determinant in the decision to make a stop appears to be the “behavior” of the vehicle. The focus on such behavior seems to vary situationally. The interstate, it is believed, is more likely to yield speeding violations rather than seatbelt violations. Participants state that it is not possible to know the race of the driver on the interstate or at night. Rather, they focus on the behavior of the vehicle. For example, in the case of Latinos, they are believed to be more likely to violate driving laws. Consider the following participants’ comment:

And I think it depends on other factors such as where do you . . . put your selective enforcement. Where are you going to put your people? Just say . . . We have accidents in . . . I'm sure. But where are you going to have the most fatalities, are you going to have up here on north of . . . Road or are going to have them down here at . . . , you know, where you have got eight or ten beer joints down there, where you know that these people are going to just leave. It has nothing to do with whether they are black or white, the blacks or whites live in that area, but you have got all of these beer joints and clubs and stuff down there and these people are going to leave this area and they are going to get out there and they are going to speed and we are going to have fatalities, . . . , you are going to concentrate on the weekend. You are going to put your people in that area where you see that you have had these accidents, where you have had fatalities. Where you have had these crashes, that is where you are going to assign your people to, during those times. That is what this strategic planning and stuff is suppose to be. Putting your people where you have problems.

I'm the same way. I'm in an area where there's a heavy influx of Hispanics and to be downright honest, they're terrible drivers and they get stopped a lot because, you know, you may mistake them for an impaired driver. They get stopped and there's a big portion of them that are impaired so they account for a lot of our DWI risks but also they're stopped and given a lot of warning tickets because they're just terrible drivers. They don't have a lot of driving experience I don't think. They come up here and get mingled in this traffic and they cause a lot of accidents. They're a victim because of their driving, because of inexperience.

I think stereotyping and violations go together. If you look at a certain group, stereotype a certain group and in that group you deem it to be, have more violations, then naturally you will work more at stopping whatever . . .

Participants were asked if they believe traffic enforcement was more likely in a “country club” area as compared to a “low-income” area. The general response is that the law is enforced whatever the circumstances without regard to social status. Still, troopers acknowledged that it is easier, because of reasons out of their control, to do their jobs in some places and not others. Simply, some citizens are more likely to resist the legality of the trooper’s action, complain to supervisors, and challenge the citation. Each of these situations is something that most troopers would like to avoid. Interestingly, but not surprisingly, it was reported that the NCSHP receives more complaints from whites, as compared to African Americans and Latinos. We had picked up

the same theme in our citizen focus groups. White citizens tended to see any stop as an unnecessary intrusion. African American citizens tended to acknowledge and accept responsibility for stops resulting from clear violations of traffic laws. This raises an auxiliary issue that compounds the problem of racial profiling: how does the coupling of expected resistance from a white—and perhaps more affluent segment of society—with possible stereotypes of the minority community (for example, the perception of a certain expected level of law violations) impact the numbers and degrees of disparity in stop outcomes? With regard to resistance, consider the following response (it should be noted that this was not the prevailing norm):

I mean, it might be true now that the area that would display less resistance. I mean if you got a, if you're looking at income, you've got a rich area and a poor neighborhood and if the rich people get nice attorneys and do a lot of things, then spread the word around as well, then you will be more apt to go to the area where you get that less resistance, where you don't get that high-priced attorney putting you on the stand and then taking you through all these things. I still think you will work both but you might at some point go to the other one a little more frequent because there's less resistance to, you know, what you're trying to do in the first place.

Your higher income people generally drink and socialize at places where your lower income people don't. Friday and Saturday night bars where your younger people target, where you have a lot of your calls are searched, a lot of your fights. Your higher income people don't socialize at the places that you make a lot of these arrests and get a lot of these calls from. You know, you'll be in the toxilizer room and occasionally somebody will bring one in where they got picked up. They were working the road and you'll pick up a high dollar somebody who just, whose driving is impaired and they pick those up. As a general rule, I don't think a lot of your police officers target the type of places that maybe your more influential people might drink at Holiday Inn lounges and places like this. It normally don't call your attention there. You really don't have a reason to be there like you're called to a lot of other places.

The rationale for targeting people of color is founded in one's perspective about what crime is, where it occurs, and the perceived risk to social order. This is a grey area where income

and social status might serve as proxies for race. Still, it should be recognized that even attention to presumed status is a form of stereotyping. Furthermore, differential treatment as a result of presumed status may also indirectly but systematically disadvantage African Americans and other ethnic groups. The following quote reflecting “lifestyle” differences is a case in point.

I would say that is also—I think that one thing that I don't know if it is factored in or not, it is a social behavior, economics has a bearing on what the behavior of some people. For example, if you are in . . . where the upper socially economics predominately live and there are both minorities and Caucasian up here but in proportion, they are mostly white. My experience has been, and it is just my perception, I can't back it up with anything, but their behavior as to how they socialize is different. It puts them less at risk than the socioeconomic people on the lower level, be it black or white. By that I mean if they have a house party and they all camp out at the house and they never leave whereas the lower socioeconomic people, both black and white, from my experience in . . . and some of these other counties I worked, they were traversed to a place like a tavern or something and our guys are circling those bases be it black or white, they are just circling them and the time that you depend on when they come out of there and you observe some kind of erratic driving, they are going to get flagged. So they get stopped if it is predominantly black home, then most of the people arrested from there are going to be black.

While there was acknowledgment of the possibility of racial profiling, it was generally believed that it is an infrequent occurrence today as compared to a more frequent occurrence in years past. Since the large drug interdiction units were dismantled, and the competitive nature surrounding the quantity of drugs seized has been lessened, troopers indicated that there have been fewer complaints. The following comments reflect the general feeling about the past and present levels of race-based law enforcement, also called racial profiling:

I think when we had our interstate squads Statewide and they were really looking for drugs and doing more searches, I think since that dissolved I haven't heard the complaints about, you know, all these people are being stopped, white or black, and they're being searched because I mean it was kind of like they were making great big drug busts and getting their names in the paper. The Governor was giving them awards and blah, blah for getting a tractor-trailer load of dope and \$350,000 so I think especially the young guys might have been a little more apt once they got you stopped to say I may get me a big drug bust here or something

about this guy ain't right and I'm going to search him. I think now that's resolved. I don't think the general population Patrol Statewide are doing numerous searches like they were when we had that interstate squad that operated all the way across the country. I haven't heard any complaints in several years now on unreasonable stops and search since the interstate squad went.

Well I'd be surprised if you don't have some that do. White and black. I mean, you know, there's no way that in 1,400 or 1,500 people you're going to have 1,400 or 1,500 people that's going to go right by the rules, you know?

There may be some whites that target blacks and there may be some blacks that target whites or they both may target Hispanics, I don't know. As a whole, I don't think the Patrol's got a problem with it. There might be some isolated instances or there may have been more in the past. I wouldn't dare stand here and tell you I don't think it ain't never happened cause I'm sure it has.

I think awareness has a lot to do with how people respond. In the last probably five years, a lot of emphasis has been put on you might say racial profiling or black/white issues and I think when people see that, naturally I think they sort of pull back a little bit because they don't want to be in where the target is. But I'd like to say that the people you have in the community, when they come into law enforcement, unless they make a conscious effort to change how they feel and think about others, then that still is prevalent within them even in law enforcement. Of course, being smart and wise, a lot of this may not come out to be where you can really point and pull it out but I still think people must make a conscious effort to change the way they was brought up or thinking and look at reality versus what is and what's not and that kind of thing. But I don't think the Patrol has a problem Patrol-wide but I will probably say that there are probably instances that these things do take place.

Senate Bill 76 mandates that troopers complete the “stop form” as a means to better understand law enforcement patterns and practices as well as the socio-demographics of citizens stopped by troopers. Most participants believe such reporting is unproductive and an undue burden on law enforcement personnel. Among other concerns, troopers reported that there was no way to match the stop forms with citations. One must at least consider the accuracy of these forms. Consider the following comments:

Well see you've got, you've got men writing a certain number of citations and writing a certain number of warning tickets. Now we don't know who these people are writing these stop forms because it's a secret number so you don't know if

you've got something wrong in that stop form. They didn't fill out all the blocks. What do you do with it? You don't know who the hell to give it back to and you don't know who to talk to about it so what do you do with it?

Some troopers fill those stop forms out daily and put them in the box. You'll pick them up. The secretary enters them. Some of them will wait until Sunday night and fill them all out at one time like that. You know, I don't recognize the numbers on the top of them but when I enter some of them, some of them I can recognize the handwriting. But I couldn't a bit more tell you what that man's number is. I don't even know what my number is right now.

If you knew who it was then you could check the number of citations they're putting down and the number of warnings tickets, but that still wouldn't take into account the vehicles they stopped that they didn't do anything on.

Enforcement Decision

It is not possible for troopers to stop every traffic infraction; therefore, discretion is a significant consideration. A vast majority of the participants state that they have made up their mind before the stop as to what action they are going to take, such as a verbal warning, written warning, or citation. They report doing so is a matter of fairness:

It is the only fair way to do it. You should issue a citation, it should be based on the violation and not necessarily and not on their personal attitude. If you are going to write a person a warning ticket for a 65 and the person is nice to you and that is what you always do you write a warning ticket, I think you should continue, if you got somebody that ain't so nice, write a warning ticket. I am not there to judge a person, I mean you stop and you don't got to love them because you stop them. I understand you write some people a citation and they got to hug your neck because you write them a ticket or whatever. You have got to do your job and just go out and do it.

There were clear differences among participants concerning written or verbal warnings. It was expressed by one participant that giving a written warning as opposed to a verbal warning was necessary to protect oneself in case of a complaint. A warning citation is perceived as a good

tool to promote vehicle safety as well as an effective public relations medium. But in any case, the decision is probably made prior to the stop as noted by the following four participants:

And I think the violation of law and to what degree. I mean, most of the time I think when the guys stop the car their mind is made up as to what they're going to do already and if the violation that you write a citation or you write a warning, you just do that but I don't think there's any standard that's set for, you know, clear cut substantial violation to issue a citation for. A potential violation, you give warnings.

You know what you are going to do before you get stopped. If it is a clear cut substantial violation, they are going to get a ticket. Otherwise, they are going to get a warning.

It is already decided.

I see the violation and it is a clear cut substantial. Before I hit my blue light, it is already decided, whoever it is, it is a violation.

Decision to Search

When asked why they decided to search a car, 64 percent (N = 22) of the troopers stated that they did not search vehicles unless absolutely necessary. Participants cite safety as a major disincentive for conducting a search.

We have some people that do that but it's not very wide across the State because that's a real good way to get hurt to do a lot of searching a vehicle and you don't have any help. Someone to watch what they're doing while . . .

Thus, there were strong sentiments expressed against conducting a search. The most notable exception is the “plain view search.” In the state of North Carolina, a police officer is required to address any contraband that they see in plain view while executing a duty of their office. It was apparent that the troopers would conduct a vehicle search if the circumstances revealed illegal items that were in plain view.

The second most prevalent reason to search a vehicle is a "search incident to arrest." The troopers stated that they conduct these types of searches. North Carolina statutes provide that any law enforcement officer may conduct a search of an arrestee or the area within the arrestee's immediate control. This would obviously include the vehicle where appropriate. The primary justification for the "search incident to arrest" is officer safety. A similar percentage of troopers who reported conducting "searches incident to arrest," also stated that safety was of primary importance when deciding whether or not to conduct a search.

If I feel like searching that person, it is because I'm doing it for my safety. I do not want to be riding down the road and have a big old gun come jumping [out] at me.

Troopers understood that they had the ability to ask for a "consent search." Law enforcement officers may ask a citizen for consent to search their property as long as a few conditions are met. First, the law enforcement officer must be sure that the subject does not think they are in custody at the time the request is made. Second, the officer must not imply menace or negative consequences for a refusal. Troopers noted that search requests are seldom made.

[You] put yourself in a bad situation because, how are you going to justify that? You go to an individual and say, you stop the speeding and there ain't nothing else there; [C]an I search your car? If you don't do that on every single stop, there is going to be something [that] look suspicious.

Minorities and Traffic Stops

Participants report that only a small number of minority stops could be attributed to "unfair troopers." They assert that in an organization their size, there would be some "bad apples." They concede that American society is racially divided, and that it would be naive to suggest that no individuals joining the ranks of law enforcement harbor these sentiments. When those attitudes infiltrate law enforcement agencies via its officers, "then all you do is put on the

uniform but you are still that same person; therefore, you are prone to have some racial bias.” However, one third of the participants voiced the belief that, while prejudice exists inside law enforcement, racial profiling is not a problem. When conducting a routine stop, these troopers behave in a professional manner.

Indeed, most participants believe that the concern about racial profiling is unfounded. They believe profiling to exist when things look unusual or out of place, for example, if two African American males are spotted circling someone’s house in an all-white neighborhood. The troopers label this as profiling because those individuals are considered to be out of place. They contend they are trained to profile, that is their profession, but not necessarily racially motivated profiles. Overall, the troopers maintain that minorities and others are stopped due to violations of the law and not due to race. They also attribute the stops to the geographic location of the patrolling trooper. If a trooper is patrolling a predominately Latino or African American area, he or she is prone to stop the individuals within that ethnic group more because they dominate that particular location.

With regard to racial differences in driving, a minority of participants maintain that Latinos commonly operate their motor vehicle without a driver’s license. This was the most commonly noted response from the troopers, and there were no other differences expressed by the troopers.

Participants reported different experiences with how fellow troopers treat minorities and ethnic groups. Some troopers acknowledged that they knew of certain troopers who they believed were prejudiced against people of color, but acknowledged that they only knew of a few of them. When asked if people needed to be concerned with racial profiling in North Carolina, one trooper replied:

Of course. I mean, as much as I like the guys I work with, you hear remarks. Things that are said. It's not hardcore derogative towards any group but you can tell, yeah, I can see if this person's put in that situation or if well, gee, every black person, I stop they're always giving me some mouth you know.

Okay, now, depending on how firm you are with that particular person. Now some people are just not as understanding. I'm not saying all of them are like that but some people are not used to being around blacks.

When asked about stereotyping within law enforcement, a few participants agreed that stereotyping existed in law enforcement. Responses provided by the troopers suggest that stereotyping serves more as “cues” in law enforcement. Although responses varied in the degree of stereotyping taking place within agencies, the troopers who answered the question agreed that it does exist in smaller law enforcement agencies as well as larger ones. Consider the following participant remarks:

I just want to say that we are all trained and I do not care what law enforcement agency you are in, you are trained to profile and you act on that. It is inherent. This business requires that.

I think stereotyping and violations go together. If you look at a certain group, stereotype a certain group and in that group you deem it to be, have more violations, then naturally you will work more at stopping whatever

According to those who believe that stereotyping does not have an impact on general law enforcement practice, a couple troopers thought that stereotyping did have an impact on who is searched.

With the variety of responses provided, it would be reasonable to conclude that the responses are based on the troopers' individual experiences and evaluations of the behavior of others. The greater consensus gathered from the troopers suggests that racial profiling, while not representative of the entire agency, does exist at an individual level—but to a lesser extent today than it did in the past. They also recognized that many of the prejudices and much of the bigotry

remains. Generally, they are confident that their professionalism and training in the NCSHP keep those problems in check. Still, troopers were less certain that the same could be said for law enforcement, generally. When asked whether or not they were concerned about being victims of racial profiling outside of North Carolina—each of the white participants said no; however, a majority of African American participants said yes.

Steps to Stop Racial Profiling

The eradication of racial profiling was discussed by all participants. The most frequent answer was accountability. The second most frequent response was training and education.

You talk about training, we are taught to recognize, to know or recognize some of the effects but the majority of us that are not involved on the interstate team where a lot of those guys are trained into what to look for, doing extensive interviews.

Only one subject reported that, in his view, mandatory stop forms would reduce racial profiling in traffic stops. Troopers were asked if they thought there were some who may misrepresent the race of the person being stopped. One participant stated:

When it first came out I heard people say that they were going to be singled out and they feel that way because they stopped five blacks and then you stop five whites.

Having a camera in the patrol vehicle as a deterrent was questioned by one participant.

We just had a trooper transfer from _____, this boy averages six or seven complaints a week. They put a camera in his car to try to, you know, catch him. Now what good is that going to do? The boy is that smart. He's smart, he won't say anything with the camera. They can't get him on anything but let the camera go ahead, he's the biggest asshole you've ever seen in your life. I'm serious. He talks down to people all the time.

More participants believed that cameras serve more as a protection (for example, protection against complaints), than as a tool to combat racial profiling.

A situation I had . . . when a guy stopped this blue jeep Cherokee. It was coming out of Alabama, a black female who alleged that the trooper used racial slurs— because she was stopped by a white trooper. . . Then I asked her did she know that the incident, from the time the decision was made by the trooper and the car was still rolling before it ever came to a stop, chasing, and all the way up to the time when the car pulled off again, after being issued a citation, it had been documented on video and did she know that. She did not know that and the last time I heard from her was she hung up the phone. . .

Whether or not Senate Bill 76 will have a substantial effect on law enforcement practices remains to be seen. The stop form appears to be widely resented and viewed as a burden by troopers. The increased scrutiny of trooper behavior may result in troopers being concerned about “balancing the books.” One participant stated:

Yeah, golly, the last four I wrote were whites, so I guess I better write a black person now, or vice versa. Now, whether they actually go and do that, I don't believe that happens, but there is a lot of conscious there, more so than there ever has been in the past. I think most of our people, like me, are dedicated, committed state troopers who are trying to do a good job if properly guided and led. I believe that what they will do is when they are faced with it, they will deal with it and I don't think it will matter who it is.

Conclusion

Racial profiling is a multidimensional issue, in other words, disadvantage can appear in numerous ways in a variety of situations. While findings from the focus groups are not conclusive for the collective organization, they provide a better understanding of law enforcement's perspectives on racial profiling, as well as a contextual framework to assist with the interpretation of stop and citation data. By way of a caveat, findings from these focus groups

should not be generalized to local and county law enforcement. Their law enforcement functions and cultures are distinct.

Law enforcement has the burden of history and perceptions, real or distorted. Historically, racial profiling and racist law enforcement in the United States are unfortunate realities, as noted in works such as that of the Kerner Commission in the mid 1960s. Such practices have continued, with and without harmful intent. The issue of prejudice is the engine to such practices, and stereotypes are the fuel. Examples abound that give credence to the public's concerns. For example, Carl Williams, former Chief of the New Jersey State Police was fired for stating that "mostly minorities" trafficked drugs. As we have argued previously, the result of casting a wide "race" net is more likely the capture of innocents and not those who are guilty. The wide net should never be mistaken for "good" police work. Most participants acknowledge that there might be a few troopers that engage in racial profiling or race-based enforcement; however, accusations of widespread practices are unfounded. It is generally believed that whatever racial profiling that might have existed in the recent past (middle 1990s) was abated with the de-emphasis of "drug interdiction" patrols. Indeed, a vast majority, if not all, of the participants note that their decision to initiate a stop is a function of the "behavior of the vehicle." Thus, they believe that the issue of race-based decisions to initiate a stop is without merit. However, deployment and enforcement patterns are another issue. It appears that some deployment decisions are based on traffic demands (such as a road with a history of accidents or fatalities), and others are based on areas with a significant "opportunity" factor, for example, opportunities associated with density of bars. ("where the fishing is good"). Such decisions are more likely to target low-income people (therefore, disproportionately people of color) than their high-income counterparts. This is manifested with the presumption that they are less likely to challenge the

action in court and the higher income areas are involved in less overall criminality and disorder.

Overwhelmingly, participants report that they do not desire to search vehicles and will only do so as a last resort. This is primarily due to safety concerns. This is contrary to the public perception of excessive searching vehicles and harassment by troopers. Troopers gave the impression that they were professional and methodical in their job performance. Indeed, each noted that they would have decided what action they were going to take before the vehicle was stopped.

The question now is what can be done to eliminate whatever racial profiling might exist. Senate Bill 76 require the completion of “stop forms.” Such a requirement undoubtedly creates a sense of institutional consciousness about racial profiling. Accountability appears to be an active management concern. Supervisors use multiple strategies to monitor trooper activities, including: 1) periodic riding with a trooper, 2) review of citations, and 3) court visits to assess quality of charges. The latter management strategies, coupled with a more diverse NCSHP, are possible avenues to trooper accountability and behavior modification.

Appendix C: Citation Charge versus Reason for Stop

In the analysis of Chapters 2 and 3, we generally use what we call the “citation event data.” We defined a “citation event” as an event during which one or more citation forms are filled out. Each form can have one or two charges listed, but multiple forms can be filled out for the same event. During the year 2000, up to eighteen charges were filed for any given event. A driver could have received more than eighteen charges at an event—there is no limit to the number of charges a trooper may file relating to a single incident. However, no person in 2000 received more than eighteen charges at any single event.

When analyzing the charges, we generally assumed that the most salient offense (and the most serious charge) of the citation event was the first charge listed for the event. However, the most serious charge is not always the charge that initially triggers the stop. When we examined the data in further detail, we determined that, in most cases, the behavior that caused the stop and a subsequent citation was not necessarily the first charge listed on the citation forms. For example, we found records of drivers who were stopped for speeding and for having a revoked license. It is more likely that the driver was stopped for speeding and then the trooper discovered that the driver had a revoked license. Yet, the revoked license is typically listed as the first charge. Appendix C examines in detail the assumption that the first charge listed on the form indicates the behavior that was the likely reason for the stop. We will limit the analysis here to citations not issued at accidents (under the assumption that our main interest is in trooper-initiated stops resulting in citations).

Table C.1 lists all of the citation events for the year 2000 by type of charge. Type of charge consists of several hundred codes that have been collapsed into the eleven categories listed below. Appendix D lists the detailed codes and the collapsing rules used to combine them into eleven categories. The behaviors which constitute the first charges on the citation forms are listed down the left side of Table C.2. The behaviors that we, the researchers, believe are likely to have been the behaviors that triggered the stops are listed across the top of the table. The eleven categories across the top were arrived at by looking at combinations of charges for citation events with more than one charge. If there was more than one charge listed in addition to speeding, we reasoned that speeding would be the behavior most likely to initially trigger a stop—simply because speeders usually catch the attention of NCSHP troopers from a substantial distance (this is always the case when using radar, and typically at a distance of many yards—often several hundred yards). As can be seen, the vast majority of cases fall along the diagonal of the table, indicating that the first charge is most likely the behavior that triggered the stop.

Other behaviors, such as having an expired or revoked license, would not normally be known to the trooper until after he or she had pulled over a driver. Equipment violations might be visible from afar, but if a speeding charge is also listed, it is more likely that an equipment violation became apparent after the car had been pulled over. Essentially, we have imposed a hierarchy of charges as follows (from right to left in the table): speeding, unsafe movement, failure to stop/yield, equipment violations, seatbelt violations, miscellaneous traffic violations, DUI, license-registration-insurance-type violations, resisting/fleeing, stolen vehicle, and finally a criminal charge. That is, if we find that a citation event has multiple charges, we rank them according to this list. For example, if there are three charges, one for speeding, one for equipment violation and one for revoked license, we would assume speeding was the charge that brought

about the stop. In the absence of a speeding charge, we would assume it was the equipment violation.

The table can be read as follows. As we can see from the percentages (*the second number in each cell of the table*), in 97.6 percent of instances in which seatbelt violations are the most likely reason for pull-overs, a seatbelt charge is the first charge listed on the citation form. When we, the researchers, think that DUI triggered the stop, only 86 percent of citations list DUI as the first charge. Note that, for some serious charges—namely DUI and license-registration-insurance violations—where there is only that one serious charge listed on the form, we will never know what behavior brought about the stop. This is because the trooper decided to simply charge the driver with the most serious offense and drop the other charges. For example, a driver may have been pulled over for speeding and then the trooper discovered that the driver was uninsured. The driver might be cited for the lack of insurance, but not the speeding. We would have no record of the speeding, but only a record of the insurance violation. As for DUI, it is probable the driver was observed driving in an irregular way and was stopped and found to be DUI—no charges were filed for “weaving,” yet it is likely that the driver was stopped for some type of irregular vehicular movement.

We think that the “license-registration-insurance violations” charge is considered by troopers to be a serious offense, one that is subject to severe sanctions in the courts. In general, a clear pattern of these charges emerges on the forms on which other charges are also filed. Apparently, a driver will often be stopped for unsafe movement, failure to stop/or yield, or an equipment violation. The trooper then finds that the driver lacks a valid driver’s license, insurance, or registration, and as a result of that discovery, the trooper lists the latter offense as the first charge on the citation form. We assume that this occurs because such offenses are

considered by troopers to be more serious than the violations that likely caused the pull-overs, and so the license, insurance, or registration violation is written-up as the first charge. Other common offenses that follow this pattern of being listed as the first charge—but not necessarily being the triggering charge—are failing to use a seatbelt and DUI. In general it can be said that, even though the first charge is not always the likely reason for the stop, it is usually most serious of the charges.

The fact that the first charge is the more serious charge—and not necessarily the charge that initiated the stop—concerns us. We would like to know which specific behavior triggered the pull-over. Given the inadequacies of the stop data, as discussed elsewhere in this report, the citation data is the primary source of information about which behaviors seemingly trigger an NCSHP intervention. To look at this question further, we next examine the same relationship between the charge that we believe is the most likely behavior to trigger a stop and the first charge that the trooper listed. In Table C.2, however, we limit the analysis to only multiple charge incidents. Again, only citation events not involving an accident are included here, and only citations for African Americans and whites (all others excluded). The top row in each category is the count of the charge and the second row of the category is the percentage of the column total. For example, let us examine the charges of DUI. In the fifth column, in 76.0 percent of the multiple-charge citation events with DUI as the likely reason for the stop, DUI is listed as the first charge. Therefore, in 24.0 percent of the DUI stops resulting in a citation, the first charge is something else—almost always a license, registration or insurance violation. Assuming that DUI is a more serious charge than having an expired license, expired registration or even no insurance, one could say that the NCSHP is not consistent in writing up DUI as the first charge (although 76 percent of the time they write the first charge up as a DUI).

We summarize the findings in Table C.2 as follows: 1) In 92.5 percent of the speeding pull-overs, the first charge is also listed as speeding. In the 7.5 percent of the cases in which speeding is not listed as the first charge, the first charge is most often either a license-registration-insurance violation, or a DUI. Thus, the results follow a pattern of the more serious charge (DUI or license-registration-insurance violations) as the charge listed first. (We assume DUI and license-registration-insurance violations are generally more serious than speeding because the punishments are generally stiffer for these violations.) 2) The pattern of discrepancy between a first charge and a pull-over charge is similar for other types of violations as it is for speeding violations: usually DUI or license-registration-insurance violations appear first among the citations. For example, in 8.3 percent of the seatbelt pull-overs, a license-registration-insurance violation appears first (the most of any non-seatbelt violation). 3) Where there is a charge of “unsafe vehicular movement,” there is often a DUI violation as well, as one might reasonably expect.

To follow up on the analysis of the relationship between the unsafe movement charge and additional violations listed on the forms, we see that in 35.2 percent of the unsafe movement pull-overs the driver is cited for DUI as the first violation. In another 30.6 percent of cases in which unsafe movement appears to be the likely reason for the pull-over, there is also a license-registration-insurance violation. In 16.9 percent of these unsafe movement cases a seatbelt charge is listed first.

Table C.1 Likely Reason for Stop by First Charge

Likely Reason for the Pull-over by First Charge: All Non-Accident Citation Events												
First Charge		Crime	Stolen Vehicle	Resist, Flee, or Escape	License, Insurance or Registration	DUI	Misc. Traffic	Seat-belt	Equipment	Fail to Stop or Yield	Unsafe Movement	Speeding
Crime	#	3589	1	11	155	91	28	47	14	1	33	21
	%	100	4.5	7.1	0.2	0.5	1.2	0	0.4	0	0.2	0
Stolen Vehicle	#		21		2					1		1
	%		95.5		0					0		0
Resist, Flee, Escape	#			145	39	10	3	9	1	3	21	24
	%			92.9	0.1	0.1	0.1	0	0	0	0.1	0
License, Insurance or Registration	#				71879	2248	395	1933	694	1147	1672	1516
	%				99.7	13.4	17.4	1.7	19.1	10.7	11.5	0.5
DUI	#					14403	216	790	103	394	1910	1512
	%					86	9.5	0.7	2.8	3.7	13.2	0.5
Misc. Traffic	#						1634	4	40		24	10
	%						71.8	0	1.1		0.2	0
Seatbelt	#							112695	278	839	752	839
	%							97.6	7.7	7.8	5.2	0.3
Equipment	#								2502	1	16	9
	%								68.9	0	0.1	0
Fail Stop or Yield	#									8360	54	40
	%									77.8	0.4	0
Unsafe Movement	#										10010	61
	%										69.1	0
Speeding	#											291744
	%											98.6

Table C.2 First Charge versus Likely Behavior for Pull-over, Multiple Charge Events Only

Likely Reason for the Pull-over by First Charge: All Non-Accident Citation Events												
First Charge		Crime	Stolen Vehicle	Resist, Flee, or Escape	License, Insur. Registration	DUI	Misc. Traffic	Seatbelt	Equipment	Fail to Stop or Yield	Unsafe Movement	Speeding
Crime	#	538	1	10	126	78	19	41	11	1	26	19
	%	100	25	52.6	0.9	1.1	3.2	0.2	1.1	0	0.6	0
Stolen Vehicle	#		3		2					1		1
	%		75		0					0		0
Resist, Flee, Escape	#			9	27	7	3	8	1	3	21	21
	%			47.4	0.2	0.1	0.5	0	0.1	0.1	0.5	0
License, Insurance or Registration	#				14625	1548	309	1562	526	811	1230	1220
	%				99	22.8	52.6	8.3	51	35.6	30.6	2.7
DUI	#					5163	164	631	85	323	1416	1257
	%					76	27.9	3.1	8.2	14.2	35.2	2.8
Misc. Traffic	#						93	2	37		22	9
	%						15.8	0	3.6		0.5	0
Seatbelt	#							17930	226	764	678	754
	%							88.5	21.9	33.5	16.9	1.7
Equipment	#								145	1	16	9
	%								14.1	0.1	0.4	0
Fail Stop or Yield	#									377	52	33
	%									16.5	1.3	0.1
Unsafe Movement	#										561	55
	%										13.9	0.1
Speeding	#											41442
	%											92.5

Therefore, in the majority of multiple charge citation events involving unsafe movement, the driver is charged first with a violation other than unsafe movement.

This issue of unsafe movement charges is of special interest in this study because it might be argued that “unsafe vehicular movement” is rather subjectively and haphazardly judged by a trooper on the highway. For example, how much “side to side” motion of a vehicle is necessary to have unsafe movement? The present data suggest to us that unsafe vehicular movement (in a multiple charge situation) is often accompanied by other violations of a more serious nature. Not only are they more serious, but they are not as subjective as connoted in the term “unsafe movement.” In the particular cases of license-registration-insurance violations and DUI, the drivers are almost always guilty of the violation—either the driver has a problem with his or her license-registration-insurance or he or she does not, and if the driver is charged with DUI he or she probably failed a breathalyzer test. Subsequently, there is little subjectivity involved in determining whether or not the driver was guilty of these other charges listed first in the multiple charge citation events. Therefore, if the initial pull-over for “unsafe movement” was pre-textual by the NCSHP (a pretext for conducting a vehicular search for drugs), the associated charges would in most instances cast doubt upon that interpretation, primarily because the drivers have often been found to be unambiguously guilty of something more serious than the subjective unsafe movement. In Chapter 4, we show that so few vehicular searches are conducted by the ordinary NCSHP troopers that it would seem unlikely that stops for unsafe movement or any other behavior would often be a pretext for a drug search (a somewhat different interpretation is given to the NCSHP Criminal Interdiction Team’s procedures and searches—see also Chapter 4).

Above, we showed that the first charge and the pull-over charge are not often the same charge. Where there is a difference between the pull-over charge and the first charge, more often than not, the first charge seems to be a more serious charge than the pull-over charge. A more

important question for the current research, however, is whether or not there are any racial differences in the type of charges appearing among the first charge versus the pull-over charge. Table C.3 shows that there are some racial differences for a variety of pull-over charges as they differ from three first charges: license-registration-insurance violations, DUI, and seatbelt violations. (Too few cases of other offense categories occur to warrant including them in the table and we drop crime charges, stolen vehicle, and resist/flee violations completely as they involve too few observations to be of concern.)

In Table C.3, we can read the table as follows: in multiple charge citation events, if an African American and a white are pulled over for a seatbelt violation, the African American is found to have a license-registration-insurance violation 7.9 percent of the time, compared to 8.3 percent of the time for a white driver. In general, the results of an analysis of Table C.3 suggest that license-registration-insurance violations are more prevalent among African American drivers involved in multiple citation events than they are among white drivers. This is not surprising since African American drivers are more often cited for license-registration-insurance violations than are whites for one-charge citation events.

Here, however, if the first charge were assumed to be the charge that initiated the vehicle pull-over, more error is introduced for African American drivers than for white drivers. Use of first charge as a proxy for pulled charge would result in disproportionately more African Americans having license-registration-insurance as the violation that caused the pull-over, rather than the behavior that is most likely to have caused it: DUI, miscellaneous traffic violations, equipment violations, failure to stop, unsafe movement, and speeding (seatbelts are the one exception to the pattern where a slightly higher percentage of whites stopped for seatbelt violations have license-registration-insurance violations (8.3 percent) than have African

Americans (7.9 percent). Note, however, that although the percentage differences appear to be somewhat large (for example, 28.2 versus 19.9 of the DUI multiple charge events—an 8.3 percent difference), these represent a much smaller percentage of the all the cases (including the single charge events). Thus, for example, if one added up all of the African Americans who were issued a first charge of a license-registration-insurance violation and who were also likely to have been pulled over for another charge of DUI or other miscellaneous violation (such as a traffic violation, an equipment violation, or failure to stop/yield violation) they would constitute about 1 percent of all African American citation events (1,249 of 122,649 African American citations).

Because only a very small bias would be involved in using first charge rather than the charged behavior that we believe is most likely to have triggered the stop, we use first charge in the analysis of Chapter 2 and 3. (For much of this analysis there is no distinction by type of charge and so the issues discussed in this appendix are moot). Although there may be a slight bias in our analysis by not using the charge we think brought about the stop, it is in a known direction: more African Americans will appear to have been stopped for license-registration-insurance violations than actually were. One should bear in mind, however, that even though we could conceivably use our hierarchically derived charge that resulted in the stop as the charge for analytic purposes in Chapters 2 and 3, the 1 percent of African American citation events involved (where there is a difference) is much smaller than the 17.3 percent of all African American citations in which license-registration-insurance violations are involved. We suspect that, for most of these 17.3 percent of cases, some behavior triggered the stop, but we do not

have any record of that behavior. In effect, the 1 percent of cases that we could “correct” by using our estimate of the charge that initiated the stop would be a very small correction indeed.

Table C.3 Racial Differences in First Charge Relative to Likely Reason for the Vehicular Pull-over (Non-Accident Citations, African Americans and Whites Only) (Column Percentages)

First Charge	DUI		Misc. Traffic		Seatbelt		Equipment		Fail to Stop		Unsafe Move.		Speeding	
	AA	White	AA	White	AA	White	AA	White	AA	White	AA	White	AA	White
License/Reg	28.2	19.9	56.6	50.1	7.9	8.3	55.9	49.4	48.3	30.3	41.1	26.1	3.4	2.4
DUI	71.2	78.5	27.9	27.9	3.1	3.1	8.8	8.1	14.8	13.9	31.4	36.8	3	2.7
Seatbelt	--	--	--	--	88.9	88.3	20.3	22.5	22.3	38.1	13.6	18.3	1.4	1.8

Example: “Of multiple charge citation events in which DUI was the initial reason for the pull-over, African Americans are more likely to have a license-registration-insurance violation listed as the first charge than are whites(28.2 percent versus 19.9 percent).”

Appendix D: Offense Codes for Citation Analysis

(See recoded variable at end of Appendix D for the hierarchy of offenses used to define the offense likely to have caused the stop).

920	14-18	VOLUNTARY MANSLAUGHTER	F	A		
922	14-18	INVOLUNTARY MANSLAUGHTER	F	A		
930	14-17	MURDER	F	A		
935	14-17	FIRST DEGREE MURDER	F	A		
940	14-17	SECOND DEGREE MURDER	F	A		
945	COMMON LAW	SOLICITATION TO COMMIT MURDER	F	O		
	941003					
950	COMMON LAW	ATTEMPTED MURDER	F	A	910408	
955	14-18.1	SOLICITATION TO COMMIT MURDER	F	O	941003961202	
999		HOMICIDE - FREE TEXT	A			
1020	14-39	KIDNAPPING	F	A		
1022	14-43	ABDUCTION OF MARRIED WOMAN	F	O		941003
1023	14-41	ABDUCTION OF CHILDREN	F	A		
1024	14-42	CONSPIRING TO ABDUCT CHILDREN	F	A		
1026	14-39	FIRST DEGREE KIDNAPPING	F	A		
1028	14-39	SECOND DEGREE KIDNAPPING	F	A		
1030	14-43.3	FELONIOUS RESTRAINT	F	A		
1032	COMMON LAW	ATTEMPTED KIDNAPPING	F	A	930423	
1040	COMMON LAW	FALSE IMPRISONMENT	M	A	940606	
1099		KIDNAPPING - FREE TEXT	A			
1103	14-27.2(A)	FIRST DEGREE RAPE	F	A		
1116	14-27.4(A)(1)	FIRST DEGREE SEX OFFENSE CHILD	F	A		
1118	14-202.1	INDECENT LIBERTIES WITH CHILD	F	A		
1120	14-27.2(A)(1)	FIRST DEGREE RAPE CHILD	F	A		
1122	14-27.3(A)	SECOND DEGREE RAPE	F	A		
1124	14-27.5(A)	SECOND DEGREE SEXUAL OFFENSE	F	A		
1125	14-27.6	ATTEMPT 1ST DEGREE RAPE	F	O	950818	
1126	14-27.6	ATTEMPT 1ST DEGREE SEX OFFENSE	F	O	950818	
1128	14-27.6	ATTEMPT SECOND DEGREE RAPE	F	O	950818	
1130	14-27.6	ATTEMPT 2ND DEGREE SEX OFFENSE	O		950818	
1132	14-27.4(A)	FIRST DEGREE SEXUAL OFFENSE	F	A		
1134	14-27.7	SEX OFFENSE - PARENTAL ROLE	F	A		
1136	14-27.7	SEX OFFENSE INSTITUTION	F	A	910408	
1137	14-27.7A(A)	STAT RAPE/SEX OFFN DEF >=6YR	F	A	951201	
1138	14-27.4	STATUTORY SEXUAL OFFENSE	F	O	940606	
1139	14-27.7A(B)	STAT RAPE/SEX OFFN DEF >4-<6YR	F	A	951201	
1140	14-27.7	ATT SEX OFFENSE-PARENTAL ROLE	F	A	910408	
1142	14-27.2	ATTEMPT 1ST DEGREE RAPE	F	A	950818	
1144	14-27.4	ATTEMPT 1ST DEGREE SEX OFFENSE	F	A	950818	

1146	14-27.3	ATTEMPT SECOND DEGREE RAPE	F	A	950818
1148	14-27.5	ATTEMPT 2ND DEGREE SEX OFFENSE	F	A	950818
1160	14-208.11	FAIL REGISTER SEX OFFENDER(M)	M	O	951201 971201
1161	14-208.11	FAIL REGISTER SEX OFFENDER(F)	F	A	951201
1199		SEXUAL ASSAULT - FREE TEXT	A		
1202	14-87	ATT ROBBERY-DANGEROUS WEAPON	F	A	
1220	14-87.1	COMMON LAW ROBBERY	F	A	
1222	14-87	ROBBERY WITH DANGEROUS WEAPON	F	A	
1224	14-89.1	SAFECRACKING	F	A	
1226	14-87.1	ATTEMPTED COMMON LAW ROBBERY	F	A	
1228	COMMON LAW	CONSP ARMED ROBBERY BUS/PERS	F	A	
1299		ROBBERY - FREE TEXT	A		
1301	14-32.1(F)	ASSAULT HANDICAPPED PERSON	M	A	
1302	14-32.1(B)	AWDWIKISI HANDICAPPED PERSON	F	O	941003
1303	14-32.1(C)	AWDWISI ON HANDICAPPED PERSON	F	O	941003
1304	14-32.1(D)	AWDWIK ON HANDICAPPED PERSON	F	O	941003
1305	14-32.1(E)	FELONY ASSAULT ON HANDICAPPED	F	A	
1306	14-32.2(A)	PATIENT ABUSE AND NEGLECT	F	A	
1320	14-33(B)(1)	ASSAULT ATTEMPT SERIOUS INJURY	M	O	N99 951201
1321	14-33(B)(1)	ASSAULT - SERIOUS INJURY	M	O	910408 940608
1322	14-34	ASSAULT BY POINTING A GUN	M	A	
1324	14-31	MALICIOUS ASSAULT IN SECRET	F	A	
1325	14-32.4	ASSAULT INFLICT SERIOUS INJ(F)	F	A	970101
1326	14-33(B)(1)	ASSAULT INFLICT SERIOUS INJURY	M	O	951201
1327	14-33(B)(1)	ASSAULT - DEADLY WEAPON	M	O	910408940606
1328	14-33(B)(3)	ASSAULT ON A CHILD UNDER 12	M	O	951201
1330	14-33(B)(2)	ASSAULT ON A FEMALE	M	O	951201
1332	14-288.9	ASSAULT ON EMERGENCY PERSONNEL	M	A	
1333	14-33(B)(7)	ASSAULT ON DSS WORKER	M	O	910408 911001
1334	14-288.9	AWDW EMERGENCY PERSON	F	A	
1336	14-33(A)	ASSAULT AND BATTERY	M	A	
1338	14-33(B)(4)	ASSAULT ON LAW OFFICER	M	O	911001
1340	14-33(B)(1)	ASSAULT WITH A DEADLY WEAPON	M	O	951201
1342	14-34.2	AWDW ON OFFICER	F	O	951201
1344	14-32(C)	AWDW INTENT TO KILL	F	A	
1346	14-32(B)	AWDW SERIOUS INJURY	F	A	
1348	14-32(A)	AWDWIKISI	F	A	
1352	14-33(B)(5)	ASSAULT ON COURT OFFICER	M	O	911001
1353	14-33(B)(6)	ASSAULT ON SCHOOL TEACHER	M	O	911001
1354	14-33	ASSAULT/AFFRAY	M	O	910408
1355	14-33(B)(8)	ASSAULT ON A GOVT OFFICIAL	M	O	911001 951201
1356	14-34.2	AWDW GOVERNMENT OFFICIAL	F	A	911001
1357	14-33(A)	SIMPLE AFFRAY	M	A	921027
1358	14-33(B)(9)	ASSAULT - SPORTS OFFICIAL	M	A	931201

1360	14-28	MALICIOUS CASTRATION	F	A		
1362	14-29	CASTRATION WITHOUT MALICE	F	A		
1364	14-29	MAIMING WITHOUT MALICE	F	A		
1366	14-30	MALICIOUS MAIMING	F	A		
1368	14-33(A)	SIMPLE ASSAULT	M	A		
1370	14-30.1	THROWING ACID OR ALKALI	F	A		
1371	113-290.1	NEGLIGENT HUNTING	M	O	911001	940606
1372	14-34.4(A)	ADULTERATED OR MISBRANDED FOOD		F	A	
1374	14-34.4(B)	ADLTRT OR MISBRAND TO EXTORT	F	A		
1380	14-32.3(A)	ABUSE DISABLE/ELDER SER INJ	F	A		951201
1381	14-32.3(A)	ABUSE DISABLE/ELDER WITH INJ	F	A		951201
1382	14-32.3(B)	NEGLECT DISABLE/ELDER SER INJ	F	A		951201
1383	14-32.3(B)	NEGLECT DISABLE/ELDER WITH INJ	F	A		951201
1384	14-32.3(C)	EXPLOIT DISABLE/ELDER(F)	F	A		951201
1385	14-32.3(C)	EXPLOIT DISABLE/ELDER(M)	M	A		951201
1386	14-33(C)(1)	ASSAULT ATTEMPT SERIOUS INJ(M)	M	A		951201
1387	14-33(C)(1)	ASSAULT INFLICT SERIOUS INJ(M)	M	A		951201
1388	14-33(C)(1)	ASSAULT WITH A DEADLY WEAPON	M	A		951201
1389	14-33(C)(2)	ASSAULT ON A FEMALE	M	A		951201
1390	14-33(C)(3)	ASSAULT ON A CHILD UNDER 12	M	A		951201
1391	14-33(C)(4)	ASSAULT GOVT OFFICIAL/EMPLY	M	A		951201
1392	14-33(C)(5)	ASSAULT SCHOOL BUS PERSONNEL	M	A		951201
1393	14-33.2	HABITUAL MISDEMEANOR ASSAULT	F	A		951201
1394	14-34.5	ASSAULT LEO/PO/OTHER W FIREARM	F	A		971201
1395	14-34.6(A)	ASSAULT EMERGENCY PRSNL(M)	M	A		951201
1396	14-34.6(B)	ASSAULT EMERG PRSNL IBI/WDW	F	A		951201
1397	14-34.6(C)	ASSAULT EMERG PRSNL FIREARM	F	A		951201
1398	14-34.7	ASSAULT LEO/PO/OTHER SER INJRY	F	A		961202
1399		ASSAULT - FREE TEXT		A		
2020	14-58	ARSON	F	A		
2021	14-60	BURNING OF A SCHOOL HOUSE	F	A		910610
2022	14-62.1	BURNING BLDG UNDER CONSTRUCT	F	A		
2023	14-62	BURNING UNOCCUPIED BLDG	F	A		
2024	14-67	ATTEMPT TO BURN BLDG OR BOAT	F	A		
2025	14-62	BURN OCCUPIED BUILDING	F	O		931101
2026	14-65	FRAUDULENTLY BURNING DWELLING	F	A		
2027	14-58	FIRST DEGREE ARSON	F	A		940606
2028	14-58	SECOND DEGREE ARSON	F	A		940606
2030	14-66	BURNING PERSONAL PROPERTY	F	A		951201
2032	14-62.2	BURN CHURCH/RELIGIOUS BLDG	F	A		961202
2099		ARSON - FREE TEXT		A		
2110	14-118.4	EXTORTION	F	A		
2112	14-118	BLACKMAILING	M	A		
2116	14-113.13	UNLAWFUL OBTAINING CREDIT CARDF		A		940606
2199		EXTORT - FREE TEXT		A		

2206	14-55	POSSESSION OF BURGLARY TOOLS	F	A		
2210	14-54	BREAKING/ENTERING AND LARCENY	F	O		
2211	14-72(B)	BREAKING/ENTERING AND LARCENY		F	O	
2212	14-54(A)	BREAKING AND OR ENTERING (F)	F	A		
2214	14-54(B)	BREAKING OR ENTERING (M)	M	A		
2215	14-54(A)	ATTEMPT TO BREAK OR ENTER (F)	FO940606			910408
2216	14-56	BREAK OR ENTER A MOTOR VEHICLE	F	A		
2217	14-56.1	BREAK COIN/CURRENCY MACH (F)	F	A		921120
2218	14-56.1	BREAK COIN/CURRENCY MACH (M)	M	A		
2219	14-56.2	DAMAGE COIN/CURRENCY MACHINE	M	A		
2220	14-56	BREAK/ENTER RAILROAD CAR	F	A		
2221	14-56	ATTEMPT BREAK/ENTER MOTOR VEH	M	A		930308
2222	14-56	BREAK/ENTER TRAILER/AIRCRAFT	F	A		
2223	14-56	BREAK/ENTER BOAT	F	A		
2224	14-57	BURGLARY WITH EXPLOSIVES	F	A		
2226	14-51	FIRST DEGREE BURGLARY	F	A		
2228	14-51	SECOND DEGREE BURGLARY	F	A		
2230	14-54(B)	WRONGFULLY BREAK/ENTER A BLDGM		O		910408
2232	14-54(A)	ATTEMPT TO BREAK AND ENTER (M)	M O	940606		910408
2240	14-51	ATTEMPT FIRST DEGREE BURGLARY	F	A		940606
2299		BURGLARY - FREE TEXT		A		
2301	14-168.1	FELONY CONVERSION	F	A		
2302	14-168.1	MISDEMEANOR CONVERSION	M	A		
2320	14-72(A)	FEL LARCENY - >\$400	F	O		911001
2321	14-72(A)	FELONY LARCENY	F	A		911001
2322	14-72(A)	MISDEMEANOR LARCENY	M	A		
2323	14-87	AID AND ABET ARMED ROBBERY	F	A		910408
2324	14-72	LARCENY BY TRICK	F	O		910408
2325	14-72(B)	LARCENY OF A FIREARM	F	A		910408
2326	14-72(A)	LARCENY BY TRICK	M	O		940606
2328	14-72.1(D)	LARCENY - REMARKING GOODS	M	O		940606
2330	14-72.1(D)	LARCENY BY CHANGING PRICE TAG	M	A		
2331	14-75	LARCENY OF CHOSE IN ACTION	F	A		910408
2332	14-72.1(D)	LARCENY - TAG TRANSFER	M	O		940606
2333	14-72.1(D1)	LARCENY BY ANTI-INVNTY DEVICE	F	A		971201
2334	14-74	LARCENY BY EMPLOYEE	F	A		
2335	14-74	"LARCENY BY EMPL >=\$100,000"	F	A		971201
2336	14-72(B)(1)	LARCENY FROM THE PERSON	F	A		
2339	14-71	RECEIVING STOLEN GOODS (M)	M	A		
2340	14-71	RECEIVING STOLEN GOODS (F)	F	A		
2341	14-71.1	POSSESSION OF STOLEN GOODS (F)	F	A		

2342	14-72.1	SHOPLIFTING CONCEALMENT GOODS	M	A	
2343	14-71.1	POSSESSION OF STOLEN GOODS (M)	M	A	
2344	14-81	"LARCENY OF HORSE, SWINE, ETC."	M	O	910408
2345	14-81(A)	LARCENY OF HORSE/SWINE/CATTLE	F	A	910408
2346	14-76	LARCENY OF PUBLIC RECORD	M	A	
2348	14-75.1	LARCENY OF SECRET PROCESS	F	A	
2350	14-77	LARCENY OF WILL	M	A	
2351	14-82	TAKE HORSE/MULE/DOG TEMP PURP	M	A	
2352	14-84	LARCENY OF DOG	M	O	910408
2353	14-72	ATTEMPTED LARCENY	M	A	
2354	14-118.5	THEFT OF CABLE TV SERVICE	M	A	
2356	14-72(B)(2)	LARCENY AFTER BREAK/ENTER	F	A	
2358	14-72.4(A)	UNAUTH TAKE/SALE DAIRY CASE	M	A	
2360	14-81(A1)	LARCENY OF DOG	F	A	910408
2363	14-79.1	LARCENY OF PINE STRAW	F	A	971201
2390	14-72.2	UNAUTHORIZED USE OF MOTOR VEH	M	A	940606
2391	14-72(A)	LARCENY OF MOTOR VEHICLE (F)	F	A	940606
2392	14-72(A)	LARCENY OF MOTOR VEHICLE (M)	M	A	940606
2399		LARCENY - FREE TEXT		A	
2404	14-72.2	UNAUTHORIZED USE OF MOTOR VEH	M	O	940606
2406	14-72(A)	LARCENY OF MOTOR VEHICLE	F	O	940606
2408	14-72(A)	LARCENY OF MOTOR VEHICLE	M	O	910408 940606
2499		STOLEN VEHICLE - FREE TEXT		O	940606
2503	14-13	COUNTERFEITING COIN	F	A	
2505	14-13	UTTERING COUNTERFEIT COIN	F	A	
2508	14-14	POSSESS COUNTERFEITING TOOLS	M	O	941003
2510	COMMON LAW	COMMON LAW FORGERY	M	A	940606
2520	14-119	FORGERY OF INSTRUMENT	F	A	
2522	14-119	FORGERY AND UTTERING	F	O	
2524	14-120	UTTERING FORGED INSTRUMENT	F	A	
2526	14-120	ATTEMPTED UTTERING	F	O	910408
2527	14-120	UTTERING FORGED ENDORSEMENT	F	A	910408
2528	14-120	FORGERY OF ENDORSEMENT	F	A	910408
2540	14-122	FORGERY OF DEEDS OR WILLS	F	A	
2542	COMMON LAW	FORGERY	F	O	930322 940606
2599		FORGERY - FREE TEXT		A	
2601	108A-53.1(A)	BUY/SELL/DISTRIB FOOD STAMPS	F	A	971201
2602	108A-53.1(B)	ILLEG POSS/USE FOOD STAMPS(M)	M	A	971201
2603	58-2-161	INSURANCE FRAUD	F	A	930208
2604	108A-53.1(B)	ILLEG POSS/USE FOOD STAMPS(F)	F	A	971201
2605	14-113.13	FINANCIAL CARD FRAUD (M)	M	A	
2606	14-107	WORTHLESS CHECK	M	O	910415

2607	96-18(B)	ESC LAW FRAUD VIOL	M	O		920203
2608	96-18	GIVING FALSE INFO TO ESC	M	O		920203
2609	96-18(B)	NO REPORT TO ESC	M	O		920203
2610	14-107(3)	WORTHLESS CHECK NO ACCOUNT	M		A	
2611	14-106	OBTAIN PROPERTY WORTHLESS CHK	M		O	
2612	14-113.9	FINANCIAL CARD THEFT	F		A	
2613	108A-53	FOOD STAMP FRAUD (M)	M		A	
2614	14-113.11	FINANCIAL CARD FORGERY	F		A	
2615	108A-53	FOOD STAMP FRAUD (F)	F		A	
2616	14-113.13	FINANCIAL CARD FRAUD (F)	F		A	
2617	108A-39(A)	PUBLIC ASSISTANCE FRAUD (M)	M		A	
2618	14-110	DEFRAUDING INNKEEPER	M		A	
2619	108A-39(B)	PUBLIC ASSISTANCE FRAUD (F)	F		A	
2620	14-168	HIRING WITH INTENT TO DEFRAUD	M		A	
2621	108A-63	MEDICAL PROVIDER FRAUD	F		A	910408
2622	14-151.1	INTERFERE WITH UTILITY METER	M		A	
2623	14-155	UNLAWFUL TELEPHONE TAP	M		A	
2624	14-151	DIVERTING UTILITY USE	M		A	
2625	14-113.1	FALSE TELEPHONE CREDIT	M		A	
2626	14-104	FAIL TO WORK AFTER PAID	M		A	
2627	14-113.1	CREDIT VIO W/O AUTH OF ISSUEE	M		O	940606
2628	14-186	FALSE HOTEL REGISTRATION	M		A	
2629	14-113.1	CREDIT VIO W/O AUTH OF ISSUEE	F		O	940606
2630	14-113.1	CREDIT VIOL AFTER CREDIT REV	M		O	940606
2632	14-100	OBTAIN PROPERTY FALSE PRETENSE	F		A	
2633	14-100	"OBT PROP FALSE PRET >=\$100,000"	F		A	971201
2634	20-30(5)	FRAUDULENT LICENSE PERMIT	T		O	940606
2636	14-114	FRAUD DISPOSAL MORTGAGE PROP	M		A	
2638	14-113.13	UNLAWFUL OBTAINING CREDIT CARD			FO	940606
2639	14-113.13	UNLAWFULLY OBTAIN CREDIT CARD			M	O
						940606
2640	14-221.1	ALTER/STEAL/DEST CRIMINAL EVID	F		A	
2641	14-221.2	ALTER COURT DOCUMENTS	F		A	
2643	14-454(B)	ACCESSING COMPUTERS (M)	M		A	
2644	14-117	FALSE ADVERTISING	M		A	
2645	14-455(A)	DAMAGING COMPUTERS(F)	F		A	
2646	14-167	FAIL TO RETURN RENTAL PROPERTY	M		A	
2647	14-455(B)	DAMAGING COMPUTERS(M)	M		A	
2648	14-214	FRAUDULENT INSURANCE CLAIM	F		O	930208
2649	14-168.4	FAIL RETN PROP RENTD PUR OPT	M		A	910408

2650	14-106	OBTAIN PROPERTY WORTHLESS CHK	M	A		
2651	14-454(A)	ACCESSING COMPUTERS (F)	F	A		
2654	14-115	SECRETING LIEN PROPERTY	M	A		
2655	14-107(4)	WORTHLESS CHECK CLOSED ACCOUNT		M	A	
2656	14-107	WORTHLESS CHK 4TH SUB OFFENSE	M	A		
2657	90-108(A)	OBTAIN CS BY FRAUD (M)	M	A		
2658	90-108(A)	OBTAIN CS BY FRAUD (F)	F	A		
2659	18B-302(E)(3)	OBT/ATT OBT ALC OTHERS ID	M	A		60992
2660	18B-302(E)(2)	OBT/ATT OBT ALC FALSE ID	M	A		
2661	18B-302(E)(3)	OBT/ATT OBT ALC OTHER DL	M	A		920609
2662	18B-302(E)(1)	OBT/ATT OBT ALC FALSE DL	M	A		
2663	96-18(A)	MISREP TO OBTAIN ESC BENEFIT	M	A		920203
2664	96-18(B)	MISREP TO PREVENT ESC BENEFIT	M	A		920203
2665	96-18(C)	EMPL SEC LAW VIOLATION	M	A		920203
2666	14-107	SIMPLE WORTHLESS CHECK	M	A		910415
2670	14-107	FELONY WORTHLESS CHECK	F	A		911001
2680	53-276	CHECK CASHING WITHOUT LICENSE	F	A		971201
2699		FRAUD - FREE TEXT	A			
2704	14-91	EMBEZZLEMENT OF STATE PROPERTY	F	A		
2705	14-91	"EMBEZ STATE PROP >=\$100,000"	F	A		971201
2710	14-90	EMBEZZLEMENT BY FIDUCIARY	F	O		940606
2714	14-94	EMBEZZLEMENT RAILROAD OFFICER	F	A		
2715	14-94	"EMBEZZLEMENT RR OFF >=\$100,000"	F	A		971201
2718	14-90	EMBEZZLEMENT	F	A		
2719	14-90	"EMBEZZLEMENT >=\$100,000"	F	A		971201
2720	14-96	EMBEZZLEMENT - INSURANCE AGENT	F	O		940606
2722	14-92	EMBEZZLEMENT-PUB OFF/TRUSTEES	F	A		850701
2723	14-92	"EMBEZ PUB OFF/TRST >=\$100,000"	F	A		971201
2799		EMBEZZLE - FREE TEXT	A			
2908	69-33	NEGLIGENT AND CARELESS BURNING	M	O		940606
2910	14-66	BURNING PERSONAL PROPERTY	F	O		940606
2912	14-160	INJURY TO PERSONAL PROPERTY	M	A		
2914	20-107	TAMPERING WITH VEHICLE PARTS	T	O		940606
2916	14-132(A)	DEFACING PUBLIC BUILDING	M	A		
2919	14-140.1	BURN WITHOUT WATCHMAN	M	A		951020
2920	14-127	INJURY TO REAL PROPERTY	M	A		
2922	20-107(B)	TAMPERING WITH VEHICLE STEAL	T	O		940606

2924	20-107	TAMPERING WITH VEHICLE	T	O		940606
2926	14-160	DAMAGE TO PERSONAL PROPERTY	M	O		940606
2928	14-160.1(A)	ALTER/REMOVE NMV SERIAL NUMBER		M	A	
2930	14-160.1(B)	POSS/SELL/BUY ALT NMV SER NO	M	A		
2940	14-138	FAIL TO EXTINGUISH FIRE	M	O		941003950606
2941	14-138.1	FAIL TO EXTINGUISH FIRE	M	A		950717
2999		DAMAGE PROPERTY - FREE TEXT		A		
3401	90-113.22	POSSESS DRUG PARAPHERNALIA	M	A		
3405	90-95(E)(8)	POSS CS W/IN 300 FT OF SCHOOL	F	A		910408
3409	90-95(E)(9)	POSS CS PRISON/JAIL PREMISES	F	A		971201
3410	90-95(E)(8)	S/D CS W/IN 300 FT OF SCHOOL	F	A		910408
3411	90-95(H)(3A)	TRAFFICKING IN AMPHETAMINE	F	A		960501
3412	90-95(H)(3B)	TRAFFICKING IN METHAMPHETAMINE	F	A		960501
3413	90-95(H)(4A)	TRAFFICKING IN LSD	F	A		960501
3414	90-95(I)	CONSPIRE TRAFFIC AMPHETAMINE	F	A		960501
3415	90-95(I)	CONSPIRE TRAFFIC METHAMPHETAMINE	F	A		960501
3416	90-95(I)	CONSPIRE TRAFFIC LSD	F	A		960501
3420	90-112	SEIZURE/FORFEITURE OF VEHICLE		F	O	910408 950206
3423	90-95.4(A)(1)	CS HIRE/USE MINOR>13 DEF 18<21	F	A		990127
3424	90-95.4(A)(2)	CS HIRE/USE MINOR<=13 DEF18<21	F	A		990127
3425	90-95.4(B)(1)	CS HIRE/USE MINOR >13 DEF >=21	F	A		990127
3426	90-95.4(B)(2)	CS HIRE/USE MINOR<=13 DEF>=21	F	A		990127
3428	90-95.6	PROMOTE DRUG SALES BY A MINOR	F	A		991027
3429	90-95.7	PARTICIP IN DRUG VIOL BY MINOR	F	A		990127
3430	90-108(A)(14)	EMBEZZLE CS BY EMPLOYEE OF REG FA		F	A	910408
3440	90-95(A)(1)	SELL MARIJUANA	F	A		971201
3441	90-95(A)(1)	SELL COCAINE	F	A		971201
3442	90-95(A)(1)	SELL HEROIN	F	A		971201
3443	90-95(A)(1)	SELL LSD	F	A		971201
3444	90-95(A)(1)	SELL SCH I CS	F	A		971201
3445	90-95(A)(1)	SELL SCH II CS	F	A		971201
3446	90-95(A)(1)	SELL SCH III CS	F	A		971201
3447	90-95(A)(1)	SELL SCH IV CS	F	A		971201
3448	90-95(A)(1)	SELL SCH V CS	F	A		971201
3449	90-95(A)(1)	SELL SCH VI CS	F	A		971201
3455	90-95(A)(1)	DELIVER MARIJUANA	F	A		971201
3456	90-95(A)(1)	DELIVER COCAINE	F	A		971201
3457	90-95(A)(1)	DELIVER HEROIN	F	A		971201
3458	90-95(A)(1)	DELIVER LSD	F	A		971201
3459	90-95(A)(1)	DELIVER SCH I CS	F	A		971201
3460	90-95(A)(1)	DELIVER SCH II CS	F	A		971201
3461	90-95(A)(1)	DELIVER SCH III CS	F	A		971201
3462	90-95(A)(1)	DELIVER SCH IV CS	F	A		971201
3463	90-95(A)(1)	DELIVER SCH V CS	F	A		971201
3464	90-95(A)(1)	DELIVER SCH VI CS	F	A		971201
3470	90-95(D)(4)	POSS MARIJ >1/2 TO 1 1/2 OZ	M	A		930208

3475	90-95(E)(5)	SELL OR DELIV CS MINOR >13-<16	F	A	990127
3476	90-95(E)(5)	SELL OR DELIV CS MINOR <=13	F	A	990127
3480	90-98	CONSPIRE SELL MARIJ	F	A	971201
3481	90-98	CONSPIRE SELL COCAINE	F	A	971201
3482	90-98	CONSPIRE SELL HEROIN	F	A	971201
3483	90-98	CONSPIRE SELL LSD	F	A	971201
3484	90-98	CONSPIRE SELL SCH I CS	F	A	971201
3485	90-98	CONSPIRE SELL SCH VI CS	F	A	971201
3486	14-401.15	CONTAMINATE FOOD/DRINK W/CS	F	A	971201
3490	90-98	CONSPIRE DELIVER MARIJ	F	A	971201
3491	90-98	CONSPIRE DELIVER COCAINE	F	A	971201
3492	90-98	CONSPIRE DELIVER HEROIN	F	A	971201
3493	90-98	CONSPIRE DELIVER LSD	F	A	971201
3494	90-98	CONSPIRE DELIVER SCH I CS	F	A	971201
3495	90-98	CONSPIRE DELIVER SCH VI CS	F	A	971201
3500	90-113.22	POSS DRUG PARAPHERNALIA	M	O	910408
3501	90-95(A)(1)	MANUFACTURE SCH I CS	F	A	
3502	90-95(A)(1)	MANUFACTURE SCH II CS	F	A	
3503	90-95(A)(1)	MANUFACTURE SCH III CS	F	A	
3504	90-95(A)(1)	MANUFACTURE SCH IV CS	F	A	
3505	90-95(A)(1)	MANUFACTURE SCH V CS	F	A	
3506	90-95(A)(1)	MANUFACTURE SCH VI CS	F	A	
3507	90-95(A)(2)	CREATE COUNTERFEIT CS	F	A	
3508	90-95(A)(1)	SELL OR DELIVER SCH I CS	F	O	971201
3509	90-95(A)(1)	SELL OR DELIVER SCH II CS	F	O	971201
3510	90-95(A)(1)	SELL OR DELIVER SCH III CS	F	O	971201
3511	90-95(A)(1)	SELL OR DELIVER SCH IV CS	F	O	971201
3512	90-95(A)(1)	SELL OR DELIVER SCH V CS	F	O	971201
3513	90-95(A)(1)	SELL OR DELIVER SCH VI CS	F	O	971201
3514	90-95(A)(2)	SELL OR DELIVER COUNTERFEIT CS	F	A	
3515	90-95(A)(1)	PWIMSD SCH I CS	F	A	
3516	90-95(A)(1)	PWIMSD SCH II CS	F	A	
3517	90-95(A)(1)	PWIMSD SCH III CS	F	A	
3518	90-95(A)(1)	PWIMSD SCH IV CS	F	A	
3519	90-95(A)(1)	PWIMSD SCH V CS	F	A	
3520	90-95(A)(1)	PWIMSD SCH VI CS	F	A	
3521	90-95(A)(2)	PWICSD COUNTERFEIT CS	F	A	
3522	90-95(A)(3)	POSSESS SCH I CS	F	A	
3523	90-95(A)(3)	POSSESS SCH II CS	F	A	
3524	90-95(A)(3)	POSSESS SCH III CS	F	A	
3525	90-95(A)(3)	POSSESS SCH IV CS	F	A	
3526	90-95(A)(3)	POSSESS SCH V CS	F	A	
3527	90-95(D)(4)	FELONY POSSESSION SCH VI CS	F	A	
3528	90-95(H)(1)	TRAFFICKING IN MARIJUANA	F	A	
3529	90-95(H)(2)	TRAFFICKING IN METHAQUALONE	F	A	
3530	90-95(H)(3)	TRAFFICKING IN COCAINE	F	A	

3531	90-95(H)(4)	"TRAFFICKING, OPIUM OR HEROIN"	F	A	
3532	90-95(I)	CONSPIRE TO TRAFFIC IN MARIJ	F	A	
3533	90-95(I)	CONSPIRE TRAFFIC METHAQUALONE	F	A	A
3534	90-95(I)	CONSPIRE TO TRAFFIC IN COCAINE	F	A	
3535	90-95(I)	CONSPIRE TRAFFIC OPIUM/HEROIN	F	A	
3536	90-95(D)(2)	SIMPLE POSSESSION SCH II CS	M	A	
3537	90-95(D)(2)	SIMPLE POSSESSION SCH III CS	M	A	
3538	90-95(D)(2)	SIMPLE POSSESSION SCH IV CS	M	A	
3539	90-95(D)(3)	SIMPLE POSSESSION SCH V CS	M	A	
3540	90-95(D)(4)	MISD POSSESSION SCH VI CS	M	A	
3541	90-95(A)(1)	MANUFACTURE MARIJUANA	F	A	
3542	90-95(A)(1)	SELL OR DELIVER MARIJUANA	F	O	971201
3543	90-95(A)	SELL MARIJUANA	F	O	971201
3544	90-95(A)	PWISD MARIJUANA	F	A	950206
3545	90-95(A)(1)	PWIMSD MARIJUANA	F	A	
3546	90-95(A)	P/W/I/M MARIJUANA	F	O	950206
3547	90-95(A)	P/W/I/S MARIJUANA	F	O	950206
3548	90-95(A)	P/W/I/D MARIJUANA	F	O	950206
3549	90-95(D)(4)	FELONY POSSESSION MARIJUANA	F	A	
3550	90-95(D)(4)	POSSESS MARIJUANA UP TO 1/2 OZ	M	A	
3551	90-98	CONSPIRE SELL OR DELIVER MARIJ	F	O	971201
3552	90-95(A)(1)	MANUFACTURE COCAINE	F	A	
3553	90-95(A)(1)	SELL OR DELIVER COCAINE	F	O	971201
3554	90-95(A)	SELL COCAINE	F	O	950206
3555	90-95(A)	PWISD COCAINE	F	A	950206
3556	90-95(A)(1)	PWIMSD COCAINE	F	A	
3557	90-95(A)	P/W/I/M COCAINE	F	O	950206
3558	90-95(A)	P/W/I/S COCAINE	F	O	950206
3559	90-95(A)	P/W/I/D COCAINE	F	O	950206
3560	90-95(D)(2)	FELONY POSSESSION OF COCAINE	F	A	
3561	90-95(A)	SIMPLE POSS COCAINE	M	O	920224
3562	90-98	CONSP SELL OR DELIVER COCAINE	F	O	971201
3563	90-95(A)(1)	SELL OR DELIVER HEROIN	F	O	971201
3564	90-95(A)	SELL HEROIN	F	O	950206
3565	90-95(A)(1)	PWIMSD HEROIN	F	A	
3566	90-95(A)	P/W/I/S HEROIN	F	O	950206
3567	90-95(A)	P/W/I/D HEROIN	F	O	950206
3568	90-95(D)(1)	POSSESS HEROIN	F	A	
3569	90-98	CONSP SELL OR DELIVER HEROIN	F	O	971201
3570	90-95(A)	SELL/DELIV HASHISH	F	O	950206
3571	90-95(A)	SELL HASHISH	F	O	950206
3572	90-95(A)	P/W/I/S/D HASHISH	F	O	950206
3573	90-95(A)	P/W/I/S HASHISH	F	O	950206
3574	90-95(A)	P/W/I/D HASHISH	F	O	950206
3575	90-95(A)	POSSESS HASHISH	F	O	950206

3576	90-95(A)	SIMPLE POS HASHISH	M	O		950206
3577	90-98	CONSP SELL OR DELIV SCH VI CS	F	O		971201
3578	90-95(A)(1)	SELL OR DELIVER LSD	F	O		971201
3579	90-95(D)(1)	POSSESSION OF LSD	F	A		
3580	90-98	CONSPIRE SELL OR DELIVER LSD	F	O		971201
3581	90-95(A)	POSS METHAQUALONE/QUAALUDE	F	O		950206
3582	90-95(A)	POSS METHAQUALONE/QUAALUDE	M	O		941003
3583	90-95(A)	P/W/S/D METHAQUALONE/QUAALUDE	F	A		
3584	90-98	CONSP SELL OR DELIVER SCH I CS	F	O		971201
3585	90-95(D)	"SIM POS-CS-SCH II, III, IV"	M	O		950206
3586	90-113.10	INHALE TOXIC VAPORS	M	A		
3587	90-113.12	SELL TOXIC VAPORS SUBSTANCE	M	A		
3588	90-113.11	POSS TOXIC VAPORS SUBSTANCE	M	A		
3589	90-95	S/D OF CS	F	O		941003
3590	90-95(D)	SIM POS - CS - SCH IV	M	O		941003
3591	90-95(A)	MFG/CREATE CS	F	O		941003
3592	90-95(D)	CS - SCH VI	M	O		941003
3593	90-95(A)	CS - P/W/I/S/M ETC	F	O		941003
3594	90-95(E)(5)	SELL OR DELIV CS TO CHILD < 16	F	O		990127
3595	90-95(D)	"SIM POS-CS-SCH II, III, IV"	F	O		950206
3596	90-95(D)	SIM POS-CS-SCH VI	F	O		950206
3597	14-258.1(A)	PROVIDING DRUGS TO INMATE	F	A		910408
3598	90-95(A)	GROWING MARIJUANA	F	O		950206
3599		DANGEROUS DRUGS - FREE TEXT		A		
3604	14-179	MISDEMEANOR INCEST	M	A		
3605	14-190.9	INDECENT EXPOSURE	M	A		
3607	14-178	FELONY INCEST	F	A		
3608	14-190.1	MISD DISSEMINATE OBSCENITY	M	O	940606	941003
3609	14-190.1	FELONY DISSEMINATE OBSCENITY	F	A		940606
3620	14-177	CRIME AGAINST NATURE	F	A		
3622	14-190.9	USE OF PREMISES INDEC EXPOSURE	M	A		
3624	14-184	FORNICATION AND ADULTERY	M	A		
3626	14-186	OCCUPY ROOM IMMORAL PURPOSES	M	O		941003
3628	14-177	ATT CRIME AGAINST NATURE (F)	F	A		
3630	14-177	ATT CRIME AGAINST NATURE (M)	M	A		941003
3699		SEX OFFENSE - FREE TEXT		A		
3705	14-190.1	DISSEMINATION OF OBSCENITY	M	O		940606
3706	14-190.1	DISSEMINATION OF OBSCENITY	F	O		940606
3799		OBSCENITY - FREE TEXT	O		940606	
3802	14-318.4	FELONY CHILD ABUSE	F	O		970108
3804	14-183	BIGAMY	F	A		
3805	14-316.1	CONTRIBUTING DEL OF JUVENILE	M	A		

3807	14-313(C)	PURCHASE CIGARETTES < 18	M	A	971201
3808	14-313	SALE OF CIGARETTES TO MINORS	M	A	940606
3809	14-313(B)	NO SIGN FOR TOBACCO SALES < 18	I	A	971201
3810	49-2	IV-D BASTARDY	M	O	910408 910429
3812	49-2	NON IV-D BASTARDY	M	O	910408 910429
3818	14-322.1	ABANDON/NON-SUPPORT OF CHILD	M	O	
3820	14-325	INADEQ SUPPORT OF FAMILY(R/83)	M	O	
3822	115C-378	SCHOOL ATTENDANCE LAW VIOL	M	A	
3824	49-2	ILLEGITIMATE CHILD/NON-SUPPORT	M	O	
3826	14-322	ABANDON DEPENDENT SPOUSE	M	O	941003
3827	14-322(C)	NON IV-D NONSUPPORT SPOUSE	M	A	
3828	14-322	NON-SUPPORT DEP SPOUSE/CHILD	M	O	
3829	14-322(D)	NON-SUPPORT OF CHILD	M	O	
3830	14-46	CONCEALING BIRTH OF A CHILD	F	A	
3834	14-318.2	MISDEMEANOR CHILD ABUSE	M	A	
3835	14-318.4(A)	FELONY CHILD ABUSE-SERIOUS INJ	F	A	970109
3836	14-318.4(A1)	FELONY CHILD ABUSE-PROSTITUTN	F	A	970109
3837	14-318.4(A2)	FELONY CHILD ABUSE -SEXUAL ACT	F	A	970109
3840	14-320.1	FELONY CUSTODY ORDER VIOLATIONF		A	920203
3858	14-322.1	NON IV-D ABANDON FOR 6 MONTHS	F	A	910408
3859	14-322.1	IV-D ABANDONMENT FOR 6 MONTHS	F	A	910408
3860	14-322.1	IV-D ABANDON/NON-SUPP OF CHILD	M	O	940606
3861	49-2	IV-D NONSUPPORT ILLEGIT CHILD	M	A	
3862	14-322	IV-D NON-SUPP DEP SPOUSE/CHILD	M	O	940606
3863	14-322(D)	IV-D NONSUPPORT CHILD	M	A	
3864	14-322(B)	IV-D NONSUPPORT SPOUSE	M	A	940606
3865	14-322.1	NON-IV-D ABAND/NON-SUPP CHILD	M	O	940606
3866	49-2	NON IV-D NONSUPP ILLEGIT CHILD	M	A	
3867	14-322	NON-IV-D NON SUPP SPOUSE/CHILD	M	O	940606
3868	14-322(D)	NON IV-D NONSUPPORT OF CHILD	M	A	
3872	50B-4.1	VIOLATE DOM VIOL PROTCT ORDER	M	A	971201
3899		FAMILY - FREE TEXT	A		
3901	14-291	SELLING LOTTERY TICKETS	M	A	
3903	14-301	OPERATE/POSSESS SLOT MACHINE	M	A	910408
3904	14-302	OPER/POSSESS GAMBLING DEVICES	M	A	
3905	14-304	MANUFACTURE/SELL SLOT MACHINEMA			910408
3906	14-305	SLOT MACHINE AGREEMENT	M	A	910408
3915	14-290	OPERATING A LOTTERY	M	A	
3930	14-291.1	POSSESSION OF LOTTERY TICKETS	M	A	
3931	14-292	GAMBLING	M	A	
3932	14-293	ALLOW GAMBLING IN PUBLIC HOUSE		M	A

3933	14-297	ALLOW GAMING TABLES	M	A	910408
3999		GAMBLING - FREE TEXT		A	
4010	14-204	AID AND ABET PROSTITUTION	M	A	
4012	14-204(7)	PROSTITUTION	M	A	
4013	14-204(5)	SOLICIT FOR PROSTITUTION	M	A	
4014	14-204.1	LOITERING FOR PROSTITUTION		M	A
4015	14-204(1)	MAINT PLACE FOR PROSTITUTION		M	A
4016	14-204(4)	TRANSPORT FOR PROSTITUTION		M	A
4018	14-3	SOLICIT CRIME AGAINST NATURE	M	A	
4099		COMMERCIAL SEX - FREE TEXT		A	
4101	18B-307(B)	MANUFACTURE LIQUOR NO PERMIT	M	A	
4102	18B-304	"LIQUOR, ILLEGAL SALE/POSSESS."	M	O	
		940606			
4103	18B-401(A)	UNSEALED WINE/LIQ IN PASS AREA	M	A	
4104	LOCAL ORDINANC	POSSESS LIQUOR FOR SALE		M	A
4105	18B-401(A)	TRANS ALC/CONTAINER NOT MANU	T	O	
		910408			
4106	18B-406	ILLEGAL TRANSPORT ALCOHOL BEV	M	A	
4107	18B-301(F)(7)	POSS/CONS BEER/WINE UNAUT PREM	M	A	
4108	18B-301(F)(4)	POS/CON F-WN/LQ/MXBV UNATH PR	M	A	
4109	18B-305	SELL/GIVE ALC TO INTOX PERSON	M	A	
4110	LOCAL ORDINANC	ALLOW ILLEGAL CONSUMPTION ALC		M	A
4111	18B-302(B)(1)	PUR MTBV/U-WN BY 19/20	I	A	
4112	18B-111	POSS/TRAN/SELL NON-TAX ALC BEV	M	O	920609
4113	18B-302(B)(1)	ATT PUR MTBV/U-WN BY 19/20	I	A	
4114	18B-302(B)(1)	POSS/MTBV/U-WN BY 19/20	I	A	
4120	18B-302(C)	AID UNDERAGE PURCHASE LIQUOR	M	O	950206
4121	18B-302(C)	AID UNDERAGE PURCHASE BEER	M	O	950206
4122	18B-302(C)(1)	AID UNDERAGE PUR ALC BY < 21M	A	A	950206
4123	18B-302(C)(2)	AID UNDERAGE PUR ALC BY > 21M	A	A	950206
4132	LOCAL ORDINANC	POSS/CONS BEER/WINE PUBLIC ST		M	
		A			
4134	LOCAL ORDINANC	POSS/CON BEER/WINE UNAUTH PREMM		A	
		A			
4136	18B-301(F)(2)	DISPLAY ALC ATHLETIC CONTEST	M	A	
4138	LOCAL ORDINANC	POSSESSION ALCOHOLIC BEVERAGE		M	
		A			
4140	18B-302	PUR/POSS BEER/WINE UNDERAGE	M	O	920609
4142	18B-302	PUR/POSS ALCOHOL UNDERAGE	M	O	920609

4144	14-329	MANUFACTURE POISONOUS LIQUOR	F	A	
4146	14-313	SALE OF CIGARETTES TO MINORS	M	O	940606
4148	18B-102	ABC LAW VIOLATION	M	A	
4149	18B-102.1	SHIP ALCOHOL FROM OUT STATE	F	A	971201
4150	18B-302(A)	SELLING BEER/WINE TO MINOR	M	O	920609
4152	18B-302(A)	SELLING LIQUOR TO UNDERAGE	M	O	920609
4155	18B-304	POSS/SELL ALC BEV NO PERMIT	M	A	
4156	18B-301(F)	CON/OFFER WINE - PUB RD/BYWAY	M	O	940606
4157	18B-301(F)(1)	CON/OFFER ALC BEV PUBLIC ROAD	M	A	
4158	18B-300(B)	CON MTBV/U-WN PREM NO PERMIT	M	A	
4159	18B-301(F)	PUBLIC CONSUMPTION	M	A	
4160	18B-1004	SELL/CONS BEER/WINE/ALC AFT HR	M	O	920609
4161	18B-1004	SELL/CON ALC BEV AFTER HOURS	M	A	
4162	18B-302(A)	SELL/GIVE MALT /WINE TO MINOR	M	O	920609
4163	18B-302(A)	SELL/GIVE ALCOHOL BEV TO MINOR	M	O	920609
4164	LOCAL ORDINANC	CONSUME BEER/WINE UNDERAGE		M	
	A				
4166	18B-302(B)(1)	PUR/ATT MTBV/U-WN NOT 19/20		MA	60992
4167	18B-302(B)(1)	POSS MTBV/U-WN NOT 19/20	M	A	60992
4168	18B-302(B)(2)	PUR/ATT F-WN/LQ/MXBV < 21	M	A	920609
4169	18B-302(B)(2)	POSS F-WN/LQ/MXBV < 21	M	A	920609
4170	18B-302(A)(1)	SELL/GIVE MTBV/U-WN TO < 21M		A	60992
4171	18B-302(A)(2)	SELL/GIVE F-WN/LQ/MXBV TO < 21		MA	60992
4172	18B-300(B)	ALLOW CON MTBV/U-WN NO PERMIT	M	A	60992
4173	18B-111	POSS/SELL NONTAXPAID ALC BEV	M	A	920609
4174	18B-111	TRANSPORT NONTAXPAID ALC BEV	M	A	920609
4175	18B-401(A)	DRINK BEER/WINE WHILE DRIVING	M	A	940606
4195	14-444	INTOXICATED AND DISRUPTIVE	M	A	940606
4199		LIQUOR - FREE TEXT		A	
4210	14-444	INTOXICATED AND DISRUPTIVE	M	O	940606
4299		PUBLIC INTOXICATION - FREE TEX		O	941003
4401	20-141(H)	IMPEDE TRAFFIC BY SLOW SPEED	I	A	
4402	20-149	OVERTAKEN VEH INC SPEED(I) I		A	
4403	20-149(A)	IMPROPER PASSING ON RIGHT	I	A	
4404	20-125	HORN AND WARNING DEVICE VIO	I	A	
4405	20-117	FLAG LIGHT END OF LOAD VIOL	I	A	
4406	20-157(C)	OBSTRUCTING FIRE OPERATIONS	I	A	
4407	20-126	MIRROR VIOLATION	I	A	
4408	20-157(B)	FOLLOWING A FIRE TRUCK	I	A	

4409	20-175	SOLICITING FROM HIGHWAY	I	A	
4410	20-140.3	INTERSTATE HIGHWAY VIOLATION	I		A
4411	20-160	SAFETY ZONE/SIDEWALKS VIOL	I		A
4412	20-146(A)	DRIVE LEFT OF CENTER	I	A	
4413	20-123	IMPROPER TOWING	I	A	
4414	20-137.1	NO CHILD RESTRAINT SYSTEM	I		A
4415	20-116	OVER LOAD SIZE/LENGTH/ VEHICLE	I		A
4416	20-154	IMPROPER SIGNAL	I	O	910408
4417	20-146	CROSSING MEDIAN	I	O	910408
4418	20-123.2	IMPROPER EQUIP - SPEEDOMETER	I		A 910408
4419	20-141(C)	SPEED LESS THAN POSTED MINIMUM	I		A
4420	20-115.1	TWIN/SEMI TRAILER VIOL(I)	I	A	920203
4421	20-122	TIRE RESTRICTIONS EQUIP VIOL	I		A 920203
4422	20-129(C)	MOTORCYCLE FAIL BURN HEADLAMP			I A
4423	20-123.1	IMPROPER STEERING MECHANISM	I		A 920203
4424	20-129(D)	MOTORCYCLE FAIL BURN TAILLIGHT			I A
4425	20-116(G)	IMPROPER LOADING/COVERING VEH	I		A
4426	20-125.1	DIRECTIONAL SIGNALS EQUIP VIOL	I		A 920203
4427	20-129(D)	REAR LAMPS VIOLATION	I	A	
4428	20-127(B)	WINDSHIELD WIPER EQUIP VIOLI		O	920203951020
4429	20-129(G)	BRAKE/STOP LIGHT EQUIP VIOL	I		A 920203
4430	20-140.4(A)	MOTORCYCLE/MOPED HELMET VIOL	I		A
4431	20-156(A)	FAIL TO YIELD FROM PRIVATE DRV	I		A
4432	20-155(A)	FAILURE TO YIELD	I	A	
4433	20-146(C)	DRIVE WRONG WAY ON DUAL LANE	I		A
4434	20-155(B)	FAIL TO YIELD LEFT TURN	I	A	
4435	20-129.1	ADDITIONAL LIGHTING EQUIP VIOL	I		A920203
4436	20-130.3	WHITE LIGHT REAR-DRIVE FORWARD			IA 920203
4437	20-146(D)	DESIGNATED LANE VIOLATION	I		A 920203
4438	20-158	TRAFFIC CONTROL DEVICE VIOL	I		A 920203
4439	20-154(A)	IMPROPER BACKING	I	A	920203
4440	20-183.2	INSPECTION VIOLATION	I	A	
4441	20-157(D)	DRIVE OVER FIRE HOSE OR EQUIP	I		A 920203
4442	20-183.3	INSP STICKER NO INSPECTION	I		A 920203
4443	20-183.6	ALTERED INSPECTION STICKER	I		A 920203
4444	20-140.2	OVERLOADED/OVERCROWDED VEHICLE			I A
4445	20-129(A)	FAIL TO BURN HEADLAMPS	I	A	
4446	20-129(A)(4)	NO HEADLIGHTS ON WIPERS ON	I		A 910408
4447	20-153	IMPROPER TURN	I	A	
4448	20-158	RED LIGHT VIOLATION	I	O	910408
4449	20-141(B)	EXCEEDING POSTED SPEED	I	A	
4450	20-141(B)(G)	SPEEDING	I	A	
4451	20-153(C)	IMPROPER USE OF TRAFFIC LANE	I		A

4452	20-158(B)(1)(3)	FAIL YLD STOPSIGN/FLSH RED LGT	IA	910408
4453	20-158.1	FAILURE TO YIELD - YIELD SIGN	I A	910408
4454	20-158(B)(1)(3)	FAIL STOP STOPSIGN/FLSH RED LT	I A	
4455	20-158(B)(2)	FAIL TO STOP-STEADY RED LIGHT	I A	910408
4456	20-141	TOO FAST FOR CONDITIONS	I O	910408
4457	20-127	OBSTRUCTED WINDSHIELD/WINDOWSIO		951020
4458	20-154	UNSAFE MOVEMENT	I A	
4459	20-154	FAIL TO SIGNAL WHEN TURNING	I O	910408
4460	20-141(M)	FAILURE TO REDUCE SPEED	I A	910408
4461	20-142.5	STOP WHERE TRAFFIC OBSTRUCTED	I A	951201
4462	20-122.1	UNSAFE TIRES	I A	
4463	20-150(A)	UNSAFE PASSING ONCOMING TRAF	I A	910408
4464	20-150(B)	UNSAFE PASSING CREST OR CURVE	I A	910408
4465	20-150(C)	UNSAFE PASSING RR OR INTERSECT	I A	910408
4466	20-165.1	DRIVE WRONG WAY-ONE WAY ST/RD	I A	
4467	20-141(A)	EXCEEDING SAFE SPEED	I A	
4468	20-141.1	SPEEDING IN SCHOOL ZONE	I A	
4469	20-150(E)	UNSAFE PASSING YELLOW LINE	I A	910408
4470	20-135.2A	FAIL TO WEAR SEAT BELT-DRIVER	I A	910408
4471	20-135.2(A)	FAIL TO WEAR SEAT BELT	I O	910408
4472	20-135.2A	FAIL TO SECURE PASSENGER UNDER 16	I A	
4473	20-135.2(A)	FAIL WEAR SEAT BELT- PASSENGER	I A	910408
4474	20-141(E1)	SPEED ON SCHOOL PROPERTY ORD	I A	971201
4475	20-141(J2)	SPEED IN HIGHWAY WORK ZONE	I A	921001
4476	20-142	FAIL TO STOP FOR RR WARNING	I O	941003
4477	20-181	FAIL TO DIM HEADLAMPS	I A	
4478	20-141(A)	SPEED FASTER THAN REASONABLE	I O	910408
4479	20-142.1	FAIL TO OBEY RR SIGNAL	I A	941003
4480	20-173	FAIL PEDESTRIAN RIGHT OF WAY	I A	
4481	20-134	NO LIGHTS ON PARKED VEHICLE	I A	
4482	20-152(A)	FOLLOWING TOO CLOSELY	I A	
4483	20-162.1	ILLEGAL PARKING	I A	
4484	20-150	IMPROPER PASSING	I A	
4485	20-135.2B	TRANSPORT CHILD OPEN CARGO BED	IA	950818
4486	20-128	IMPROPER MUFFLER	I A	
4488	20-124	IMPROPER BRAKES	I A	
4489	20-183.8	FICT/OTH IMPROPER INSPECTION	I A	920817
4490	20-127(A)	WINDSHIELD WIPER EQUIP VIOL	I A	951020
4492	20-129(B)	DRIVE WITHOUT TWO HEADLAMPS	I A	
4494	20-163	LEAVE VEH UNATTENDED/UNSECURE	I A	
4495	20-162	PARK FIRE HYD/STATION/PRIV DR	I A	

4496	20-162.1	OVERTIME PARKING	I	A		
4497	20-37.6(E)	HANDICAPPED PARKING VIOLATION	I	A		
4498	20-127(C)(D)	DARKENED WINDSHIELD/WINDOWS	I	O		951020
4499	20	INFRACTION - FREE TEXT	I	A		
4531	20-11(L)	LIC/PERMIT SEAT BELT VIOL <18	I	A		971201
4532	20-11(L)	LIC/PERMIT SEATING VIOL <18	I	A		971201
4535	20-11(L)	LIC/PRMT VIOL OTH RSTRCTN <18	I	A		971201
4801	14-223	RESIST/OBSTRUCT PUBLIC OFFICER	M	O		940606
4818	14-288.5	FAILURE TO DISPERSE	M	O		940606
4820	20-114.1	FAIL TO OBEY TRAFFIC OFFICER	T	O		940606
4822	14-277	IMPERSONATION- PEACE OFFICERS	M	O		940606
4899		OBSTRUCT POLICE - FREE TEXT		O		940606
4904	14-267	HARBORING FUGITIVE	M	A		
4905	14-259	FELONY HARBORING ESCAPEE	F	A		
4910	14-255	ESCAPE BY HIRED PRISONER	M	A		
4912	14-256	ESCAPE FROM LOCAL JAIL	M	O		940606
4914	148-45(B)	ESCAPE FROM STATE PRISON (F)	F	A		
4916	148-45(A)	ESCAPE FROM STATE PRISON (M)	M	A		
4918	15A-722	FUGITIVE	F	O		920203
4920	14-266	PERSUADING INMATES TO ESCAPE	M	A		
4922	14-256	MISDEMEANOR ESCAPE LOCAL JAIL	M	A		
4924	14-256	FELONY ESCAPE LOCAL JAIL	F	A		
4926	14-256.1	ESCAPE PRIVATE CORRECTION FAC	F	A		990127
4999		FLIGHT/ESCAPE - FREE TEXT		A		
5000	75D-7	RICO PERJURY	F	A		940606
5001	15A-727	GOVERNOR S WARRANT	F	O		920203
5003	14-209	PERJURY	F	A		
5004	14-210	SUBORNATION OF PERJURY	F	A		
5006	14-218	OFFERING BRIBES	F	A		940606
5008	14-217	RECEIVING BRIBES	F	A		940606
5010	COMMON LAW	OBSTRUCTING JUSTICE	M	A		930426
5020	15A-543(B)	FAILURE TO APPEAR ON FELONY	F	A		
5022	15A-543(C)	FAILURE TO APPEAR ON MISD	M	A		
5023	14-225.2	HARASSMENT OF JUROR	F	A		910408
5024	14-226	INTIMIDATION OF A WITNESS	M	O		950206
5025	14-226	INTIMIDATING WITNESS	F	A		950206
5026	5A-11	VIOLATION OF COURT ORDER	M	A		
5028	5A-11	CRIMINAL CONTEMPT	M	A		
5029	5A-11(A)(9A)	CONTEMPT BY PROBATIONER	M	A		940509
5030	15A-1345	MISDEMEANOR PROBATION VIOL	M	A		
5032	15A-1345	FELONY PROBATION VIOLATION	F	A		
5034	5A-15	SHOW CAUSE	M	A		
5036	5A-11	BILL OF PARTICULARS	M	O		940606
5038	15A-1345	MISD PROB VIOL OUT OF COUNTY	M	A		

5040	15A-1345	FEL PROB VIOL OUT OF COUNTY	F	A	
5042	15A-1347	PROBATION REVOCATION APPEAL	M	A	
5044	15A-1347	PROBATION REVOCATION APPEAL	M	O	940606
5046	15A-951	MOTIONS	M	A	
5048	15A-1344.1(D)	MOTION TO WITHHOLD WAGESM		O	940606
5099		PUBLIC ORDER-FREE TEXT		A	
5102	14-218	BRIBERY - OFFERING	F	O	940606
5103	14-217	BRIBERY - RECEIVING	F	O	940606
5199		BRIBERY - FREE TEXT		O	940606
5201	14-316	PERMIT CHILDREN USE FIREARMS	M	A	
5202	14-269(A)	CARRYING CONCEALED WEAPON	M	A	
5203	14-269.2	POSS WEAPON ON SCHOOL GROUNDSM		O	931201
5204	14-269.7(A)	POSSESS HANDGUN BY MINOR	M	A	930927
5205	14-315(A)	SELL/GIVE WEAPON TO MINOR (M)	M	A	961202
5206	14-315(A1)	SELL/GIVE HANDGUN TO MINOR	F	A	970101
5208	14-49	MALICIOUS USE OF EXPLOSIVE	F	A	
5209	14-49(B1)	USE EXPLOSIVE DEVICE CHURCH	F	A	961202
5210	14-269.8	PURCH FIREARM VIOL DOM ORDER	F	A	951020
5213	LOCAL ORDINANC	SHOOTING WITHIN CITY LIMITSMO			940606
5220	14-34.1	DISCHARGE WEAPON OCCUPIED PROP		F	A
5221	COMMON LAW	GO ARMED TO TERROR OF PEOPLE		M	A
5222	14-409.9	POSSESSION OF MACHINE GUN (M)	M	O	951201
5223	14-409	POSSESSION OF MACHINE GUN (F)	F	A	941003
5224	14-415.1	POSSESSION OF FIREARM BY FELON	F	A	
5225	14-258.2	POSSESS WEAPON BY PRISONER (M)	M	A	
5226	14-402	SELL/PURCHASE WEAPON NO PERMIT		M	A
5227	14-258.2	POSSESS WEAPON BY PRISONER (F)	F	A	941003
5228	14-280	SHOOTING/THROWING AT TRAIN (M)	M	A	
5229	14-280	SHOOTING/THROWING AT TRAIN (F)	F	A	941003
5230	14-288.7	DEADLY WEAPON OFF PREMISES	M	A	
5232	14-288.8	POSSESS WEAPON MASS DESTRUCT	F	A	
5234	14-269.2	WEAPONS ON EDUC PROP/AID (F)	F	A	931201
5235	14-269.2	WEAPONS ON EDUC PROP/AID (M)	M	A	931201
5240	14-269(A1)	CARRYING CONCEALED GUN(M)	M	A	951201
5242	14-269(A1)	CARRYING CONCEALED GUN(F)	F	A	951201
5244	14-415.21(A)	CONCEAL HANDGUN PERMIT VIOL(I)	I	A	951201
5246	14-415.21(B)	CONCEAL HANDGUN PERMIT VIOL(M)	M	A	951201
5299		WEAPON OFFENSE - FREE TEXT		A	
5301	14-288.2	FELONY INCITING TO RIOT	F	A	941003
5302	14-288.2	RIOT - INCITING	M	O	941003
5303	14-288.6(A)	TRESPASS DURING EMERGENCY (M)	M	A	
5304	14-288.6(B)	TRESPASS DURING EMERGENCY (F)	F	A	
5308	14-286	FALSE FIRE ALARM	M	A	

5309	14-196(A)(3)	HARASSING PHONE CALL	M	A		
5310	14-223	RESISTING PUBLIC OFFICER	M	A	940606	
5312	14-288.5	FAIL TO DISPERSE ON COMMAND	M	A	940606	
5314	14-277	IMPERSONATE LAW ENFORCEMENT (M)		MA	940606	
5315	14-277(A)(4)	OPERATE VEH WITH BLUE LIGHT	F	A	961202	
5316	14-277(B)(5)	BLUE LIGHT CAUSE STOP/YIELD	F	A	961202	
5320	LOCAL ORDINANC	BARKING DOG	M	O	941003	
5321	67-12	DOG RUN AT LARGE AT NIGHT	M	A	941003	
5322	14-196	INDECENT LANGUAGE ON TELEPHONE				911003
5325	LOCAL ORDINANC	BLOCKING FIRE EXIT	T	O	941003	
5328	14-277.1	COMMUNICATING THREATS	M	A		
5329	14-277.3	MISDEMEANOR STALKING	M	A	921005	
5330	14-288.4	DISORDERLY CONDUCT	M	A		
5332	14-275.1	DISORDERLY CONDUCT AT TERMINAL				M A
5333	14-401.14(A)	ETHNIC INTIMIDATION	M	A	930927	
5334	14-401.14(B)	TEACHING ETHNIC INTIMIDATION	M	A	930927	
5335	14-277.4	OBSTRUCT HEALTH CARE FACILITY	M	A	930927	
5336	20-157	OBSTRUCTING FIRE OPERATIONS	T	O		
5338	14-196(A)(2)	THREATENING PHONE CALL	M	A		
5339	14-277.3	FELONY STALKING	F	A	921027	
5341	136-26	DRIVE ON CLOSED/UNOPENED HWY	M	A	920203	
5342	136-91	PUT INJURIOUS OBJECT IN ROAD	M	A		
5344	136-72	EXCEED BRIDGE LOAD LIMIT	M	A		
5345	14-69.1	FALSE BOMB REPORT	F	A	971201	
5346	14-69.1	FALSE BOMB REPORT	M	O	971201	
5347	14-69.2	HOAX BY FALSE BOMB	F	A	971201	
5348	14-35	HAZING	M	A		
5350	14-188	KEEPING A DISORDERLY HOUSE				M A
5352	14-401.8	REFUSE RELEASE LINE EMERGENCY	M	A		
5354	14-416	HANDLING DANGEROUS REPTILES	M	A		
5356	14-197	PROFANE LANGUAGE ON HIGHWAY	M	A		
5358	62A-12	MISUSE OF 911 SYSTEM	M	A		
5360	14-286.1	FALSE AMBULANCE REQUEST	M	A		
5362	14-225	FALSE REPORT TO POLICE STATION	M	A		
5364	14-288.2	PUBLIC DISTURBANCE	M	A		
5370	14-399(E)	COMMERCIAL LITTERING	F	A	911003	
5371	14-399(E)	LITTERING HAZARDOUS WASTE		F A	941003	
5372	14-399(C)	LITTERING NOT > 15 LBS	M	A	941003	
5373	14-399(D)	LITTERING 15 - 500 LBS	M	A	941003	

5374	14-399(E)	LITTERING > 500 LBS	F	A	941003		
5399		PUBLIC PEACE - FREE TEXT		A			
5400	20-30(1)	POSS/DISP ALT/FICT/REVD DR LIC	T	A	910408		
5401	20-30(1)	KNOW FICT/CANC/REV/SUSP LIC	T	O	941003		
5402	20-141(J)	SPEED TO ELUDE ARREST	T	O	971201		
5403	20-139(A)	DUI - DRUGS (REPEALED /83)	T	O	910408		
5404	20-138(A)	DUI - ALCHOLIC BEVERAGE (R/83)	T	O	900628		
5405	20-138.1	DRIVING WHILE IMPAIRED	T	A			
5406	20-141.4(A1)	FELONY DEATH BY VEHICLE	F	A			
5407	20-57(C)	NO REGISTRATION CARD	T	A			
5408	20-174.1	IMPEDE TRAFFIC SIT/STAND/LIE	T	A			
5409	20-30(2)	ALLOW USE OF LICENSE OR PERMIT	T	A			
5410	20-32	ALLOW UNLICENSE MINOR TO DRIVE	T	A			
5411	20-30(3)	DISPLAY/USE ANOTHER LICENSE	T	O	910408		
5412	20-146	DRIVE LEFT OF CENTER	T	O			
5413	20-140(C)	RECKLESS DRIVING AFT ALC(R/83)	T	O	900628		
5414	20-137.1	NO CHILD RESTRAINT SYSTEM	T	O			
5415	20-116	OVER LOAD SIZE/LENGTH/ VEHICLE	T	O			
5416	20-119	DOT SPECIAL PERMIT VIOLATION	T	A			
5417	20-106	POSSESS STOLEN AUTOMOBILE	F	A			
5418	20-28(A)	DWLR	T	A			
5419	20-141(C)	SPEED LESS THAN POSTED MINIMUM	T	O			
5420	20-59	FAIL TO SURRENDER TITLE	T	A			
5421	20-7.1	FAIL TO NOTIFY DMV ADDR CHANGE	T	A			
5422	20-129(C)	MOTORCYCLE FAIL BURN HEADLAMPS	T	O			
5423	20-12.1	DUI-DRIVING INSTRUCTOR (R/83)	T	O			
5424	20-129(D)	MOTORCYCLE FAIL BURN TAILLIGHT	T	O			
5425	20-116(G)	IMPROPER LOADING OF VEHICLE	T	O			
5426	20-157(A)	FAIL TO HEED LIGHT OR SIREN	T	A			
5427	20-129(D)	DRIVE W/O REAR LAMPS	T	O			
5428	20-217	FAIL TO STOP FOR STOPPED BUS	T	A			
5429	20-138	DUI - FOURTH OFFENSE (R/83)	T	O			
5430	20-140.4(2)	MOTORCYCLE FAIL TO WEAR HELMET	T	O			
5431	20-138(B)	DRIVE W/.1 OR MORE BL ALC(R83)	T	O	900628		
5432	20-155	FAILURE TO YIELD	T	O			
5433	20-146	DRIVE WRONG WAY ON DUAL LANE	T	O			
5434	20-30(5)	FICTITIOUS DRIVERS LICENSE	T	A			
5435	20-166(B)	HIT - RUN PROPERTY FAIL INFO	T	O	910408		

5436	20-166(B)	HIT AND RUN - FAIL INFO	T	O		910408
5437	20-166(B)	HIT-RUN PERSON-FAIL ASSIST (M)	T	O		910408
5438	20-166(A)	HIT-RUN PERSON FAIL STOP (F)	T	O		910408
5439	20-166(C)	HIT-RUN -- UNATTENDED VEHICLE	T	O		910408
5440	20-183.2	INSPECTION VIOLATION	T	O		
5441	20-7(A)	NO OPERATORS LICENSE	T	A		
5442	20-141.3(A)	PREARRANGED SPEED COMPETITION	T	A		
5443	20-141.4(A2)	MISDEMEANOR DEATH BY VEHICLE	T	A		
5444	20-140.2	OVERLOADED/OVERCROWDED VEHICLE		TO		900628
5445	20-129(A)	FAIL TO BURN HEADLAMPS	T	O		
5446	20-140(B)	RECKLESS DRIVING TO ENDANGER	T	A		
5447	20-153	IMPROPER TURN	T	O		
5448	20-158	RED LIGHT VIOLATION	T	O		
5449	20-111(2)	ALLOW FICTITIOUS REG PLATE	T	O		910408
5450	20-141(J1)	SPEEDING	T	A		
5451	20-153(C)	IMPROPER USE OF TRAFFIC LANE	T	O		
5452	20-141.3(B)	SPEED COMPETITION	T	A		
5453	20-138	ALLOW INTOX PERSON DRIVE(R/83)	T	O		
5454	20-158	STOP SIGN VIOLATION	T	O		
5455	20-7(E)	FAIL COMPLY LIC RESTRICTIONS	T	A		
5456	20-141	TOO FAST FOR CONDITIONS	T	O		
5457	20-127	OBSTRUCTED WINDSHIELD	T	O		
5458	20-154	UNSAFE MOVEMENT	T	O		
5459	20-138.1	DWI 2ND OFFENSE	T	O		910408
5460	20-141(M)	FAILURE TO REDUCE SPEED	T	O		
5461	20-111(2)	EXPIRED REGISTRATION CARD/TAG	T	A		
5462	20-122.1	UNSAFE TIRES	T	O		
5463	LOCAL ORDINANC	SPINNING TIRES	T	O		910408
5464	20-140(A)	RECKLESS DRVG-WANTON DISREGARD		T		A
5465	20-111(2)	FICT/CNCL/REV/ALT REG CARD/TAG	T	O		961202
5466	20-165.1	DRIVE WRONG WAY-ONE WAY ST/RD		T		O
5467	20-141(A)	EXCEEDING SAFE SPEED	T	O		
5468	20-29	FICTITIOUS INFO TO OFFICER	T	A		
5469	20-7(F)	EXPIRED OPERATORS LICENSE	T	A		
5470	20-34	ALLOW UNLICENSED TO DRIVE	T	A		
5471	20-138.1	AID AND ABET IMPAIRED DRIVING	T	A		941003
5472	20-138	DUI - SECOND OFFENSE (R/83)	T	O		
5473	20-138	DUI - THIRD OFFENSE (R/83)	T	O		
5474	20-28(B)	DWLR PERMANENT	T	O		950206
5475	20-16.1(B)	FAIL COMPLY RESTRICTED DRIVING	T	A		
5476	20-142	FAIL TO STOP FOR RR WARNING	T	O		
5477	20-181	FAIL TO DIM HEADLAMPS	T	O		
5478	20-141(A)	SPEED FASTER THAN REASONABLE	T	O		

5479	20-166.1	FAIL TO REPORT ACCIDENT	T	A		
5480	20-173	FAIL PEDESTRIAN RIGHT OF WAY	T	O		
5481	20-134	NO LIGHTS ON PARKED VEHICLE	T	O		
5482	20-152	FOLLOWING TOO CLOSELY	T	O		
5483	20-162.1	ILLEGAL PARKING	T	O		
5484	20-150	IMPROPER PASSING	T	O		
5485	20-111(1)	DR/ALLOW REG PLATE NOT DISPLAY	T	A		
5486	20-128	IMPROPER MUFFLER	T	O		
5487	20-7(A1)	NO MOTORCYCLE ENDORSEMENT	T	A		
5488	20-124	IMPROPER BRAKES	T	O		
5489	20-309	NO LIABILITY INSURANCE	T	A		
5490	20-166(B)	LEAVE SCENE OF ACCIDENT	T	O		910408
5491	20-111(1)	DR/ALLOW VEH NOT REG/TITLED	T	A		
5492	20-129(B)	DRIVE W/O 2 HEADLAMPS	T	O		
5493	20-7(N)	LICENSE NOT IN POSSESSION	T	A		
5494	20-313(A)	OPERATE VEH NO INS	T	A		
5495	20-162	PARK FIRE HYD/STATION/PRIV DR	T	O		
5496	20-162.1	OVERTIME PARKING	T	O		
5497	20-37.6(E)	HANDICAPPED PARKING VIOLATION	T	O		
5498	20-141.3(B)	WILLFUL SPEED COMPETITION	T	O		941003
5499	20	TRAFFIC OFFENSE - FREE TEXT	T	A		
5501	20-218	SPEEDING SCHOOL/ACTIVITY BUS	T	O		941003
5502	20-50	FAIL TO OBTAIN REG OR TITLE	T	A		
5503	20-67	REG/TITLE ADDRESS CHANGE VIO	T	A		
5504	20-8	OPER MOPED LESS THAN 16 YOA	T	O		910429
5505	20-29	FAIL EXHIBIT/SURRENDER LICENSE	T	A		
5506	20-21	USE FOREIGN LICENSE WHILE DWLR	T	A		
5507	20-136.1	LOCATION OF TV IN VEHICLE	T	A		
5508	20-73	FAIL TO APPLY FOR NEW TITLE	T	A		
5509	20-106.1	MOTOR VEHICLE RENTAL FRAUD	F	A		
5510	20-117	FAIL TO SEC RED FLAG ON LOAD	T	O		920203
5511	20-138.1(A)	DWI - LEVEL 1	T	A	910408	
5512	20-138.1(A)	DWI - LEVEL 2	T	A	910408	
5513	20-138.1(A)	DWI - LEVEL 3	T	A	910408	
5514	20-138.1(A)	DWI - LEVEL 4	T	A	910408	
5515	20-138.1(A)	DWI - LEVEL 5	T	A	910408	
5516	20-138.1(A)	DWI - LEVEL 5 - AID/ABET	T	O	910408	941003
5517	20-138.1(A)	DWI (.10) - LEVEL 1	T	O	910408	910429
5518	20-138.1(A)	DWI (.10) - LEVEL 2	T	O	910408	910429
5519	20-138.1(A)	DWI (.10) - LEVEL 3	T	O	910408	910429
5520	20-138.1(A)	DWI (.10) - LEVEL 4	T	O	910408	910429
5521	20-138.1(A)	DWI (.10) - LEVEL 5	T	O	910408	910429
5522	20-138.1(A)	DWI (.10)-LEVEL 5 AID/ABET	T	O	910408	910429

5523	20-7	AID & ABET OPERATORS LIC VIOL	T	A	951201	
5524	20-7(L)	LEARNERS PERMIT VIOLATION		TO951201	971201	
5525	20-37.8	SPECIAL ID FRAUD VIOLATION	T	A	951201	
5526	20-138.3	DWI - PROVISIONAL LICENSE	T	O910408	941003	
5527	20-138.5	HABITUAL IMPAIRED DRIVING	F	A	910408	
5528	20-183.6	REPRODUCTION OF INSP STICKER		T	A	920203
5529	20-183.11	FAIL WEIGH/ENTER WEIGH STATION		TO920203	951020	
5530	130-185	DOG VACCINATION	M	O	940606	
5531	20-28	AID AND ABET DWLR	T	A	941003	
5532	20-287	BUY/SELL VEHICLE NO LICENSE		T	A	940606
5533	20-75	FAILURE TO DELIVER TITLE	T	A	940606	
5535	20-30(3)	DISPLAY ANOTHERS LIC AS OWN		T	A	951201
5536	20-72(B)	FAIL SURR TITLE/REG CARD/TAG		T	A	951201
5537	20-72(B)	DELIVER/ACCEPT OPEN TITLE	T	A	951201	
5538	20-111(3)	GIVE/LEND/BORROW LIC PLATE		T	A	951201
5539	20-149	OVERTAKEN VEH INC SPEED(M)		T	A	951201
5540	90-109	TREATMENT W/O REQ LICENSE		MO	940606	
5541	86A-1	BARBERING W/O CERTIFICATE	M	O	940606	
5542	86A-1	NO BARBERSHOP/SCHOOL PERMIT	M	O	940606	
5544	130A-25	PUBLIC HEALTH VIOLATION	M	O	940606	
5545	130A-178	FAIL TO REPORT FOR TB TREATMNT	M	O	941003	
5546	130-335	SEWAGE DISPOSAL VIOLATION		M	O	940606
5548	14-284.2	DUMP TOXIC SUBSTANCES	F	O	940606	
5549	20-218(B)	SPEEDING IN SCHOOL BUS	T	A	941003	
5550	20-114.1	FAIL TO OBEY TRAFFIC OFFICER		T	A	940606
5551	20-106.2(B)	MV SUBLEASE VIOLATION (M)	T	A	940606	
5552	20-106.2(B)	MV SUBLEASE VIOLATION (F)	F	A	940606	
5555	20-106	POSSESION STOLEN VEHICLE(MISD)		TO910408	930208	
5556	20-111(2)	FICT/CNCL/REV REG CARD/TAG		T	A	961202
5558	20-111(2)	ALTERED REG CARD/TAG	T	A	961202	
5560	20-28(A)	DWLR VIOL LIMITED DRIVE PRIV		T	A	910408
5561	20-7(A)	DRIVE W/O LIC FOR VEH-NON COMM		TO910408	941003	
5562	20-30(7)	SELL FALSE DRIVERS LIC/PERMIT		TO910408	941003	
5563	20-37.6(C3)	SELL HANDICAPPED PLACARDS		T	A	930927
5564	20-30(5)	OBTAIN DR LICENSE BY FRAUD		T	A	940606
5565	20-313	PERMIT OPERATION VEH NO INS		T	A	910408
5566	20-107	TAMPERING WITH VEHICLE PARTS		T	A	940606
5567	20-107(B)	TAMPERING WITH VEHICLE STEAL		T	A	940606
5568	20-107	TAMPERING WITH VEHICLE	TA	940606		
5570	20-138.3	DRIVE AFTER DRINKING PROV LIC		TO910408	951020	
5571	20-12.1	IMPAIRED SUPERV/INSTRUCTION		T	A	971201
5572	18B-401(A)	DRINK BEER/WINE WHILE DRIVING		MO910408	940606	
5573	20-63(G)	COVERING/DISGUIISING REG PLATE		T	A	910408
5574	20-79	IMPROPER USE DEALER PERMIT/TAG	T	A	910408	
5575	20-102.1	FALSE REPORT OF THEFT OF MV		T	A	920203

5576	20-130.1(E)	USE OF RED OR BLUE LIGHT	T	A	920203	
5577	20-138.3	DRIVE AFTER CONSUMING < 21		T	A	951020
5579	20-396	RADAR DETECTOR COMM VEHICLE		TO941003		951201
5580	20-115.1	TWIN/SEMI TRAILER VIOL(M)	T	A		910408
5581	20-166(A)	FEL HIT/RUN FAIL STOP PER INJ		F	A	
5582	20-166(B)	HIT/RUN LEAVE SCENE PER INJURY		T	A	
5583	20-166(C)	HIT/RUN FAIL STOP PROP DAMAGE		T	A	
5584	20-166(C)(1)	HIT/RUN LEAVE SCENE PROP DAM		T	A	
5586	20-37.7(E)	NC ID CARD FRAUD	T	A	930621	
5588	20-343	CHANGE OF MILEAGE VIOL	F	A		930621
5590	20-109(B)	ALTERING SERIAL NUMBERS	F	A		910408
5591	20-71(A)	ALTER TITLE	T	O	910408	941003
5592	20-71(A)	ALTER TITLE	F	A	941003	
5593	20-30(7)	SELL FALSE DRIVERS LIC/PERMIT		F	A	941003
5594	20-138.7	OPEN CONT AFTER CONS ALC 1ST		T	A	951020
5595	20-138.7	OPEN CONT AFTR CONS ALC SUBOFN	T	A		951020
5596	20-127(C)	WINDOW TINTING VIOL	T	A	951020	
5597	20-118.1	FAIL TO ENTER/WEIGH STATION		T	A	951020
5599		TRAFFIC OFFENSE-FREE TEXT		A		
5610	20-138.2	DWI COMMERCIAL VEHICLE	T	A		910408
5615	20-138.2(A)(1)	COMMERCIAL DWI UNDER INFLUENCETA				910408
5620	20-138.2(A)(2)	COMMERCIAL DWI >=.04	T	A		910408
5622	20-138.2A	CONSUME ALCOHOL COMM VEH		T	A	990127
5624	20-138.2B	CONSUME ALCH SCH BUS/CHILD VEHTA				990127 5630
	20-7(L)	LEARNERS PERMIT VIOLATION >18		T	A	971201
5633	20-11(L)	LIC/PRMIT TIME LIMIT VIOL <18		T	A	971201
5634	20-11(L)	LIC/PERMIT NO SUPV DRIVER <18		T	A	971201
5640	20-141.5(A)	FLEE/ELUDE ARREST W/MV (M)		T	A	971201
5641	20-141.5(B)	FLEE/ELUDE ARREST W/MV (F)	F	A		971201
5642	20-141.5(B)	ELUDE ARRST MV 2 AGRVTG FCTRS		F	A	971201
5643	20-141.5(B)	ELUDE ARRST MV >=3 AGRV FCTRS		F	A	971201
5655	20-7(A)	NO DRIVERS LIC COMM VEHICLE		T	A	910408
5657	20-28(D)	DRIVE CVEH CLIC DISQUALIFIED		T	A	950606
5660	20-37.12	DRIVE CVEH W/C LIC SUS/REV/DQD		TO910408		950606
5661	20-37.12(A)	COMM DL NOT IN POSSESSION	T	A		951201
5662	20-37.12(A)	DR COM VEH W/O PROPER ENDORSE		T	A	921005
5670	20-101	FAIL TO MARK FOR HIRE VEHICLE		T	A	951201
5674	20-290	FAIL DISPLAY/ADVT LIC OR LIST		T	A	951201
5680	20-396(A)	RADAR DETECTOR COMM VEHICLE		T	A	951201
5682	20-396(A)	MOTOR CARRIER LOG BOOK VIOL		T	A	951201
5699	20	COMMERCIAL LICENSE - FREE TEXT	T	A		910408
5704	14-227.1	SECRET LISTENING PRISONER/ATTY	M	A		
5705	14-159.6(B)	TRSPSS POSTED PROP PINE STRAW		M	A	971201
5706	14-159.6	TRESPASS ON POSTED PROPERTY		M	A	941003
5707	14-134	TRESPASS WITHOUT A LICENSE		M	O	910408

5708	14-159.12	FIRST DEGREE TRESPASS	M	A		
5709	14-159.13	SECOND DEGREE TRESPASS	M	A		
5710	COMMON LAW	FORCIBLE TRESPASS	M	A		
5711	62-319	RIDE ON TRAIN UNLAWFULLY	M	A		
5712	14-160	INJURY TO PERSONAL PROPERTY	M	O		
5713	14-460	RIDE ON TRAIN UNLAWFULLY	M	A		990127
5714	14-202	SECRET PEEPING	M	A		
5716	14-134.3(A)	DOMESTIC CRIM TRESPASS(M)	M	A		
5717	14-134.3(B)	DOM CR TRESPSS SAFE HOUSE WEAP	F	A		990127
5720	14-149	DESECRATING GRAVES	F	A		940606
5729	14-159.3	TRESPASS W/ALL TERRAIN VEH	M	A		971201
5730	63-26.1	TRESPASS ON AIRPORT PROPERTY	M	A		
5799		INVADE PRIVACY - FREE TEXT	A			
6030	130A-185	DOG OR CAT VACCINATION	M	A		940606
6040	90-109	TREATMENT W/O REQ LICENSE	M	A		940606
6041	86A-1	BARBERING W/O CERTIFICATE	M	A		940606
6042	86A-1	NO BARBER SHOP/SCHOOL PERMIT	M	A		940606
6044	130A-25	PUBLIC HEALTH VIOLATION	M	A		940606
6045	130A-144(F)	FAIL TO REPORT FOR TB TREATMNT	M	A		940606
6046	130A-335	SEWAGE DISPOSAL VIOLATION	M	A		940606
6048	14-284.2	DUMP TOXIC SUBSTANCES	F	A		940606
6099		HEALTH LAW - FREE TEXT	A			940606
6170	105-113.110	CONTROLLED SUBSTANCE TAX CRIME		F		O
	910408	951020				
6175	105-113.111	TAX ON DRUGS	M	O		910408 941003
6180	105-236	FAIL TO FILE/PAY INCOME TAX	M	A		
6185	105-236(9A)	AID/ASSIST FRAUD TAX RETURN	F	A		941003
6188	105-236(7)	ATTEMPT TO EVADE OR DEFEAT TAXF		A		941003
6190	105-236	FAIL TO FILE/PAY SALES TAX	M	A		
6195	105-308	FAIL TO LIST PROPERTY FOR TAX	M	A		
6199		TAX REVENUE - FREE TEXT	A			
6205	14-361	INSTIGATE CRUELTY TO ANIMALS	M	A		940606
6206	14-360(B)	CRUELTY TO ANIMALS(F)	F	A		990127
6207	113-270.1(B)	FISHING WITHOUT A LICENSE	M	A		971201
6208	113-270.1(B)	HUNTING WITHOUT A LICENSE	M	A		971201
6209	113-270.1(B)	TRAP WITHOUT A LICENSE	M	A		971201
6210	14-360(A)	CRUELTY TO ANIMALS(M)	M	A		
6211	14-361.1	ABANDONMENT OF AN ANIMAL	M	A		
6212	113-271	FISHING WITHOUT A LICENSE	M	O		971201
6213	113-272	NO TROUT LICENSE	M	A		
6214	113-270.2	NO HUNTING LICENSE	M	A		
6215	113-270.3	NO BIG GAME LICENSE	M	A		
6216	75A-6	MOTORBOAT W/O LIFESAVING DEV	M	A		

6217	113-270.3	HUNT/FISH/TRAP-NO GAME LICENSE	M	A		
6218	113-291.8(A)	FAILURE TO WEAR HUNTER ORANGE I		A		
6220	113-294	SELLING/BUYING WILDLIFE	M	A		
6221	113-291	TAKE GAME DURING CLOSED SEASON		M	A	
6222	113-291	EXCEEDING GAME LIMIT	M	A		
6223	75A-4	OPER MOTORBOAT INVALID NUMBER		M	A	
6224	75A-6	BOATING W/O REQ LIGHTS/EQUIP	M	A		
6225	113-291.1(F)	USE UNPLUGGED SHOTGUN	M	A	920928	
6230	75A-10(B1)	DWI - MOTOR BOAT/VESSEL	M	A	930621	
6231	113-134	FISH WITH UNLAWFUL BAIT	M	A	951201	
6240	113-291.1(B)(2)	SPOTLIGHTING DEER	M	A	941003	
6242	113-291.1(E1)	SHINE/SWEEP LIGHT FOR DEERM		A	941003	
6244	113-60.25	OPEN BURNING WHEN PROHIBITED	M	A	941003	
6246	113-270.3(C)	FAIL REPORT/TAG BIG GAME	M	A	941003	
6248	113-264(A)	LITTER GAMELAND/ACCESS AREA	M	A	941003	
6250	113-285	HUNT/FISH POST PROP NO PERMIT	M	A	941003	
6252	113-291.1(B)(1)	HUNT FROM MOTOR VEHICLE	M	A	941003	
6254	113-135	DRIVE ON GAMELANDS ILLEGALLY	M	A	941003	
6256	75A-15	EXCEEDING NO WAKE SPEED	M	A	941003	
6258	113-135(A)	UNLAWFUL CAMPING	M	A	941003	
6259	14-362.2	DOG FIGHTING	F	A	971201	
6260	14-362	COCKFIGHTING	M	A	941003	
6261	14-362.1	ANIMAL FIGHTING	M	A	941003	
6262	113-187	TAKE SHRIMP IN CLOSED AREA	M	A	941003	
6264	113-292	FISH TROUT WATER CLOSED SEASON	M	A	941003	
6266	113-152	COMM FISHING NO VESSEL LICENSE	M	A	911003	
6267	113-154(A)	COM/MECH FISH NO SHELLFISH LIC	M	A	951201	
6268	113-152	GILL NET VIOLATION	M	A	941003	
6269	113-135(A)	CRAB OR CRAB POT VIOLATION	M	A	951201	
6271	113-290.1	NEGLIGENT HUNTING	M	A	940606	
6279	113-135(A)	POUND NET OR STAKES VIOLATION	M	A	951201	
6281	113-187(D)(1)	TAKE SHELLFISH POLLUTED WATER	MA	A	951201	
6282	113-135(A)	TAKE/POSS UNDERSIZE BLUEFISH	M	A	951201	
6283	113-135(A)	TAKE/POSS UNDERSIZE CLAMS	M	A	951201	
6284	113-135(A)	TAKE/POSS UNDERSIZE FLOUNDER	M	A	951201	
6285	113-135(A)	TAKE/POSS UNDERSIZE OYSTERS	M	A	951201	
6286	113-135(A)	TAKE/POSS UNDERSIZE RED DRUM	M	A	951201	
6287	113-135(A)	TAKE/POSS UNDERSIZE SPOT TROUT	M	A	951201	
6288	113-135(A)	TAKE/POSS UNDERSIZE STRPD BASS	M	A	951201	
6289	113-135(A)	TAKE/POSS UNDERSIZE WEAKFISH	M	A	951201	
6299		WILDLIFE - FREE TEXT		A		
7101	127A-131(A)	CONVERSION OF MILITARY PROP.	M	O	941003	
7102	127A-131(A)	DESTRUCTION OF MILITARY PROP	M	O	941003	
7103	127A-131(B)	FAIL TO REGISTER MILITARY PROP	M	O	941003	
7110	14-401.4	REMOVE/ALTER ID NUMBER	M	O	941003	
7112	14-434	RETAIL UNLAWFUL RECORDINGS	M	O	941003	

7199	PROPERTY - FREE TEXT	O		941003
7301	75D-7 PERJURY - RICO	F	O	940606
7399	PUBLIC ORDER - FREE TEXT		O	940606
8410	LOCAL ORDINANC	CITY/TOWN VIOLATION (I)	IA	940606
8499	LOCAL ORDINANCE(I)-FREE TEXT	I	A	
8501	LOCAL ORDINANC	TRAFFIC CONTROL DEVICE VIOLI		920203
8502	LOCAL ORDINANC	PARKING VIOLATION	T	O
8503	LOCAL ORDINANC	PARKING VIOLATION	I	A
8504	LOCAL ORDINANC	NOISE ORDINANCE VIOLATION		M
	A			
8505	LOCAL ORDINANC	LOITER FOR DRUG ACTIVITY	MA	940606
8506	LOCAL ORDINANC	URINATE IN PUBLIC	M	A
8507	LOCAL ORDINANC	BEACH STRAND VIOLATION	MA	940606
8508	LOCAL ORDINANC	CITY/TOWN VIOLATION (M)	MA	940606
8509	LOCAL ORDINANC	SPINNING TIRES	I	A
				941003
8510	LOCAL ORDINANC	LEASH LAW VIOLATION	M	A
8511	LOCAL ORDINANC	SCREECHING TIRES	M	A
8512	14-399	LITTERING PUBLIC/PRIV PLACES	M	O
8514	LOCAL ORDINANC	LITTERING BEER/WINE CONTAINER		M
	A			
8516	LOCAL ORDINANC	ILLEGAL DUMPING	M	A
8518	LOCAL ORDINANC	NO CITY DOG TAG	M	A
8520	91-2	UNLICENSED PAWNBROKER	M	O
8522	LOCAL ORDINANC	PEDDLE LICENSE VIOLATION	M	A
8526	LOCAL ORDINANC	NO CITY TAGS	T	A
8528	67-12	ALLOW DOG RUN AT LARGE/NIGHT	M	O
8530	LOCAL ORDINANC	DEFRAUDING TAXI DRIVER	M	A
8532	LOCAL ORDINANC	SELLING BEER/WINE W/O LICENSE	MO	941003
8534	69-31	VIOL OF FIRE EXTINGUISHER LAW	M	O
8536	LOCAL ORDINANC	LOITERING	M	A
8538	LOCAL ORDINANC	OBSTRUCT PEDESTRIAN SIDEWALK		M
	A			
8540	LOCAL ORDINANC	TRESPASS OR SLEEP IN PARK VIOL	M	A
				951201
8543	LOCAL ORDINANC	DISCHARGE FIREARM IN CITY	MA	940606
8544	LOCAL ORDINANC	POSSESS FIREARM ON CITY PROP		M
	A			
8546	LOCAL ORDINANC	CARELESSNESS WITH FIRE	M	A
8555	14-399(C)	LITTER-NOT>15LB/27CUFT	M	O
				910408
				911001

8556	14-399(C)	LITTER BEACH-NOT>15LB/27CU FT	MO	910408	
	911001				
8557	14-399(D)	LITTER - 15-500LB/27-100CU FT	M	O910408	911001
8558	14-399(E)	COMM LITTER-ANY QUANTITY		FO910408	941003
8559	14-399(E)	LITTER QUAN>500LB OR>100 CUFT		FO910408	911001
8560	14-399(E)	LITTERING HAZARDOUS WASTE		FO910408	941003
8561	14-399(C)	LITTERING-NOT > 15 LBS.	M	O911001	941003
8562	14-399(D)	LITTERING 15 - 500 LBS.	M	O	911001
8563	14-399(E)	LITTERING > 500 LBS.	F	O	911001
8570	LOCAL ORDINANC	BARKING DOG	M	A	941003
8572	LOCAL ORDINANC	BLOCKING FIRE EXIT	M	A	941003
8599	LOCAL ORDINANCE-FREE TEXT		A		
9901	15A-727;733;734	EXTRADITION/FUGITIVE OTH STATE	FA		920203
9902	17-1	HABEAS CORPUS	F	A	920203
9905	14-18.2(B)	INJURY TO PREGNANT WOMAN(F)	F	A	990127
9906	14-18.2(C)	INJURY TO PREGNANT WOMAN(M)	M	A	990127
9910	14-7	ACCESSORY AFTER THE FACT (F)	F	A	971201
9911	14-7	ACCESSORY AFTER THE FACT (M)	M	A	971201
9912	14-6	ACCESS. BEFORE THE FACT (R/81)	F	O	
9914	COMMON LAW	FELONY AID AND ABET	F	A	
9916	COMMON LAW	MISDEMEANOR AID AND ABET	M	A	
9918	14-2.4(A)	FELONY CONSPIRACY	F	A	
9919	14-2.4(B)	MISDEMEANOR CONSPIRACY	M	A	941003
9920	90-95.1	CONTINUING CRIMINAL ENTERPRISE	F	A	
9922	14-7.1	HABITUAL FELON	F	A	
9923	14-7.7	VIOLENT HABITUAL FELON	F	A	961202
9924	14-254	CORPORATE MALFEASANCE	F	A	
9926	14-344	SCALPING TICKETS	M	A	
9928	14-118.1	SIMULATION OF COURT PROCESSES	M	A	
9930	14-230	WILLFUL FAIL DISCHARGE DUTIES	M	A	
9954	20-16.5	CIVIL REVOCATION DR LIC (10)	M	A	971201
9955	20-16.5	CIVIL REVOCATION DR LIC (30)	M	A	971201
9956	20-138.3	DRIVE AFTER DRINK-PROV LIC T	O		910408
9958	20-138.1	AID AND ABET DWI	T	O	910408
9960	18B-401(A)	DRIVE-CONS MALT BEV PASS AREA	T	O	910408
9962	20-141.1	SPEEDING - SCHOOL ZONE	T	O	
9964	20-141(B)	EXCEEDING POSTED SPEED	T	O	
9966	90-108(A)(7)	MAINT PLACE CONTROLLED SUB (M)	M	A	
9968	90-108(A)(7)	MAINT PLACE CONTROLLED SUB (F)	F	A	
9974	14-410	POSSESSION OF PYROTECHNICS	M	A	
9975	14-410(B)	SALE PYROTECHNICS TO < 16 YR	M	A	951201
9980	20-63(G)	COVERING/DISGUIISING REG PLATE	TO		910408

9984 20-79 IMPROPER USE OF DEALER PERMIT T O 910408
9999 OTHER - FREE TEXT A

Defining Charges:

Below, the variable called "chargen" is recoded into a variable with eleven categories, called "firstc11." This variable was then recoded into "fircrnk." Categories are defined as below. The variable was reordered to correspond to a hierarchy in which speeding is number 1 (most likely to be the charge that caused the stop), unsafe movement is 2, and so on.

recode chargen (lowest thru 2399,2503 thru 4299,4900 thru 5339,5342 thru 5345,5346,5347 thru 5364,5370 thru 5399,5406,5443,5453,5470,5471,5479,5523,5531,5532,5533,5540 thru 5552,5704,5711, 5509,5540 thru 5548,5562,5563,5564,5566,5568,5574,5575,5576,5588,5590,5591,5592,5593,5686,5704 thru 6040, 6041,6042,6044 thru 6205,6207 thru 6299,7101 thru 7112,8410,8499,8503,8504,8506,8514,8599,9901 thru 9999=1)
(2404 thru 2499,5417,5555,5567=2)
(4401,4402,4403,4412,4419,4431,4432,4433,4434,4416,4437,4439,4447,4451,4458,4459,4463 thru 4466,4469,
4482,4484,5412,5413,5419,5422,5424,5445,5446,5447,5457,5458,5464,5466,5484,5492=3)
(4448,4452 thru 4455,4476,4479,4480,5428, 5432,5454,5476,5477,5480,5482=4)
(4407,4418,4421,4422,4423,4424,4425,4426,4427,4428,4429,4430,4435,4436,4462,4486,4488,4490,4491,4492,5424,5427,
5430,5462,5486,5488,5596,=5)
(4449,4450,4456,4460,4467,4468,4474,4475,4478,5442,5450,5456,5460,5467,5478,5498,5501,5539,5549=6)
(4414,4470 thru 4473,4485,4531 thru 4535,5414,5444, = 7)
(4404,4405,4406,4408,4409,4410,4411,4413,4415,4417,4420,4438,4441,4444,4445,4446,4457,4461,4477,4481,4483,4494,
4495 thru
4499,5341,5408,5415,5425,5463,5479,5481,5483,5487,5495,5496,5497,5499,5507,5510,5530,5532,5579,5580,
5589,5597,5599,8509=8)
(4801 thru 4926,5402,5426,5435 thru 5439,5448,5468,5490,5550,5581 thru 5584,5640 thru 5643=9)
(4440,4442,4443,4489,
5400,5401,5407,5409,5410,5411,5416,5418,5420,5421,5434,5440,5441,5449,5455,5461,5465,5469,5470,5471,5474,
5475,5485,5489,5491,5493,5494,5502,5503,5504,5505,5506,5508,5523,5524,5425,5525,5528,5529,5531,5533,5535 thru 5538,5451,5452,

```
5556,5558,5560,5561,5565,5573,5586,5630,5633,5634,5655 thru 5682,5699=10)
(5403,5404,5405,5423,5429,5431,5453,5459,5472,5473,5511 thru
5522,5526,5527,5559,5570,5571,5572,5577,5578,5594,5595,5610,
5615,5620,5622,5624,5625=11) into firstc11.
recode firstc11 (6=11)(3=10)(4=9)(5=8)(7=7)(8=6)(11=5)(10=4)(9=3)(2=2)(1=1) into fircrnk.
execute.
value labels fircrnk 1'speed' 2 'unsafe move' 3 'fail to stop yield' 4 'equip' 5 'seatbelt'
6 'other misc traff' 7 'dui' 8 'license' 9 'resist or esc' 10 'st veh' 11 'crim charge'.
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Appendix E: Self-Reported Police Speeding Stops: Results from a North Carolina Record Check Survey

**Donald Tomaskovic-Devey
Cynthia Pfaff Wright**

Abstract

Survey reports of police stops are a potential methodology for examining the magnitude and prevalence of the “driving while black” phenomenon. Respondents can be asked to report on police stops as well as their own driving behavior. Estimates of the magnitude or correlates of racial disparity in police stops from self-reported survey data are potentially compromised if there are racial differences in the accuracy of self-reports of police stops and driving behavior. We report on the results of a record check survey in which we directly assess the degree and consequences of racial differences in self-reports of police stops. In our sample of drivers who have been cited for speeding in the last year we found that 74.8 percent of whites and 66.8 percent of African Americans admitted to being stopped. Thus, while both groups under-report stops, African Americans do so at a higher rate. This finding is consistent with many past studies which report stronger social desirability effects on survey responses among African Americans. Thus, survey data will tend to under estimate the magnitude of the “driving while black” phenomenon. We find that the people who fail to report a speeding stop also tend to report lower levels of such other undesirable driving behaviors as rolling through stops signs and speeding. *There were no race by reporting interactions in reports of other driving behaviors.*

An early draft of this paper was presented at the American Society of Criminology Meeting, November 17, 2000, San Francisco, California. Contact Tomaskovic-Devey don_tomaskovic-devey@ncsu.edu or at above address.

Introduction

Much of the research on the “driving while black” phenomenon relies on what we can learn about police stops recorded in the police reports filed related to the incident. Ordinarily, official records capture information for citations and searches, and some police organizations

officially record written warnings. As useful as this information may be, a large number of stops are not routinely and officially recorded and are thus missing for purposes of analysis. This unfortunate omission of stop information makes it difficult to address two key questions related to racial profiling: 1) Are minority citizens the target for unwarranted stops in order to further investigate the driver and/or occupants of the vehicle? (such stops could be considered pure harassment); 2) Are the post-stop experiences of minority citizens different than the experiences of majority citizens?

A recent report (Langan et al. 2001) suggests that we can assess the magnitude of “driving while black” and learn a great deal about the quality of those interactions by directly surveying and asking drivers about their stop experiences. In a survey context, it is also possible to ask drivers about their driving behavior and then model, from the drivers’ points of view, the probability of stops having been initiated by the drivers’ driving behavior as well as their personal demographics. The types of models we have in mind might regress the probability of being stopped because of race, while controlling for racial differences in age, gender, miles driven, highway versus local driving, tendency to speed, and tendency to violate routine driving regulations (rolling through stop signs, failure to use seat belts). To the extent that self-report driver surveys can inform racial profiling research, such a technique may prove to be valuable to law enforcement agencies and communities looking for a simple and straightforward means by which they can assess police-citizen contacts.

Using self-report survey data may also appear to be as flawed as simply reviewing written records of stop, citation, and warning data. Survey responses may under-report police stops because of the sensitive nature of reporting violations of traffic and other laws and drivers’ distrust of the police or surveys in general. Responses may also over-report police stops because

of the high profile and political nature of the issue. If sources of non-reporting are associated with the race of respondent, then survey-based analyses may be misleading—either by exaggerating or underestimating the degree of racial disparity in police stops.

For example, of the drivers in our North Carolina Citizen Survey, 18.1 percent of whites and 26.4 percent of African Americans report being pulled over by police in the last year. Given these self-reports, we might conclude that African Americans are 1.45 times more likely to be pulled over than whites. But are they actually pulled over at that rate? Researchers answering yes to this question must make the strong assumption that white and African American drivers are equally likely to accurately report being pulled over. As we will see, a review of the literature indicates that a significant number of African Americans tend to under-report in response to all types of questions that they interpret as being potentially threatening, sensitive or embarrassing at a much higher rate than most whites typically under-report in response to the same questions. In order to test the assumption that there is no racial variance in under-reporting instances of being pulled over by police, we conducted a Record Check Survey of North Carolina drivers with known speeding citations in North Carolina. In a standard record check survey, the investigator knows the answer to the question before administering the survey, and then he or she surveys respondents to measure the accuracy of their responses compared to the original records.

Background Literature

Item Under- or Non-Reporting for Sensitive Questions

Our chief undertakings in the record check survey are to discover the levels and types of inaccuracy in survey data and to identify the characteristics of inaccurate responders. Sudman and Bradburn (1982) identified four factors related to survey response errors: memory,

motivation, communication, and knowledge. Motivation errors are the major concern of this part of the report. Motivation errors are inaccurate answers to survey questions which occur solely because the respondent wants to manage the interaction in order to be viewed in a more positive light by the interviewer. Motivation-based reporting errors are most likely to occur when the survey item elicits a social desirability response. This type of reporting bias can manifest itself as non-reporting, over-reporting, and/or under-reporting.

Over-reporting is a common occurrence for survey items that measure socially desirable activities such as voting. Under-reporting is more common for survey items that measure undesirable activities, for example, drug use (see Sudman and Bradburn, 1979 for a review). In the literature, questions asking about undesirable activities are often referred to as “threatening” or “sensitive” questions. These types of questions encompass activities which are thought to be private, embarrassing, or illegal (such as personal income, party affiliation, religion, sexual habits, or criminal activity). Respondents generally under-report when answering these types of sensitive questions because they think that by admitting to having engaged in such behaviors, the interviewer would not view them as favorably as they might have had they not known about these so-called sensitive behaviors. Social desirability is thought to be at the root of non-response and under-reporting to sensitive questions (Kormendi, 1988). These two phenomena are directly connected to the reduced accuracy or validity of answers. Self-reports of police stops clearly are instances of threatening questions at risk for a social desirability-based under-reporting.

Threatening question item response rates vary by study. The non-response rates tend to range from fewer than 5 percent for questions considered to be less threatening (such as witnessing a crime but not reporting it, Clark and Tiff, 1968), to as high as 73 percent for questions considered to be more threatening (such as bankruptcy, Bradburn et al., 1979). The topic of victimization,

often studied with the use of questions considered to be potentially threatening, has been repeatedly studied. The reviews of this research show that victimization is consistently under-reported (Czaja et al, 1994; Yost and Dodge, 1970; Dodge, 1970; and Turner, 1972). In fact, research indicates a direct relationship: the more extreme the crime, the greater the under-reporting. For example, the findings in the victimization studies show larger under-reporting on questions about assault than for questions about burglary.

Race and Item Under- or Non-Reporting for Sensitive Questions

Stocking (1979) used the Marlowe-Crowne Social Desirability Scale (Crowne and Marlowe, 1960, 1964) to review these studies. He found that nonwhite respondents are more likely to attempt to please the interviewer by giving socially acceptable answers to sensitive questions. African Americans are more likely to complete interviews (thereby cooperating with the interviewer), but the information may be less valid in response to sensitive questions due to the respondents' attempts to provide socially acceptable answers. Indeed, overall, African Americans, as a group, are more likely than whites to respond to surveys (Groves and Couper, 1998, Cohen and Carlson, 1995, Brehm, 1993, Jackson et al, 1982, O'Neil, 1979, Hawkins, 1975).

For our purposes, those drivers who refuse to be interviewed are not as important to the study as is our ability as researchers to discern whether or not the information provided by the driver in the interview is accurate—especially the responses to what may be interpreted by the drivers as threatening questions. Women, nonwhites, and those with lower levels of education are more likely to under-report unacceptable behaviors or counter-normative attitudes (DeLamter, 1982:168). Sudman and Bradburn (1974), summarizing previous research on responses to

attitude questions, report that, for those questions arousing concern, African Americans are more likely to have a response effect than whites. Witt et al (1992), in a study of item non-response to questions about drug use, report that nonwhites are more likely to be item non-respondents than whites. Cox et al. (1992) found that, compared to whites, African Americans and Hispanics not only had a higher non-response rate, but also higher incidences of inconsistent responses for answers to questions about drug use.

Previous research using the record check survey methods have found further evidence of African Americans being more likely to under-report sensitive or threatening questions. Czaja et al (1994), for example, examined respondents' strategies for the recall of crime victimization incidents. They found that 71 percent of whites reported victimization—compared to only 44 percent of African Americans. Hence, the odds of whites reporting their victimization was 1.9 times higher than nonwhites (see also Sparks 1981, Biderman and Lynch 1981, and Dodge 1983, for similar findings on victimization; see Czaja and Blair 1990, and Czaja et al 1992, for studies on other types of questions). There is also evidence that African Americans are more likely than whites to conform, or acquiesce to questions with positive social desirability cues (Lenski and Leggett, 1960 and Hare, 1960).

Record Check Surveys

The validity of self-reported behaviors that violate state or federal laws are often questionable because of the afore-mentioned problems of under-reporting and incomplete or inaccurate reports. It seems that participants, in their attempts to be both good respondents (by answering the question) and to present a positive self-image to the interviewer, often do not refuse to answer the question. Instead, more often, they report that they did not engage in the

threatening behavior being asked about (Bradburn et al 1978). For instance, Clark and Tiff (1966), using a polygraph to check validity, found that 15 percent of the respondents refused to answer a question on speeding while 38 percent of respondents under-reported speeding.

In this study, the possible consequences of more under-reporting by African Americans compared to whites would be to under-estimate the extent of the “driving while black” phenomenon. Conversely, if African Americans are more likely than whites to report stops, this factor could result in exaggerated indictment of law enforcement behavior. While the previous literature strongly suggests that African Americans are less likely than whites to report threatening behavior, it may be that the current politicization of the “driving while black” phenomenon encourages African Americans to recall and report driving stops. Since media reports tend to place the blame for stops on police and not African American drivers, the social desirability effects may be weakened for reports of police stops of African Americans in the current political climate.

One method used to identify under-reporting and inaccurate respondent reports is to conduct a record check survey. The Record check survey is a methodological tool used to evaluate the validity and accuracy of respondents’ answers. Researchers create a survey, asking questions about information about the individuals who are surveyed that the researchers already have. Data collected from the respondents can then be compared to the known answers—thereby effectively assessing the accuracy of the respondents’ answers. The purposes of the Record check survey in this study are to determine whether or not drivers who have been stopped by police are willing to report the stops during a telephone interview, and to assess how accurately the drivers report the incidents. Using the findings from a Record check survey allows researchers—without access to respondents’ known behaviors—to statistically compensate for under-reporting.

The design of the National Crime Survey (NCS) included three record check surveys. In these record checks, police reports were compared to survey answers for a sample of citizens with known police contacts (Yost and Dodge, 1970; Dodge, 1970; and Turner, 1972). Based on the findings from these record check surveys, the National Crime Survey was then redesigned to use survey items that produced less under-reporting. While we have built some question-wording experiments into our survey to improve future surveys on police stops of motorists, our primary objective is to determine whether or not racial differences in the probability of under-reporting police stops effect estimates of the magnitude or existence of the “driving while black” phenomenon.

Study Methods

Design of North Carolina Record Check Survey

Since our research question focuses directly on the relationship between the race of the respondent and his or her propensity to be stopped by the police for speeding, we are specifically interested in race-based variation in reporting of police stops. While studying the validity of respondent answers is important in its own right, the overarching goal of our record check survey is to increase the precision of estimates of race-based response variation in citizen surveys.

For the North Carolina Record Check Survey, a sample of known North Carolina speeders were selected from a database of North Carolina citizens who had been ticketed for speeding between June 1, 1999 and June 1, 2000. The list of inclusive names was obtained from the N. C. Administrative Office of the Courts (AOC). We used the list of names of ticketed drivers within the one-year period as the population from which we drew a stratified weighted sample of names. Our targeted goal was 600 completed surveys. We weighted our sample in order to have approximately one-half African American respondents and one-half white respondents. This

weighted sample design was advantageous because our research question concentrates on discovering adjustment measures for race-based differentials in response to police stops. The cooperation rate for the survey was 70 percent for white respondents and 69 percent for African American respondents. As in previous research, whites refused to participate at slightly higher rates and African Americans were slightly more difficult to locate. It is more difficult to find valid phone numbers for African Americans than for whites,, but African Americans cooperate with the survey at higher rates than do whites.

One week before the initial telephone contact attempt, advance letters were sent to each of the persons included in the sample. The letters explained that the survey focused on the driving experiences of people in North Carolina and their observations of other drivers on North Carolina roads, and the results would be used to aid traffic safety and policy decisions. The survey itself was administered by telephone and averaged nine minutes to complete. Most of that time is allocated to general driving questions, designed partly to reduce the threat associated with questions about police stops. In addition to our goal of finding estimates of response bias, we also included an experimental manipulation of the wording in one-half of the questionnaires to estimate the effects of the wording of threatening questions on question response. We will report the results of that experimental manipulation in a future paper.

One issue of concern in record check surveys of stopped drivers is the possibility that interviewers will know or guess that everyone had been stopped and so encourage higher self-reports. As a precaution to minimize such potential interviewer bias, previous researchers have seeded record check survey samples with respondents who had not experienced the event in question (Sudman, et al, 1977). Because we were fielding a larger survey with many of the same questions, and also because the record check survey contained an experimental manipulation, we

simply informed the interviewers that the three surveys were linked and contained experiments in question wording and questionnaire design. Therefore, the high proportion of respondents reporting stops in the last year in the record check survey was disguised by the much larger number of respondents reporting no stops in the larger driver survey.

Analyses

Since all respondents selected for our survey had been stopped for speeding, the record check survey concerns the probability of a respondent admitting to the speeding stop event. We are particularly interested in any racial differences in reporting speeding stops, since the larger project uses survey self-reports as one avenue of exploring the “driving while black” phenomenon.

Response Bias in Reports of Police Stops

Table E.1 shows the distributions of drivers admitting to having been stopped by the police. The findings from the record check survey are that we can expect police stops of all types to be under-reported by about 29 percent, and that there is a significant racial difference in self-reports of stops. Whites are 8 percent more likely to report any stops than are African Americans.

Table E.2 reports the core of the reverse record check, self-reports of speeding stops among drivers whom we know, from official records, had been stopped for speeding in the last year. Thirty percent of drivers do not report the specific speeding event which we used to select them for the sample. There is a significant racial difference in self-reports: African Americans

Table E.1 Self-Reports of Stops by Police in the Last Year by Race

	One or More Stops Reported	No Stops Reported	
Total (n=604)	70.8% (427)	29.2% (176)	Chi-Square = 4.639 Probability = .019
White (n=305)	74.8% (228)	25.2% (77)	
African American (n=299)	66.8% (199)	33.2% (99)	

are 2.9 percent less likely than whites to admit to a speeding stop which happened in the last year. This suggests that survey-based self-reports of police stops may under-represent actual racial disparities in police activity.

Table E.2 Self-Reports of Speeding Stops by Police in the Last Year by Race

	Percent of One or More Stops Reported	Percent of NoStops Reported	
Total (n=602)	63.5	36.5	Chi-Square 10.85 Probability = .001
White (n=305)	69.8	30.2	
African American (n=297)	56.9	43.1	

Since the probability of speeding and being stopped is tied to other demographic characteristics that may be associated with race, we examine whether or not this basic finding is sustained after controls are applied for gender, age, education, and home ownership (as a proxy for social class). Table E.3 reports a logistic regression of self-reports of speeding stops upon race as well as a series of demographic and behavioral control variables. The findings from the models indicate that racial differences in the reporting of police stops is sustained after controlling for gender, age, education, and home ownership. Model 2 also suggests that younger drivers are more likely to report their speeding stops than men and older drivers. We also ran

interactions of race with age, gender, education, and home ownership. The racial differences in self-reported speeding behaviors do not interact with any of these correlates of race.

Table E.3 Logistic Regression of Self-Reported Speeding Stop upon Race and Demographics Controls: Logodds, Odds Ratio, (Probability).

	Model 1	Model 2
Race (1=African American)	-44, .64, (.015)	-47, .63, (.015)
Gender (1=female)		.22, 1.25, (.266)
Age		-.03, .97, (.000)
Education		-.01, .99, (.907)
Home Ownership		.25 1.28 (.262)
Degrees of Freedom	1	5
Model Chi-Square	5.961	23.917

Using Reverse Record Check Estimates of Response Bias to Adjust for Race Differences in Stop Reports

The previous analyses demonstrate that there are racial differences in the likelihood of reporting stops in general (Table E.1), and of speeding stops in particular (Tables E.2 and E.3). The North Carolina Record Check Survey was keyed to speeding stops, but more general surveys of racial differences in stop experiences would be more likely to focus on all police stops. Speeding stops actually provide somewhat less room for police discretion in drivers who are stopped than other reasons for stops, as discussed elsewhere in this report. In this section, we use the racial differences in any stop reports from Table E.1 to estimate racial differences in police stops using a larger, general survey of North Carolina drivers.

Table E.4 Full Survey Preliminary Estimates of Racial Differences in Police Pull-overs, Adjusting for Response Bias

	Self-Reported Stops		Adjusted for Response Bias	
	White	African American	White (267 Self-Reported Stops/.748)	African American (360 Self-Reported Stops/.669)
Drivers with Stops	267	360	356	538
Total Drivers	1477	1368	1477	1368
Percent Stopped	18.12	26.32	24.1	39.76
Ratio African American/White Stops	1.47		1.65	

From the main survey of drivers we have data on 1,477 white drivers and 1,368 African American drivers. Of the 1,477 white drivers, 18.1 percent report being pulled over by police in the last year. African Americans report being pulled over about 45 percent more often—26.3 percent of the African American respondents report being pulled over by police in the last year.

The reverse record check results suggest that both of these are likely to be under-estimates. Recall that in Table E.1 we saw that whites reported only 74.8 percent of actual stops and African Americans reported even less—at 66.9 percent. We can calculate, based on reported stops, the likely actual incidence of stops within race. For whites, that number is 356 (267 self-reported stops divided by .748) and for African Americans, our estimated number of stops is 538 (360 self-reported stops divided by .669). Thus, the reverse record check suggests that the driver survey estimate of racial differences in stops (Table E.4) should be adjusted upward based on racial differences in self-reports of stops. The self-report data suggest that African Americans are 1.45 times more likely than whites to have been pulled over in the last year. Adjusting for

response bias based on the reverse record check, the data suggest that African Americans are actually 1.65 times as likely to have been stopped in the last year.

Response Bias and Self-Reports of Other Driving Behaviors

An additional goal of the driver survey is to identify racial differences (if any) in risky driving behavior. Using the data from the reverse record check survey, Tables E.5 and E.6 produce estimates of racial differences in reported risky driving behavior among those that report and fail to report speeding stop incidences. In this analysis, we investigate if the response bias identified in the record check survey is associated with racial differences in self-reports of driving behavior.

The dependent variable in Table E.5 is an additive scale of that we refer to as “risky driving behavior.” It is meant to capture some of the driving behaviors that, while seemingly minor, could bring one to the attention of a police officer. Risky driving behavior sums self-reports of rolling through stop signs, speeding up for yellow lights, failure to signal, and not using seat belts. African Americans report .44 fewer risky behaviors, significantly less than reported by white drivers. This number is reduced slightly after controls for accurate self-reports of speeding stops, but shows no significant interaction with bias in reporting speeding behavior. Drivers who fail to self-report speeding stops also report significantly fewer (.26) risky driving behaviors.

In Table E.6, we examine self-reports of typical speeds driven in 35 mph and 65 mph speed limit zones. There are no significant racial differences in self-reported speeding behavior in a 35 mph zone. African Americans report driving more than 1 mph slower than whites in a 65

mph zone. For both speed limits, those who are more likely to admit being stopped also admit to higher typical driving speeds. In neither case is there a significant interaction with race.⁵¹

Table E.5 Regressions of Self-reported Risky Driving Behavior on Self-report of Stops, Race, and Their Interaction; Metric Coefficient (Significance), n=604.

	Risky Driving Behavior		
Race (1=African American)	-0.439 (.000)	-0.406 (.000)	-0.302 (.034)
Self-Report of Speed Stop		.262 (.000)	.512 (.069)
Self-Report* Race			-.163 (.350)
Adjusted R ²	0.042	0.055	0.044

Table E.6 Regressions of Self-Reported Speeding Behavior on Self-Report of Stops, Race, and Their Interaction; Metric Coefficients (Significance), n=604

	Driving Speed When Limit is 35			Driving Speed When Limit is 65		
Race (1=African American)	-.594 (.098)	-.433 (.228)	-.504 (.398)	-1.252 (.001)	-1.135 (.004)	-.568 (.378)
Self-report of Speed Stop		1.282 (.000)	1.112 (.355)		.911 (.024)	2.271 (.081)
Self-Report* Race			.112 (.881)			-.888 (.272)
Adjusted R ²	0.003	0.021	0.019	0.016	0.022	0.023

These analyses lead to two conclusions. First, respondents who are truthful on the record check question are also likely to report higher rates of illegal driving behavior. We interpret this to represent a general tendency toward a more accurate response to threatening survey questions among this population. To test this conclusion we also ran a secondary analysis of

⁵¹We repeated the analyses in Table E.6 using a dummy variable for reporting driving 10 or more miles per hour above the speed limit, and the results were the same. We also re-ran the analyses in both Tables E.5 and E.6, deleting a single African American case with very low reported normal driving speeds, but the substantive results were unchanged.

nonthreatening questions—self-reports of miles driven last week and per year. In neither case was non-response on the reverse record check associated with self-reports of miles driven. Thus, when the question was nonthreatening, there was no bias associated with a tendency to accurately self-report a speeding stop. The second conclusion is that non-response bias in reporting police stops is unlikely to effect estimates of race differences in driving behavior.

Conclusions

As in past research, we find that African Americans are more likely than whites to give socially desirable answers to threatening survey questions. This tendency means that surveys of drivers designed to estimate the magnitude of the “driving while black” phenomenon will tend to underestimate police stops for both minority and majority drivers. This tendency will, however, be greater for African Americans. Therefore, survey reports of police stops will tend to underestimate the actual degree of racial disparity in police stops.

Respondents who fail to report police stops are also likely to provide more socially appropriate responses to questions on risky driving behaviors or speeding. There is, however, no evidence that African Americans are particularly likely to under-report either risky driving behaviors or speeding. Evidently, the degree of threat in these items is not sufficient to produce the type of race-linked social desirability responses we see for reports of police stops. This simplifies the use of survey data on race and police stops.

Survey-based estimates of the magnitude of the “driving while black” phenomenon are likely to underestimate the true degree of racial disparity in police stops. In the North Carolina Record Check Survey, we found that 69.8 percent of whites and 56.9 percent of African Americans who had been stopped for speeding in the last year actually reported such stops. This

suggests that self-reports of speeding stops by whites will be under-reported by about 31 percent. Similarly, African American self-reports of speeding stops, at least in North Carolina, are likely to be under-reported by 43 percent. Self-reports of police stops from survey data should probably be adjusted upward to reflect these biases. Similarly, multivariate statistical analyses of the causes of police stops (for example, race, gender, age, or driving behavior) should probably be weighted so that those who report stops represent their expected proportion in a population.

Appendix F: Citizen Focus Groups

Citizen Perception of the “Driving While Black” Phenomenon: Research Summary From Six Focus Groups

A version of this Appendix was presented at the annual meeting of the American Society of Criminology. San Francisco, November 17, 2000. C. Robert Fenlon (North Carolina Central University) was author.

Introduction

Focus group research in this study of citizen perception of “driving while black” is mainly used to gather qualitative data to explore several issues. These are the reported and perceived reasons for police stops, the perceived treatment of citizens by North Carolina State Highway Patrol (NCSHP) troopers as reported by the respondents, their experiences with other law enforcement encounters (local and county), how the police-citizen encounter began and developed, and what knowledge citizens can report about police-citizen encounters by other community members, friends and relatives. As such, our intent is to gather information on personal experiences with the NCSHP troopers and other law enforcement officers, as well as “vicarious” information as reported by the respondents regarding knowledge of racial profiling.

Focus groups can provide a source of detailed information about a particular social phenomenon that may not be otherwise captured with more traditional methodological approaches. Some would argue that an over-reliance on strictly quantitative methodology hinders the complete understanding of the issue being studied (Krueger, 1994). The focus group technique, as a data collection method used in conjunction with the information obtained from the larger study, is an effective way to extract respondents’ attitudes, opinions, and knowledge through the use of probing, specific questions in order to obtain information on an issue such as racial profiling.

Morgan (1988) points out that focus groups allow for uncovering what individuals think and why they think the way they do about an issue. Focus groups allow an examination of interactions in a group setting, wherein how individuals in the group react to other opinions and

how they respond to opinions that might be somewhat different from their own (Morgan, 1996) can be assessed.

We gathered indepth information from four African American focus groups and two white focus groups in four geographically distinct locations in North Carolina.

Literature Overview

Absent the existence of a federal reporting system capturing information on police-community conflict, generally, and overall police misconduct specifically, systematically gathered information about racial profiling is sparse. The issue of racial profiling is grounded in the history of the often controversial relationship between the police and minority groups.

Evidence does suggest that conflict and tension between the police and the community has steadily increased since the 1960s, when strained police-community relations were indicated as a contributing cause of turmoil and civil disturbances in more than 150 American communities (National Advisory Commission on Civil Disorders, 1968). Several studies (Carter 1985; Dugan & Breda 1991; Gallup 1997, Henderson et al 1997; Murty, Roebuck and Smith 1990; Radelet 1986; Weitzer and Tuch 1999; and Weitzer 2000) have indicated that minorities (African Americans and Hispanics) have less confidence in law enforcement relative to whites. However, most studies to date indicate that the overwhelming majority of citizens—white and minority—report positive perceptions of police (Radelet, 1998).

Even though a majority of citizens in the United States have positive views of the police, African Americans are more likely to believe that the police treat their community concerns with indifference, to feel that their neighborhoods receive inferior treatment relative to white

neighborhoods, and to believe that they are more likely to be targeted unfairly by the police due to their race (Weitzer, 2000).

In a more recent examination of public perception of the police, Radelet and Carter (1994) reported that African Americans indicated the following complaints:

- > Substandard or poor police protection
- > Substandard or poor service to minorities, especially inner-city residents
- > The expectation that the police will not treat them fairly
- > Numerous incidents of verbal abuse and harassment
- > Stereotyping of minorities as criminals, particularly in “stop and frisk incidents”
- > Police use of excessive force
- > Discrimination in police personnel administration.

Still, Murty, Roebuck and Smith (1990) conducted a study assessing public perception of the police in predominately African American communities in Atlanta, Georgia. They sampled 600 African Americans, and their results indicated that 65 percent expressed a positive attitude of the police while 35 percent expressed a negative attitude.

Lastly, Weitzer’s (2000) study examines citizens’ perceptions of racialized policing in three Washington, D.C. neighborhoods: a middle-class white community, a middle-class African American community, and lower-class African American community. His findings indicate that there is across the board agreement that police treat African Americans differently from whites. Most of the white respondents in his study took the position that black crime and criminality leads to discriminatory, yet justifiable, treatment because “blacks have a greater likelihood or a higher rate of black involvement in crime in Washington, D.C.” This seems to be the modal explanation for the white community as to why African Americans are treated more harshly by the police. It was interesting to note that a minority of African American respondents held the same view, cutting across similar logical deductions, correctly or not, regarding this rationale of police treatment of African Americans.

Overall, given limited research, minority perceptions of police are more negative than whites. This is understandable given the history of relations between minorities and law enforcement in the United States. Racial profiling or “driving while black” seems to be indicative of the historical relations between the police and minorities.

Methodology

This focus group research study on public perception of racial profiling used an exploratory methodological design approach to help better understand this issue. The results are intended to stand on their own without any follow-ups. The research team chose to conduct focus groups in four major North Carolina cities distributed across the state. Four African American and two white focus groups were conducted. The research team determined that segmentation was needed to create groups of similar racial makeup due to the nature of the issue, racial profiling. To achieve the highest quality of information, maintain an environment conducive to forthright opinions that are not guarded or masked, and overall, make focus group respondents feel more comfortable, we believed that intra-racial composition was needed. Each focus group, with consideration for age and gender, consisted of ten persons from the same race, having a range in ages from 24 to 60, and had at least four females. The two white focus groups were held in the same city, in succession after two of the African American focus groups. Respondents were selected by a research firm located in each selected city.

Two trained facilitators external to the research team were used to conduct the focus group sessions. The research team believed that an intra-race facilitator and respondent were needed. Both moderators had considerable experience conducting focus groups on sensitive issues. Each session lasted approximately 90 minutes and on some occasions went beyond this time mark.

Debriefing sessions were held subsequent to each focus group session, in order to discuss observations made by the moderator and the research team regarding facilitating style and participant responses. At each focus group session, the research team took notes in an observation room within each facility from behind a one-way mirror. Each of the respondents voluntarily agreed to participate and signed permission forms allowing them to be recorded and video taped at each of the focus group sessions. However, no last names were used in discussions or recorded to ensure confidentiality. The audio tapes of each session were transcribed and sent to the research team along with the video tapes.

The moderator of each focus groups imposed two kinds of structures: areas at issue and the level of control exerted to complete the information gathering process in the allotted time. At the onset of each focus group, the moderator opened with an introduction that fully explained the intent and purpose of the research being conducted, who was doing the research and why, and what was expected of them, so that each of the respondents understood the nature the research study. The following issues were discussed:

- ▶ Personal feelings in general about the police;
- ▶ Differences in police behavior by type/jurisdiction of officer (such as local, county, and state);
- ▶ Personal experiences involving police stops;
- ▶ Reasons why police may target African Americans drivers.

Data Analysis

We were looking for direct information, expressed by focus group respondents, that exemplified the core of the issues. While preliminary, the results reported here do reflect the research team's overall assessment of the tone, flavor, and direction of the focus group analysis.

This afternoon we will address some of the material where it best highlights the commonalities and differences in the experiences and perceptions of racial profiling within the white and African American groups.

Issue One: General Feeling Toward the Police

It was perceived by the researchers that the general feeling toward police overall was positive in the African American focus groups. African Americans generally expressed a positive notion about police performance, practices, and behavior when addressing the safeguarding of their community. While African American citizens held favorable views of police in general, they expressed a tremendous distrust of police as individuals. For example, one respondent indicated

“policemen are humans, too, but there are good ones and there are bad ones . . . and it’s the bad ones that give the entire police department a bad name.”

Another respondent explained his mistrust of police, even though he had not had any negative experiences with them, in this way:

“Well, I personally, I’d never had a bad run in with any police officer. But, I am afraid of them.”

In another focus group located in the western part of North Carolina, respondents expressed similar sentiments as well. They believed that police are there to protect and serve the community, and that they do a good job as a whole. However, a statement made by one of the participants illustrates the gulf that exists between acceptance of what law enforcement is intended to accomplish and the level of trust in the expectation that this will be accomplished:

“They are there to protect and serve but you have to be cautious of them. . . . I have never had a run in with the police. . . . but I would not necessarily put my guard down around a police officer.”

Within the same focus group, two respondents had different views of the police in their assessment.

“Growing up, policemen were portrayed as good . . . now they’re portrayed as bad . . . when I see a police I always think negative. I don’t think positive.”

The other respondent indicated

“when I was younger, I guess I was a little bit negative about the police . . . but as I got older, got to meet some of them personally, heard their point of view . . . it is a very challenging job . . . as a whole . . . I think they’re going a good job”

The white respondents had very positive perceptions of the police. It seemed to us that white citizen participants saw police activity (enforcement) as external to their community. Enforcement, as it should be, is focused where “law and order” seem to be more of a problem. The white respondents, as a whole, seemed to have a sense that police service their community with little or no conflict; therefore, they felt comfortable with police presence. They seemed willing to give up on some freedom, such as stops at police check points, and did not see such inconveniences as an intrusion on their rights. They did not expect such stops to have any great effect on their neighborhoods, since most of the crime committed is outside their community.

One respondent indicated the following:

“They (police) patrol areas where there are potential problems . . . stop things, you know, drug areas they drive through and try to prevent things from happening.”

Another respondent had this to say about the police:

“You have got the rule of law and the police are the ones that enforce that . . . they keep the order . . . and if they (police) have a license check set up . . . have drug dogs . . . something looks suspicious . . . check it out . . . in doing that it has caught a lot of people . . . I for one think its worth a slight inconvenience to get that (drugs) off the road. I very much support him (police).”

Issue Two: Compared Types of Law Enforcement

African American citizens reported a sense that NCSHP troopers were more professional in their dealings with the public than were local law enforcement. They attributed this differences to the level of training received by each. Citizens also spoke negatively of the stereotypical sheriff lounging across the food counter at the convenience store with his shirt hanging out. It was believed that the NCSHP would enforce the law more consistently and would rely on personal relationships with citizens less often in determining the final outcome of the stop than local law enforcement would. On balance, African American citizens expected to be treated fairly by the NCSHP but not so fairly by local police.

“I feel that they are more professional, the state troopers.”

“If you are speeding, they are going to give you a ticket.”

“If you are breaking the law, they are going to make it right, but a lot of town cops . . . allow certain folks to do this and do that, and then certain other folks can’t do the same thing.”

An African American citizen speaking of her white neighbor driving through the neighborhood speed trap:

“We meet the city cop, he throws up the hand, she throws up the hand, she continues to go, I feel like I need to break my speed.”

This last comment leads us to white citizens’ assessment of differences among law enforcement agencies. Generally, there is agreement between whites and African Americans here. Both groups express the opinion that NCSHP is more likely to treat one in a formal manner (that is, stop and ticket), while local police are more likely to be idiosyncratic in their policy enforcement. Whites see an advantage with local police in comparison to NCSHP, and this advantage is the result of familiarity between the police and citizenry.

Issue Three: Police Stops and Fairness

African American citizens reported a remarkable willingness to take responsibility for their actions. Especially in cases where speeding was the infraction triggering the stop, these citizens reported that the stop was fair. Still, there was a serious concern about the treatment after the stop. Here issues of respect and understanding surfaced. Citizens reported confronting such questions as “Do you live around here?” and “Is this your car?” It should not be surprising that, even when a citizen is knowingly in the wrong, such questions will tarnish future interactions between citizens and police. The situation is magnified when the stop itself is illegitimate, as reflected in the following:

“I see him (the officer) just looking around my car and what not, and he said, ‘You know the reason why I pulled you over, don’t you?’ He said, ‘Your tag has September ‘98 on it.’ I said, ‘It’s only May.’ He was like, ‘That’s right. My fault.’ He gave me my driver’s license and registration back and said, ‘You don’t have any guns in the car do you?’ I said, ‘No, it’s at home with the dope.’”

Another encounter was the result of mistaken identity. A young man and his brother had the misfortune of being in the general vicinity of a search. The two brothers were stopped, tackled, searched, and detained without ever being told the reason for the encounter.

Walk back towards the front of my car. He told me to walk backwards. With my hands in the air. I walked backwards about five steps. They stopped me there and told me to lift up my shirt and turn around so that they could see that I didn’t have anything in my waist. I did that. I took three more steps back and that’s when I was tackled. I don’t know how many of ‘em grabbed me but I felt at least four of ‘em. One on each hand, you know, somebody grabbed me around my neck and they handcuffed me and had me down, patted me down. I was asking, what did I do? What’s going on? He said, just don’t say anything just be quiet. We’ll tell you later. Just stand up and get in the back of the car. And, okay, all right. They put me in the back of the car. Did the same thing to my brother except they only let him stand up out of the car and when faced forward, a whole bunch of them just ran over towards him and tackled him into the grass. They held both of us for ten minutes and we were sitting in the back of the car. They searched my car, they opened trunk, went through everything in the car. Nobody told me why they were stopping me. Nobody told me what was happening, my license disappeared for three days. I don’t know

who took it. One of the officers took my wallet and my license was taken out of my wallet. My wallet was given back to me but whoever took my license got in their car and left with it. I got it back in the mail three days later. I don't know where it went. I don't know who took it and don't know why it was taken."

No whites reported this type of treatment, although whites did note that police do sometimes single out drivers. We heard of "driving while blond" and being stopped for "driving a flashy car." The point here is that the stop experiences that whites shared were specific to themselves and, except in very general terms, were not shared by others. Interestingly, while white drivers do not report pretextual stops, they were much more likely than African Americans to display a sense of entitlement and even resentment when they were stopped. "Sure I was speeding, but they should have cut me a break." Or the often standard "Why are they stopping me when they could be doing something useful?"

Issue Four: Targeting African American Drivers? (African American Citizens) Who Gets Targeted by the Police (White Citizens)

A majority of African Americans reported that, other than race itself, police are more likely to stop them due to the presumption of "black criminality" and physical stereotyping. It is expected that police are more likely to target African Americans whose dress is less conventional, who drive cars deemed to be linked to criminal activity, and who have hair that is counter to the "norm" (dread locks, for example). African American participants recognize that criminal stereotyping is predictive of disparate treatment. This recognition heightens the sense of unfair treatment of African Americans by the police. Additionally, there is a presumption by the participants that African Americans are less likely to challenge what they know to be unfair treatment by the police. A lengthy court challenge to an unfair ticket, in the long run, may be more costly in time and money than the actual cost of the ticket.

“Most of the time, we don’t fight it, we don’t complain, we just pay it.”

“Blacks are less likely to complain, have less resources with which to get things done.”

For whites, police are seen as on the lookout for teens, certain types of vehicles, types of individuals “likely to commit a crime,” persons who “don’t fit the car.” While such stereotypes might suggest a clear racial bias on their face, participants were just as likely to suggest that young and male, regardless of race, would be enough to trigger disparate treatment. For example, one subject noted:

“I think all young males—I think if you see a young male in a fancy sports car with all of the gadgets and the radio is loud, I don’t think they would care if he were black or white, they would pull him.”

After some recognition that young African Americans, in particular, might be more likely than whites to be stopped, especially if they were somehow “out of place” (neighborhood, type of car) whites wanted to discuss how racial discrimination could go both ways.

“I think it exists, but I think it exists on the other side too.”

“A black police officer pulling a white guy.”

“I mean, you see it in your workplace. It is the same thing.”

Whites seemed more comfortable with targeting and profiling when it could be expressed as affecting both whites and African Americans. In sum, whites know about “driving while black” from media accounts, but they don’t think that the extent of the problem is as great as it is portrayed. Indeed, whites tended to not be particularly sympathetic to African American drivers who were being targeted by police on the basis of race. They suggested that police were stopping African Americans for good reasons such as drugs or violence. Their examples were not connected to driving at all, suggesting a disquieting generalization from one realm of experience

to another. We end this section with one of the most extreme positions noted in the white focus groups:

“They can ride around all times of the day or night, they don’t bother to have job any where, but they keep gas in the car, so you have got to suspect that they have got something going on in order to keep the car full of gas and on the road.”

Conclusions

The issues surrounding racial profiling by police are complex. One pattern is revealed through the examination of official police stop statistics. We suspect that often a very different pattern will appear when actions are viewed through the eyes of police, and also of drivers.

While generalizations based upon a small number of interviews should be cautiously avoided, the information we do have suggests that, although African American and white drivers may agree in principle that profiling—even racial profiling—exists, they do not agree on what it is or on what are its benefits or consequences. While disagreements on all of the issues were found within each of the focus groups, the strongest differences were found to be between the two races when the groups were examined collectively.

Our focus groups with African American drivers revealed a generally positive evaluation of the job that the police do. Participants were quick to say that the police had an important and tough job and that they were grateful for the good work they do. At the very same time, however, many of the African American drivers had very little trust in individual police officers. They felt themselves to be at risk of harassment and bias based on race, and they made considered analytic distinctions for each and every time they were stopped. Some stops were judged fair—typically when they realized or admitted that they had broken a law and they were subsequently treated with respect. In general, African Americans in the groups described law enforcement as an institution as legitimate and reasonable, yet conversely, they described individual police as

suspect. They attributed racial bias to “bad apples.” There was, however, some disagreement among African Americans as to how common the bad apples are.

Stops that were not tied to serious illegal driving behavior—the most common was the “rolling stop” pull-over—were treated as likely instances of racial bias. In many of these cases African Americans assumed race was the cause of the stop, because they did not recognize any other legitimate reason. In some cases this assumption was confirmed by the police officer, such as when reporting that the African American citizen was stopped for being in the wrong—or, in other words, white—neighborhood (and thus out of place). One young man spoke of the time he was stopped (with his brother), removed from the car, tackled, and had guns drawn on him for driving in a neighborhood where another African American man on foot was being pursued by the police. Here, apparently, “young, black male on foot” was interpreted as “black male anywhere.”

Lack of respect by the police during legitimate stops were also evaluated by some African American drivers as likely instance of racial bias. Lack of respect in the interaction was interpreted as an indicator of racial bias, and encouraged the suspicion that the pull-over was racially motivated as well. Troopers of the NCSHP, in contrast to officers attached to various local police forces, were singled out as treating drivers professionally and with respect.

A clear pattern emerged in the focus groups, revealing that African American drivers were less suspicious of the NCSHP than they were of other police officers. While this evaluation mirrors our findings in the citizen survey (that racial disparity in police stops is lower among the NCSHP than among other law enforcement agencies in North Carolina) the focus group participants used respectful treatment, rather than the rate of stops, as the basis for arguing that the NCSHP was better.

In general, African Americans were more likely to perceive racial bias in a stop if the officer interacted with them in a disrespectful manner or they were stopped without what they believed to be a legitimate driving infraction. They seemed to be more than willing to acknowledge their responsibility for a “real” violation. Minor violations created a different perception of the possibility of racial bias. In these cases, African Americans saw race as the predictor of the stop, not the violation. This is in contrast with white drivers who tended to see all stops—legitimate or minor—as discretionary and idiosyncratic. White drivers talked about “driving while blond” or “driving while a musician” or “he should have cut me a break.” In many ways, white drivers evaluated the police more harshly than African American drivers did and were also more likely to generalize “unnecessary” enforcement across agencies. African American drivers saw many stops as legitimate and some as potentially racially biased. White drivers saw most stops as illegitimate, but idiosyncratic.

We also found stark contrasts between African American and white drivers in evaluations of the “driving while black” phenomenon. African Americans tended to see it as just another example of racial bias. Racial bias in the policing of drivers was seen as a form of discrimination similar to the other forms of discrimination faced each day. Its existence was confirmed by some combination of their own experiences, stories they had heard from friends and family, media reports on police bias (Rodney King was often mentioned), and the existence of general levels of prejudice and discrimination in the society at large.

White descriptions were considerably simpler and more disturbing. The white focus groups tended to accept that police targeted African American drivers, but described racial targeting as at least understandable if not fair and justifiable. Since African Americans were stereotypically assumed to be more dangerous and thus more culpable, white citizens typically

saw police stops on the basis of race as reasonable. Whites tended to use stereotypes and statistical discrimination arguments similar to those sometimes used by police to justify racial targeting. It seemed very easy for the white subjects to collectively justify discrimination in policing, even though they were quite resistant to taking personal responsibility for their own police stops. As such, disgruntled white drivers are no natural allies for African American drivers who fear they are being harassed because of their race.

Appendix G: General Issues in Measurement

Before we began the data analysis of the stop, citation, and written warning records, we found that it was necessary for us to evaluate various aspects of the quality of the data themselves. We argue below that the stop data are less complete than the citation and written warning data, and thus much of our subsequent analysis centers on the latter data sets. The primary problem is that we cannot distinguish between citations issued at checkpoints from citations issued as a result of an officer stopping a vehicle in routine patrolling (the data set does not have a variable that allows us to make this distinction). We will also describe data on accidents, as well as observational data on speeders (details of which are in Appendix A).

The Stop Data

Beginning in January of 2000 the NCSHP was required to record every vehicular stop that its troopers made. The stop information is recorded on a form, (the “stop form”) and then entered into a data base. These data seem particularly valuable because they are unique in providing us with information about police-citizen contacts that were previously unavailable to researchers: stops that do not result in citations or written warnings. These types of stops are of interest because they may be argued to be “pre-textual” stops – stops initiated as a pretext for the trooper to ask questions, possibly leading to a search of the vehicle. The data allow us to test the hypothesis that African Americans are stopped and given “no action” or only a “verbal warning” more than whites. Thus, the stop data provide us with further measures of the troopers’ behaviors and thus better enable us to grasp the entirety of police interaction with citizens.

Unfortunately, the value of the stop data can only be evaluated in conjunction with citation and written warning records, so as to determine if the stop records are generally being completed as often as they should be. There are no stop forms completed at checkpoint stops (stops where all or random vehicles are pulled over) and thus we cannot evaluate fully whether all the stop forms that should be completed are being completed. We came to the conclusion that there are too few stops relative to the number of written warnings and citation records to warrant study of the stop records as a stand-alone data base (at least relative to the goals of the present research). Stated another way, the evidence suggests that the troopers are filling out the necessary forms to provide supervisors and researchers with a track record of their transactions with citizens, but sometimes the stop forms are missing, either because they are not being completed (the form is not filled out) or at least they are not being entered (someone fails to enter the completed form into the data base) or because they are not required (checkpoint stop). We estimate that in up to a third of the situations in which we think that stop forms should be filled out and entered into the data base, they are not being completed or entered into the data base when citation and written record forms for the same incidents are being recorded. There are several possible reasons for the under-recording of stop records. For one, as stated above, we do not know how many checkpoint stops there are. Two, there could be confusion as to what constitutes a stop (see discussion below). In addition, unlike the data collecting/entry process for citations and for written warnings, the data processing procedure for stop forms does not include, as far as we are aware, standard data quality checking procedures, such as ascertaining that all the stop forms are processed. It appears that often troopers fill out written warning forms, capturing the race of the driver, but fail to reproduce the information on the accompanying stop form. While written warnings certainly provides a “paper trail” related to the stop, it makes it

sometimes impossible for the researchers to verify that a stop record indicating that a written warning was issued, and a written warning record of a similar event are actually records of the same event. This complicates the research effort, as considerable data processing and verification is necessary to evaluate the quality of the data that are analyzed.⁵²

As mentioned above, ambiguities of what constitutes a stop may also play a role in reducing the number of stop forms filed. Stop forms are to be filled out when a trooper initiates and completes a stop of a motor vehicle. Thus, stop forms are not filled out when a trooper is called to the scene of an accident (or witnesses an accident) or at a checkpoint stop. Nor is a stop form filled out when a driver is already stopped for some other reason such as vehicular dysfunction or rest. In such cases written warnings and citations would not normally result in a corresponding stop form. Unclear is the situation when a citizen calls in a report of another driver's behavior, resulting in a dispatch call to a trooper and a subsequent stop of a driver, since the stop was actually initiated by a citizen. Also, reports from a citizen-band radio, resulting in a driver being stopped should be considered a stop, but may not always be defined as such. We do not know why the stop forms are not always filled out, but we do have evidence that they often are not.⁵³

Although NCSHP troopers are instructed to fill out a stop form every time they initiate a stop, it is not entirely surprising that the form is not filled out when one considers that the data are

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For example, it would be useful to match records of the same incident across stop and citation data bases, or across stop and written warning data bases.

We initially hypothesized that in the first few months data were missing because troopers were learning how to fill out the new stop forms. We examined the data by month and find no trend in the "missing" data. Also, it should be mentioned that when the stop data were sent to us we verified that the number of records received equaled the number of records sent, so we did not lose the data by data processing mistakes.

largely redundant with information – including race -- already included on other forms. Thus, if a trooper failed to file a stop form at a stop, yet issued a citation or written warning, a record of the event was placed on file and subject to our review. A “paper trail” exists. The trooper may think that filing a separate stop form is unnecessary because presumably researchers or supervisors should be able to “figure out” who was stopped by examining all the data files potentially relevant to his or her stop (the citation, written warning or search files). This assumption would hold if all of the information collected on a stop form were collected on each of the other types of forms (such as written warning and citation forms). Unfortunately for researchers, not all the information on the stop form is contained on these other forms. Written warning records, for example, omit age of driver, time of day, and the area of a county in which the written warning was issued. Also, the quality of the stop form data themselves is deficient in other ways. For example, information is not recorded as to the sub-area of a county in which the stop occurred. Thus, the stop data are not useful for defining a unit of aggregation (a unit of analysis) that we elsewhere argue is valuable – the highway area (stretch of highway within an area of a county (roughly a fourth of a county in size).

It would be useful to know whether the failure to record a stop record in the data base is systematic or random across district or across officers. To evaluate that question is surprisingly difficult because there is no identification number linking a stop form to either a written warning record or a citation record. Thus, some matching criteria, such as the demographic characteristics of the driver, time of day, etc., are necessary to match stops with citations or written warnings. To link stop forms that indicate a written warning was issued with written warning files, we initially thought that matching criteria such as the following would allow for accurate matching: the district in which the written warning was issued (fifty-three districts), the trooper

identification number indicating who the trooper is issuing the written warning or filling out the stop form, the month and day of the week, the race and gender of the driver, and some general information about the nature of the charge. However, we found that there were too many inconsistencies in the definitions of these criteria across data sets. For example, type of behavior resulting in a stop or written warning is coded differently in the two data bases so that it is difficult to determine clearly the “reason for the stop” and resulting written warning for any offense other than speeding, equipment violation, or for a license/registration irregularity. Moreover, the reason for a stop may not be the same as the reason for the written warning: the trooper may have stopped the vehicle for speeding but issued a written warning because the inspection sticker had expired (dropping the speeding charge altogether).

As researchers, we must be able to distinguish a citation record which has had a stop record filed from a citation record which does not have a stop record filed in order to know precisely how many stops a trooper made. If the researcher cannot distinguish stop and citation records precisely then we can only estimate how many stops were made by the NCSHP. In the present context this means that fairly liberal matching criteria must be used to achieve a match across data bases. As it turned out, we can only match records by date, district, trooper and gender of person stopped, warned or cited. (Using more criteria results in very few matches.) For matching purposes we cannot distinguish between records of people of different ages or races, because age is not recorded across record types and race is problematic regarding the definition of Hispanics (and by implication whites – see discussion below). Because the matching criteria are liberal, there will be “false positive” matches — some of the matched stop and written warning (or citation) records may not involve the same actual event. As just stated, we used liberal criteria that defined two records as matched if they have the same codes for district, trooper, day, and sex

of driver. Thus, if a trooper issues several written warnings in the same district on the same day to several males, we cannot be sure which specific stop record information on which males stopped and warned (in written form) by the same trooper on the same day matches correctly with a written warning record. Despite this limitation (which we think is minor relative to our primary analysis goals), the data do provide us with an estimate of how many records do not match between the data bases. For example, some stop records will not have a corresponding written warning record and some written warning records will not have a stop record indicating a written warning that we can match.

To get an idea of the completeness of the stop data by themselves, examine Table G.1 below. Here there is a breakdown of the number of stops by the actions taken by the NCSHP trooper at the stop. Also included is the number of non-accident citations and written warnings as recorded in the respective data bases (that is all the citations issued at non-accident encounters with drivers, and all written warnings issued.) As can be seen, there are substantially more citations and written warnings recorded at non-accident interactions between police and citizens than are recorded on stop records. Of course some of these citations and written warnings are issued at checkpoints stops, which do not require that a stop form be filled out.

One possibility that could account for the discord is that troopers do not consistently fill out the stop data forms. This is possible, perhaps, as suggested earlier, because the forms are largely redundant with the data that must be recorded on what are perceived by troopers to be “more important” forms – the citation and written warning forms. Conversations with NCSHP troopers in our focus groups suggest that not all troopers give high priority to completing the form immediately following the stop or submitting the stop forms for data entry daily. For example, one trooper reported that he fills out the forms only at the end of the week or when he has enough

time, and does so by copying the information from their citation and written warning forms. As a consequence of this practice, stops not resulting in a citation or written warning may be overlooked or forgotten. We estimate that there is a substantial percentage of the stop forms that are not filed.

Despite the missing stop forms, some unique information is collected on these forms, making them valuable. Some of the stop forms record “no action” and the “verbal warning” actions taken by the trooper. These two forms of action, however, represent only **2.4 percent** of all the stop records. Approximately 26 percent of the stops resulting in citations and /or arrests in calendar year 2000 do not seem to result in a stop record being filed.⁵⁴ More prevalent is the failure to report written warnings on the stop records, where 55.6 percent of the written warning records have no stop record equivalent. The relatively high rate of missing stop records for written warnings partially corroborates the hypothesis that the troopers do not take filling out or filing the stop forms as seriously as they might, since the written warning outcome would generally be regarded by troopers as a less serious type of intervention than a citation. (that is, we would hypothesize that the less the intervention is consequential for the driver, the less likely the stop form will be filled out.) Because of the missing records, we are reluctant to

⁵⁴ This is actually a conservative percentage because we do not have available information on the actual number of arrests (only the arrests recorded on the stop forms). To attain as complete a list of matches as possible we include arrests at a stop with the citations because a citation has been filed. Troopers would indicate on a stop form that the most serious intervention that occurred was a arrest, and not a citation.

Table G.1 Comparison of Number of Stops to Number of Citation Events and Written Warnings

Action Taken by Trooper at Stop	Number of Stops Resulting in Action Taken	Percent of Records “Missing” (percent of known events not in stop data base)	Events Known (Non-Accident) from Originating Data Base
Citations (& Arrests)	440,098 (including 11,815 arrests)	25.8	593132
No action	3000	--	--
Verbal Warning	10366	--	--
Written Warning	109550	55.9	248296

try to interpret any patterns with the stop data as a “stand alone” data base because it would appear that stops are occurring that are not being entered into the data base.

Despite the fact that most of the information on stop records is redundant with information recorded on other records, it is lack of redundancy that has been responsible for most of our research concerns. One of our research goals is to verify that the official records of written warnings and citations are reasonably complete in the sense that the forms that are filled out are entered to the data base. To determine the extent that forms are being filed, we attempt to match stop records to both the written warning and citation files. We were concerned that not all of the stop forms were being filled out, but we were also concerned that not all of the written warning and citation information was being filled out or processed. To assess the prevalence of missing written warning and citation records, we initially matched stop records to written warning and citation records. The matching process was made difficult, however, because the forms differ

somewhat in the categories of information recorded, making the match rate less than perfect. On the stop form, for example, race and ethnicity are recorded as separate variables, while on the written warning and citation forms, race and ethnicity are recorded as one variable (called “race”). This difference is a source of discrepancy between the different record types. For example, all else equal, it is likely that more Hispanics will be recorded as Hispanic on a form that has a separate variable designating Hispanics than on a form that does not have a separate variable for Hispanics. Before continuing our discussion of the possibility of missing written warning and citation records, we will discuss our concerns about the measurement of who is Hispanic in the various official record files.⁵⁵

Measurement and Classification of Hispanic Drivers

On the citation and written warning records, which have no special variable to indicate whether the driver is Hispanic, 5.3 and 3.3 percent of the records, respectively, (see Table G.2) indicate an Hispanic driver, compared to the stop records, which indicate that 5.4 percent of those stopped are Hispanic (5.6 percent of those getting citations and 3.5 percent of those getting

⁵⁵ By separate variable we mean that there is a separate question as to whether the driver is Hispanic or not. Where there is, a separate variable is to be distinguished from the situation where Hispanic is a category among several others such as “African American” or “white”. Presumably in the former situation more Hispanics would be counted than in the latter situation, where some of the Hispanics would be classified as white or African American instead of Hispanic.

Table G.2 Type of Form and Measurement of Hispanic Drivers

Stop Form	Racial Categories: White, African American, Indian, Asian, Other	Hispanic is not a race option
Written Warning Form	Racial Categories: White, African American, Hispanic , Indian, Other, Unknown/Uncertain	3.3 percent Hispanic
Citation Form	Racial Categories: White, African American, Hispanic , Indian, Other, Unknown/Uncertain	5.3 percent Hispanic
Stop Form with Written Warning as the Trooper Action	Driver's Ethnicity: Hispanic, Not Hispanic	3.5 percent Hispanic
Stop Form With Citation as the Trooper Action	Driver's Ethnicity: Hispanic, Not Hispanic	5.6 percent Hispanic

written warnings). While the similarity in the findings of these two data sets might lead one to believe that the definition of who is an Hispanic is similar in both the stop records and the written warning and citation records, we do not know which specific individuals are coded as “Hispanic” on a stop record **and** on a written warning or citation record because there is no common identifier to link the records. We only know that in the aggregate the percentages are only slightly different (3.3 versus 3.5 percent and 5.3 versus 5.6 percent in the table above).

In general, we are doubtful about the accuracy of the coding of Hispanics. In comparing N.C. Division of Motor Vehicle data on Hispanics to citation data, our analysis revealed that 21.6 percent of the NCSHP-defined Hispanics cited by the NCSHP in 2000 did not have Hispanic listed with the DMV as their “race.” Thus, it is possible that NCSHP troopers over-identify

people as Hispanic or the DMV under-identifies them, or that some Hispanics choose to identify themselves as white or other. At the same time, of those drivers identified as Hispanic by the DMV and differently identified by the NCSHP, we find that only .1 percent are identified as “White,” less than .1 percent as “African American,” while .6 percent are classified as “other” by the NCSHP. Thus, relative to the standard of racial/ethnicity identification used by the DMV, NCSHP troopers tend to “over-identify” drivers as Hispanic and only rarely “under-identify” Hispanics as some other race or ethnicity (most often here as white).

We are especially doubtful about the accuracy of the coding of Hispanics on forms that do not provide for an Hispanic ethnicity classification (the written warning and citation forms). On the stop forms, which have an Hispanic ethnicity category, the race of 89.5 percent of those Hispanics is coded as ‘U’ (unknown/uncertain), with 9.3 percent of Hispanics defined as white (the remaining 1.2 percent are coded as Asian, African American, or Indian). Only 1.1 percent of the written warning records are of ‘U’ drivers (another 3.3 percent are explicitly coded Hispanic) and 3.3 percent of citations are to ‘U’ or ‘O’ drivers (unknown/uncertain or other), with another 5.3 percent listed as Hispanic race. In other words, Hispanics are not generally coded as being of the white race on the stop form, but rather are “unknown/uncertain,” while on the citation and written warning forms, Hispanics are coded as Hispanic for race, with some unknown number of them likely coded as white (we could only know for sure if we had an identification number matching stop and written warning or citation records, such as the stop identification number. Such a number exists, but it is not linked to the written warning or citation records).

There is variation on the stop form in the classification of Hispanics as ‘unknown or other’ versus white (recall there is no Hispanic code for race on the stop form). Only 84.5 percent of the Hispanics on the stop form with a written warning action are coded as “unknown” race, compared

to 90.2 percent of the Hispanics on the stop forms with a citation action. The roughly 5 percent difference is substantial, indicating that it is likely that there is some doubt in the troopers' minds as to how to code Hispanics as far as a racial category (are Hispanics "white" or "unknown/other?"). The possibility exists that a trooper might define an Hispanic as "white" on the stop form, but as "Hispanic" on the written warning or citation form (typically filled out within a few minutes of the stop form). We do not have a ready way to verify how often troopers vary their classification of the race of Hispanics from one form to the next (for example, stop form to citation form) at the same stop incident.⁵⁶ The fact that 9.3 percent of Hispanics (defined so under the NCSHP ethnicity classification) are coded as whites instead of "unknown/uncertain" makes the percentage of Hispanics in the citation and written warning data bases dubious. In short, it is unlikely that we will be able to identify the number of Hispanics "mis-classified" as

⁵⁶ A substantial proportion of the 'unknowns' who have citation records according to the stop data are not Hispanic (21.6 percent). Similarly, 25.4 percent of the 'unknown/uncertain' category for written warnings are not Hispanic, so we cannot assume that all "unknown/uncertain" classifications on the written warning and citation records are actually Hispanic. It is possible that none are Hispanic since the trooper has a race category option for Hispanic, which is presumably the most likely category chosen by the trooper for someone who appears to be Hispanic.

whites in the present data. At the same time, it seems likely that a small proportion—but a non-trivial number— of whites in the citation and written warning records are mis-classified and should be defined as Hispanics.

Missing Stop Records by District

The error associated with the classification of a driver as Hispanic also makes it less likely that we will be able to match all of the stop records unequivocally with the citation or written warning records. Our primary motive for doing so is to ascertain that any missing records (from the citation or written warning data bases) include a disproportionate number of African Americans. In some initial data analysis we attempted to determine how many stop records were “missing” by comparing to all the stop records for the months January through July, of calendar year 2000. (Since we only have trooper identification information for those seven months, they are the only months worth considering for matching purposes.)⁵⁷ However, we found that we could not match a fairly large proportion of cases between the stop and the citation and/or written

⁵⁷ A modification of the original legislation required that an identification number be used by each trooper on the stop form after July, 2000, but that “outside” persons would not have access to this number. This was interpreted by the NCSHP representatives to mean that we, as researchers, should not have access to any identification number, even one without an accompanying list of names. This is not an unreasonable interpretation by the representatives since we as researchers have information on approximately 97 percent of the stops made by the NCSHP from August through December of 2000 that could in principle result in the identification of a trooper. Specifically, knowing the time of day and location of the citation and written warning records, we could make a reasonably accurate estimate of who the trooper was who was involved in a stop. As it stands, however, we do not know whether the same trooper was involved in any two stops for the August through December data because no identification number of any kind was provided to us. The troopers’ identities are known to us, however, for the months of January through July, 2000.

Table G.3 “Missing” Stop Records (Comparing non-Accident Citations and all Written Warnings to the Number of Stop Records Resulting in Citation or Written Warning)

District	Written Warnings and Citation Events	Stops for Written Warnings or Citations	Proportion of Written Warnings and Cites “Missing”
1	18311	11,504	.372
2	14314	12,076	.156
3	12147	8,517	.299
4	11767	8,634	.266
5	9205	6,921	.248
6	8149	6,691	.179
7	26443	15,809	.402
8	11780	8,999	.236
9	15920	11,387	.285
10	20416	14,691	.280
11	20316	14,553	.284
12	17410	12,163	.301
13	11125	9,063	.185
14	11106	7,448	.329
15	18088	14,210	.209
16	10566	8,868	.161
17	34050	21,467	.370
18	16612	10,928	.342
19	13170	9,578	.273
20	13807	8,713	.369
21	21889	15,801	.278
22	9727	7,473	.232
23	21870	17,634	.194
24	11013	9,100	.174
25	7964	6,292	.210
26	19832	14,328	0.278
27	11886	5,440	0.542

District	Written Warnings and Citation Events	Stops for Written Warnings or Citations	Proportion of Written Warnings and Cites “Missing”
28	12670	10589	0.164
29	12818	8,512	.336
30	20068	15,467	.229
31	22970	16,710	.273
32	10416	7,813	.250
33	12557	8,675	.309
34	7264	4,060	.441
35	15556	9,941	.361
36	12728	8,659	.320
37	17580	10,625	.396
38	26482	16,711	.369
39	9519	7,869	.173
40	10833	4,803	.557
41	14199	9,379	.339
42	14646	9,022	.384
43	14658	8,696	.407
44	11454	8,320	.274
45	16142	11,673	.277
46	10078	7,746	.231
47	13243	10,848	.181
48	8770	5,757	.344
49	19925	12,540	0.371
50	11070	8,591	0.224
51	13688	9,261	.323
52	9343	7498	.197
53	13434	11172	0.168

warning data bases for the months from April through July. We suspect that the high failure rate is due to the fact that there was no data verification for the identity numbers that we assigned to troopers for April through July.⁵⁸ (we, the researchers sent each trooper an identity number to use). Some troopers may not have used the identification number that we provided (although most seemed to), or may have been confused about which number to use (our identification number, their regular identification number) or may have simply forgotten their identification number (we resent identification numbers to several troopers the first month). Others may have been careless in recording the correct number. Without basic data verification procedures in place, the April through July data (when the troopers were supposed to use the identification number that we provided them) are suspect for matching purposes.

It is interesting to note that the proportion of “missing” written warnings and citations omitted from the stop data base vary considerably across patrol districts (see Table G.3). The fact that the proportion of “missing” stop records varies as much as it does from district to district supports the idea that some troopers— especially in some districts— are less attentive to filling out the forms. As best we understand, there are no systematic monitoring procedures in place. Of course, use of checkpoints can also vary across districts, and account for some of the “missing” stops.

We summed the number of citations and written warnings in each of the fifty-three districts (excluding citations issued at accidents since there is usually no stop associated with

⁵⁸ Troopers were mailed an id number that only we, as researchers, knew who had what number. However, we did not have a way to verify how consistently that the numbers were being used by every trooper for every stop. Most of the troopers, if not all of them, appeared to be using a number within a range of numbers that we sent them, but we do not know if they always used the correct number.

accidents). Across the fifty-three NCSHP districts, the average proportion of written warnings and citations “missing” is about 29 percent. (In the table, the districts have been arbitrarily assigned an identification number, so our district number is not the NCSHP district number.) Several districts are missing more than a third of their stop records, and one district is missing an estimated 56 percent of its stop records. The fact that what we estimate to be the proportion of “missing” stop records varies considerably across districts suggests to us that the lack of standard data verification procedures results in haphazard reporting of stops in some districts. We doubt that checkpoint citations and written warnings could account for all the “missing” stops.

To simplify the analysis, in the next section we will focus on only the first three months of data in calendar year 2000. Here, troopers were using their regular identification number, the same number they use on the written warning and citation forms. Thus, there is a greater chance that the stop records will match with the citation and written warning records.

Comparison of Stop Written Warning Actions to Written Warning Records

We continue our analysis with a comparison of the stop records indicating that a written warning was issued (the trooper took the action of issuing a written warning) and the actual written-warning records. From January through March, we could identify (locate and match on several characteristics) roughly 96 percent of the written warning records when a stop record indicated that a driver was given a written warning. That is, we could successfully find the matching written warning record when the stop record indicated that there should be a written warning record. We matched the stop-written warning to the written warning record information for the following variables: district of the stop, identification number of the trooper, month and

day of the stop, and sex of the driver. A considerably higher failure rate was found (in other words, fewer matches) when we added race as a criterion for the match. This supports our suspicion that the classification of the Hispanics was not as consistent as we—as researchers—would have liked it to be.

The missing 4 percent of written warning records could be due to any number of factors, including the following: the trooper made an error in recording the district number (perhaps the trooper recorded his/her official district assignment number instead of the number of the district in which the event occurred), his or her identification number, the date, or the sex of the driver. In reviewing the records that we could not match, we noticed that the dates were often different by a few days. This raises the possibility that the trooper simply forgot the date, entered the wrong date, or that the trooper was filling out the stop form at a later date and simply used that date on the form rather than the date of the actual stop. The plausibility of the latter was reinforced during the focus groups with the troopers, when some troopers indicated that they do not always fill out the stop forms at the time of the stop. Instead, the stop forms are completed at a later date by copying the information from the written warning records onto the stop records. Perhaps when doing so, they use the date the form was completed rather than the correct date of the stop (creating a paper trail but one not specific to the actual stop date).

One possible data analysis strategy would have been to use the written warning stop records when we could not find the written warning record. However, there are three reasons why we did not do this. First, it seems unlikely that the 4 percent missing written warning records were really missing (as opposed to not matching). There would be little reason for a trooper to report on the stop form that he or she is issuing a written warning, and then fail to do

so. There is a large surplus of written warning records that do not have matching stop records, and it seems likely that the real matching record is, in fact, somewhere in that pool. Second, the information on the stop form regarding written warnings is generally less useful to us because there is less information about the location of the event. Thus, these observations would drop out of some of our subsequent analysis in which the location of the event was a variable in the data base (they would constitute “missing” data for the analysis). Third, there is virtually no difference in the racial categorization in those records that match versus those that do not match across data bases. Specifically, there is only a .1 percent difference in the percent of African American stop-written warning records missing and the percent white stop-written warning records missing. Thus, there does not seem to be any bias by race in non-matching stop written warnings.

More common than missing written warning records were missing stop records indicating that a written warning had been issued. Approximately 33 percent of the written warning records in January through March had no identifiable stop record associated with it (note that this is a much lower rate of missing stop records than we observed above for the whole year, 56 percent). The 33 percent missing stop records suggests that NCSHP troopers do not always fill out stop records when issuing written warnings (or the written warning was issued at a checkpoint stop). From a data analyst’s perspective, it is unfortunate that the NCSHP trooper does not always fill out a stop record, but the fact that a written warning record was filed by the trooper indicates that a “paper trail” exists, and we can evaluate those warnings for possible racial disparity.

Missing Citation Records

In addition to being concerned about written warning records that possibly were not being filled out or processed, we were even more concerned about the possibility of missing citation records (since citations represent a more severe sanction than written warnings). We were doubtful that there could be missing citation records (for example, a stop form suggests a citation was issued, but we cannot locate the specific citation record) because it would seem unlikely that a trooper would not file a citation if he or she had issued one to a driver. Nevertheless, we conducted an analysis similar to that for written warning records, matching records that were identical in district, trooper identification, month, day and gender. We were able to match 95 percent of the stop records that indicated that a citation was issued. Stated another way, we found that 5 percent of all citation events could be deemed missing by this standard (compared to 4 percent of the written warnings discussed above). We think, however, that—similar to the argument above for written warnings—our failure to identify a citation record as a match to a stop record indicating that a citation was issued could be attributed to recording errors of any of the data elements used to match records. Nor do we find anything but a trivial difference in the racial breakdown of this 5 percent of cases. We, therefore, did not pursue the possibility of generating citation records from the stop records (in other words, creating a citation record because we could not find one that we think was issued based on a stop record), but instead proceeded to analyze the citation records as they had been given to us.

In summary, our analysis of stop record data indicate that a large number of stop records do not appear to have been filed by NCSHP troopers. However, there does not appear to be any justification for a suspicion that significant numbers of written warning and citation records are

also not being filled out or filed by the troopers. At most, 4 percent of the written warnings and 5 percent of the citations could be “missing,” but we think it is more likely that we simply cannot identify the appropriate record. The records exist; we simply cannot identify them from the rather large pool of unmatched records. One thing is certain: a single identifier used across all records would allow both better monitoring of stop reports and assist researchers in the future.

Accidents

In order to determine if there is any racial disparity in stops, citations, or written warnings, it would be useful to compare the proportion of such interventions involving African American drivers with some baseline measure of African Americans violating traffic laws. In the absence of measures of such law violating behavior, we look to other sources of information to approximate the measures we lack. We argue that accident reports filed by the NCSHP represent a useful baseline for comparison of the percent of African Americans who have been issued citations and/or written warnings. Here, we initially ask the question whether or not the accident data represent a meaningful measure of all the reported vehicular accidents, and whether accidents can provide a useful basis for assessing who is driving on the highways or who is perhaps driving dangerously on the highways.

As for the first concern, in large metropolitan areas it is most often the local police who do the “paper work” associated with official records of accidents, and they will not be represented in the NCSHP accident files. (We do not have access to the local police accident files). Of course, the NCSHP generally do not patrol heavily in metropolitan areas (except for major highways such as I-85 or I-40), so the exclusion of local accident data may be less relevant to our concerns. That

is, if the local police process accidents in their primary jurisdiction and the NCSHP processes accidents in theirs— and there is no overlap, then the NCSHP accident data could be a reasonably complete source of information on highway accidents.⁵⁹ Unfortunately, we do not have a means to measure jurisdictional patrol overlaps, so there will be some doubt about the completeness of the NCSHP accident data for our purposes.

Table G.4 shows some correlations between three measures obtained from the Automobile Association of America's (AAA) 1999 data on driving miles and accidents in North Carolina, and four measures of accidents from the NCSHP data files (AAA, 2001). The most recent year for which such AAA data are available is 1999, and we assume that the 1999 data would be highly correlated with the 2000 data. The first AAA variable is a measure (in thousands) of vehicular miles traveled per county (here the 100 counties in North Carolina have been aggregated into the fifty-three NCSHP districts). The second measure is the number of collisions per 10,000 vehicular-miles-driven. It should be noted that these collisions are for all jurisdictions in North Carolina, not only those of the NCSHP. Thus we would expect the correlations between the NCSHP accident data and the data for the whole state to be somewhat attenuated (and they are). The third measure is the number of collisions (again across all jurisdictions), and the fourth measure is the ratio of accidents to injuries (included to show that there is variation in the severity or seriousness of accidents relative to driving prevalence).

As can be seen in the correlation matrix in Table G.4, there is a very high correlation between the number of collisions and the number of VMT (vehicular miles driven)— .965. Thus, much of the variation in collisions across districts can be attributed to the sheer volume of traffic.

⁵⁹ We would also have to rule out accident reports being filled out by each county's Sheriff Department.

The moderately high correlation of collisions per 10,000 VMT with VMT indicates that collisions are more prevalent in high VMT areas independent of the sheer volume of the traffic, suggestive that the density of the traffic may be responsible for the high collision rate, although other safety related factors may be also involved.

The NCSHP data base on accidents provides us with a count of the number of accidents per district. It would be expected to correlate more weakly with the VMT because the jurisdictional area of the NCSHP is a subset of all the areas within a district (VMT is for the entire area, including the metropolitan areas that are not usually patrolled by NCSHP). The correlation of .585 indicates that there is, nevertheless, a moderately strong correlation between the NCSHPs' count of accidents and the vehicular miles driven. The number of people injured in NCSHP-recorded accidents correlates .632 with VMT and .580 with the number of accidents (.511 with number of collisions).

The number of people injured in accidents is not strictly a linear function of the number of accidents. In fact there is much variation in the ratio of accidents to injuries, varying from 1.68 to 12.47 with a mean of 5.907 and a standard deviation of 2.94. Generally the ratio is higher where there is more traffic and a higher rate of collisions per VMT. One could speculate that the "fender bender" type of accident is more prevalent where traffic is heavier. Where traffic is relatively light, an accident has a higher chance of involving an injury than where the traffic is heavy. As for deaths, there is virtually no correlation between the number of deaths and any of the other variables except number of injuries. The number of deaths on NCSHP highways is relatively rare, compared to the traffic volume or number of accidents (the mean number of deaths per district in 2000 is twenty-four). The relative rarity of highway deaths suggests to us that the

number of deaths is quite random and largely a product of such other considerations (not measured here) as promptness of ambulance service, proximity to hospitals, or highway engineering factors.

Therefore, we conclude from this preliminary analysis of the NCSHP accident data that there is a plausible foundation for using such data as a basis for further comparisons. For example, we could use the accident data to compare the percent of accidents with African

Table G.4 Correlations Between Accident Measures*

	VMT (AAA)	Collisions Per 10000 VMT (AAA)	Col- lisions (AAA)	NCSHP Recorded Accidents	NCSHP Recorded Deaths Due to Accident s	NCSHP Recorded Injuries	Ratio of Accidents to Injuries (NCSHP)
VMT	1						
Collisions Per 10000 VMT	0.624	1					
Collisions	0.965	0.743	1				
NCSHP Recorded Accidents	0.585	0.383	0.527	1			
NCSHP Recorded Deaths	.059 n.s.	-.112 n.s.	-0.05	.216 n.s.	1		
NCSHP Recorded Injuries	0.632	0.354	0.511	0.58	0.618	1	
Ratio of Accidents to Injuries	0.289	0.235	0.293	0.829	-.072 n.s.	0.092	1

*All correlations above are statistically significant except where noted as non-significant (n.s.).

American drivers to the percent of those cited who are African American. We discuss further the validity of using accident data in the section below on the observational baseline study in which we observed the race and speed of motorists on fourteen select highway segments.

Baseline Observational Study

In an attempt to determine if there is variation in the speeding of motorists on highways in North Carolina, we observed the race of passing drivers while ourselves driving at the speed limit and timed the speed of those drivers with stop watches. The details of what we call our baseline observational study can be found in Appendix A. Of importance here is the fact that we have gathered some data on fourteen highway segments (both directions of traffic for stretches of highway that are 10 to 15 miles in length, one-way), selected because they are areas where the NCSHP stops many vehicles (both white and African American drivers). The observational study is a modification of a method pioneered by Lamberth, but in many ways is unique. For example, we actually measure the speed of the passing vehicles. Also, we measure the location so as to compare data with stop and citation data from the same or proximate area. Since we were the first to attempt this method, there are improvements that could be made to the study— primarily, that it would have been helpful to have gathered more data at each of the fourteen locations. At the time we did the study (May and June, 2000) we did not know as much about speeding and stops of speeders as we know today. The implication of this understanding is that the results from our baseline observational study are somewhat ambiguous as to the possible presence of racial disparity in the stops and citations on the fourteen highway segments studied. The method,

however, has been proven useful, in our opinion, to the further study of vehicular driving behavior as well as policing behavior.

One of the facts that we learned from the baseline study—after we had collected the data— is that many highways have different “speeding thresholds”— that is, speeds that are likely to result in a pullover and citation. Importantly, these speeding thresholds are quite high relative to our expectations going into the study. These thresholds were roughly twelve to fifteen mph above the posted speed limits, whereas we had expected that seven to nine mph above the posted speed would be likely to trigger a patrol stop and citation. Thus, when we learned that a driver on any of the fourteen highway segments had nearly a zero probability of being cited for driving seven to nine mph above the speed limit, we were surprised. As a consequence of these considerations, we did not find as many speeders (threshold speeders) as we thought we were finding when we collected the data. One unfortunate consequence of the high threshold speeding values, is that we cannot conduct a rigorous comparison of the speeding and ticketing of African Americans in the same geographic areas— there are too few “real speeders” (threshold speeders) to do so. However, we will discuss the general pattern of findings below, which indicates that, overall, there are slightly more African Americans stopped and cited for speeding in the fourteen areas than there are African Americans found speeding. The differences observed, however, are generally not statistically significant across the sites.

In Table G.5 below, we present some of the findings from the observational baseline study of each of the fourteen sites. Note that these fourteen sites represent a convenience sample of locations in North Carolina. To be included in the study, a highway segment had to have four lanes of traffic (for safety reasons— see discussion in Appendix A) and had to be among the

highways with a high number of citations in the early months of the year 2000.⁶⁰ Because the sample is a convenience sample, one should resist the temptation to generalize from our sample of fourteen sites to other four-lane highways or highway segments. We do not have a simple random sample of locations, so generalizing to other geographic areas would be hazardous.⁶¹ As can be seen in Table G.5 the percent of African Americans who speed is examined relative to two standards. The more conservative standard is the speeding above the median local threshold (defined as the median mph above the posted speed limit from those citations issued in 2000 on the same highway [for example, I-95] in the same county area [roughly one fourth of a county]).⁶² A more liberal threshold value would be the speed at the first decile of the distribution of those cited (a decile represents 10 percent of the observations, so the first decile represents the speed at

⁶⁰Note that the stop data includes milepost markers as one of the variables, thus allowing us to identify segments of a highway with relatively many stops. It should also be noted that we checked to see that there were numerous stops of African Americans as well as of whites in these segments, although we did not formalize this process by requiring that a certain number or proportion of those stopped be African American.

⁶¹ We did not have sufficient data or information about speeding to draw a simple random sample of areas. A simple random sample of segments of four-lane highways would yield mostly highway segments where there are too few stops or citations to compare statistically to the observed speeding. We also did not feel confident that the stop data base had all of the stops (as per the discussion earlier in Chapter Two), so that a rigorous sampling of areas with high rates of stops would be inappropriate. The fourteen sites represent fourteen of the top 60 highway areas in the state in terms of number of stops. Most of the areas not selected were two lane highways, or in highly traveled areas around large cities (where we thought it unsafe to drive and collect data on speeders) or too far from Raleigh, where our research team was based, such that we could not afford to drive to the site every day for a week.

⁶² It should also be noted that for the non-Interstate highway segments we generally do not have mile post data, so the location of the stop is only approximate (we know the highway, county and area of the county in which the stop took place, but not exactly the 10 to 15 mile segment of the US or NC highway). It is only for the Interstate highways that we have mile post data and thus know within one mile where the stop occurred. It should be acknowledged, however, that we have no way to check on the validity of the mile post marker information. If a trooper mis-records the mile post where the stop occurred, we would not have a means of finding such an error unless the milepost indicated is in a different police district.

which **Table G.5 Various Measures of “Threshold Speeding” by Highway Segment, Observational Baseline Study**

Segment	Posted Speed Limit	Median Threshold Speed (MTS)	Percent Over MTS who are African American	First Decile Threshold Speed	Percent Over Decile Threshold Speed who are African American	Percent Stopped for Speeding who are African American	Percent in Accidents who are African American (Highway Area)
I-95-N	70	82	22.9	79	14.6	29.4	21.6
I-95-J	65	80	25.4	78	17.6	27.3	27.2
I-95-H	70	80	25	79	21.5	33.8	30.9
I-85-G	65	80	55.9	79	33.9	40.6	41.5
I-85-W	65	80	40	79	22.8	36.8	31.4
I-85-V	65	80	47.6	78	34.3	40.5	32.1
I-85-RD*	55 & 65	70 & 80	12.5*	69&78	17.7*	25.0*	28.1
I-40-J	70	85	15.4	79	14.7	14	15.3
I-40-P	70	84	15.2	79	11.6	14.1	15.7
US-L	55	70	20	69	22.2	22.0**	28.4
US-C	65	80	12.2	77	14.9	19.5**	14.8
US-G	55	70	26.6	69	24.8	26.6**	28.3
US-N	65	80	42.3	77	31.3	44.9**	35.9
NC-G	65	80	21.1	77	10	28.9**	31.6

*Two posted speeds across this highway segment. Construction at site reduced speeding, so N of MTS is very low (16).

** = estimates based on the highway of the observational study for the entire area of county. Since there were no mile posts on these highways, it was impossible to identify stops in the highway segments studied. Estimates of percent African American are generally higher using the county area data than using only the highway segment data (by a percentage or two).

10 percent of the vehicles are stopped and cited). Comparing the two threshold speeds indicates that African Americans are somewhat over-represented among those who are driving at or above the fifty percentile speeding threshold within the fourteen sites.⁶³ Note that we do not have available to us the speeds for which drivers are stopped, only the speeds for which drivers are issued tickets. Thus, it would be reasonable to assume that drivers are stopped at speeds generally lower than the speeds for which troopers issue citations. Many drivers stopped for speeding are given written warnings (roughly a third), and for those drivers we do not have a recorded speed. Also, we have no direct measure of the risk of being stopped at a given speed, only evidence on the speed information of cited speeders.

The table may be read as follows: in highway segment I-95-N, where the speed limit is 70 mph, there is a conservative (or high) estimate of the speed that will result in a stop (82 mph) and a liberal (or low) estimate (79 mph). Half or more drivers are issued citations for speeding at the conservative threshold, and 10 percent or more are cited for speeding at the liberal threshold. (“Conservative” here means that we are probably underestimating the risk of being stopped and cited on I-95-N if one is driving 82 mph, and “liberal” means we are over-estimating the risk of being stopped and cited if one is driving 79 mph.)

The percentage of drivers observed speeding at or above the conservative speeding threshold varies across segments— from a low of 15.2 in segment I-40-P, to a high of 47.6 in I-

⁶³ Generally the first decile value and the median value were quite close, so that there is only small differences in the percent African American using the two definitions. That is because there seems to be a “magic” number for each highway area, and often only one mph difference between the first decile cut-off point and the median cut-off point, e.g, 14 mph more than posted speed is the first decile value and 15 mph the median value. In other words, 10 percent of those cited were cited for traveling 14 mph more than the posted speed, and 50 percent (or more) were cited at 15 mph more than the posted speed. This was a common finding across highway segments.

85-V. Using the more liberal speeding threshold measure, we see that the percent of speeders who are African American varies from a low of 10.0 in NC-G, to a high of 34.3 in I-85-V. These results are both interesting and disappointing. We probably are measuring the speed of vehicles accurately within (plus or minus) 2 mph— see discussion in Appendix A. Yet, the difference between our conservative and liberal estimate of threshold speed is between 1 and 3 mph. The difference that a couple of miles per hour makes in terms of the risk of being stopped is quite large (for that matter, when using radar equipment to measure speed, there is a margin of error within 1 mile per hour, therefore indicating that there is presumably more error in our measurement of speed than in a trooper's). On most of the highways we studied, as the term “threshold” implies, there is a substantial change in risk of being stopped that occurs as one approaches speeds of 80 mph. Traveling 77 mph on some highways elevates the risk substantially; for other highways one needs to be driving 79 mph to incur relatively high risk. By 80 mph, the driver's risk is substantial on all but three of the observed highways. On those three highways, substantial risk does not occur until traveling 82, 84, or 85 mph (I-95-N, I-40-P, I-40-J, respectively).

One of the implications of these numbers is that we would have to be measuring speed exactly (within .5 mph) to know precisely what percentage of African Americans we should expect would be stopped for speeding on each of the highway segments. Instead, we can only be approximate in our estimates. The conservative and liberal criteria initially proposed here suggest that a percentage of African Americans somewhere between 14.6 and 22.9 percent could be expected to be stopped for speeding on I-95-N. This is a rather large interval, pointing again toward the fact that there is some error in measurement that prohibits us from making strong

claims as to racial disparity in our baseline observational study. As we will discuss below, however, we think it is even more complicated than that.

The table also shows that there is some association between the percentage of drivers in accidents who are African American and the percentage driving above the speeding thresholds (both conservative and liberal). Note that the comparison is imperfect regarding the geographic area covered. NCSHP accident data do not include mile post data, so we can only compare the observed speeding behavior within the highway segment with the accident behavior in what we call the highway area (in other words, all the accidents on the same highway within about a quarter of a county).⁶⁴ The actual correlations across the fourteen sites are .793 and .731 between the percentage of drivers in accidents who are African American and the percentage of threshold speeders who are African American (conservative and liberal thresholds, respectively). Omitting the I-85-RD segment (because of low reliability) raises the correlations somewhat—to .842 and .739 (conservative and liberal thresholds, respectively).

These correlations are encouraging in that there are reasonably high correlations between a potential baseline measure (percent of accidents with African American drivers) and measures of police behavior (percent of citations issued to African Americans). The correlation between the percentage of drivers in accidents who are African American and the percentage stopped for speeding who are African American is .865 (.874 if I-85-RD is omitted). Thus, there is a slightly higher correlation between the percentage of African American drivers in accidents and the

⁶⁴ This comparison of a highway segment with what we call a highway area is generally comparing a 10 to 15 mile stretch of highway with a longer stretch of highway that includes the 10-15 mile segment. The longer highway we estimate to generally be two to three times longer. Also, it should be noted that we are counting all accidents from 1998 through 2000 in the highway

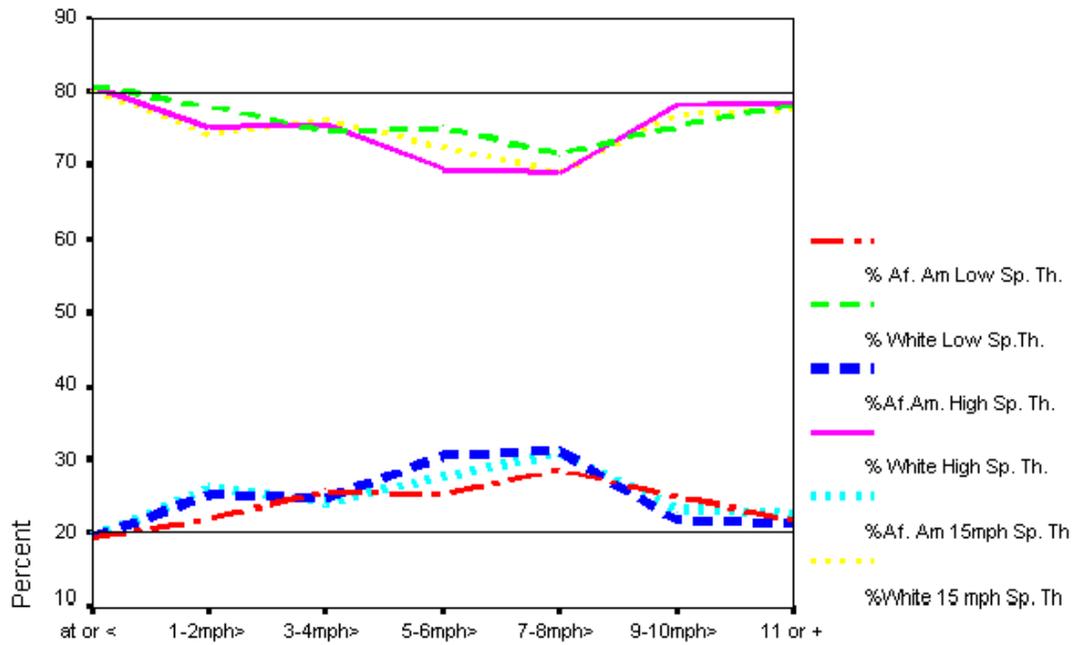
percentage stopped for speeding than there is between the percentage of threshold speeders who are African American. Note here that the number of highway segments studied is only fourteen so the correlations are highly susceptible to fluctuations. In any event, there is an empirical basis for expecting a reasonably strong relationship between accidents and NCSHP intervention in an area.

In our analysis, we looked at the stop records for May through July, 2000, for daytime hours—approximating the hours that we observed speeders (weekdays, 9 a.m. to noon and 1 p.m. to 5 p.m.)— and found that, for example, 29.4 percent of the drivers stopped for speeding on I-95-N (see table) were African American, a disparity in the direction of bias against African Americans. In seven of the fourteen districts, relative to the conservative threshold, disparity in the direction of hypothesized bias is found, but in five districts there is more speeding observed of African Americans than there are stops of African Americans (one district has unreliable data, and one district is a tie). Using the liberal risk of speed threshold, we find that all but two highway segments (U.S.-L and I-40-J) have fewer African Americans speeding above the threshold than those who are stopped.

It should be noted, however, that we do not know much about the selection process by which troopers decide to pull over vehicles for speeding. For example, it is likely that not every driver observed speeding at the threshold value gets stopped, but those that speed well above the threshold values are more likely to be stopped. We would need data on what troopers actually observe to make a better assessment of the real threshold values that trigger stops. Such values may vary with how busy the trooper has been that day, or how long it has been since a trooper has

pulled over someone. It seems likely, however, that the more grievous the speeding violation, the higher the risk of someone being stopped.

Figure G.1 Percent of Drivers Speeding More Than the Speeding Thresholds by Race



MPH Over Various Speed Limit Thresholds

Reference Line @80% = % White Overall

Line @20% = % African American Overall

In Figure G.1 we show that, across all fourteen highway segments of the observational study, African Americans make up a higher proportion of the “threshold speeders” (up to 30 percent) than they make up of all drivers on the highway (20.2 percent).⁶⁵ Threshold speeders here is defined three ways: low threshold of speeding (at or above the first decile of speeds at which drivers are cited), high threshold speeding (at or above the median at which drivers are cited) and a flat fifteen mph above the posted speed limit. (we added the 15 mph above posted speed as a threshold because it is a standard used in a New Jersey study— Lange et al., 2001). For example, we see that African Americans make up approximately 30 percent of those traveling 7-8 mph above all three speeding thresholds, while they make up only 20.2 percent of those traveling on the highway (the flat reference line at approximately the 20 percent mark on the vertical-axis). We can see that African Americans are over-represented among those who are speeding above the point at which speeding citations are likely. We can say that this pattern is “robust” across definitions of speeding thresholds in the sense that the pattern is the same regardless of how the thresholds are defined. At the top of the graph are three lines representing the percent white of the total number of all white and African American drivers. Thus, the percent white represents a complement to the percent African American (the two percentages must sum to 100).

Note, however, that the proportion African American among the threshold speeders does not rise continually across the speeding values on the horizontal-axis. Thus, the pattern is dissimilar to that shown by researchers in New Jersey. They found that African Americans made up a progressively higher percentage of the speeders at the higher speeds (Lange et al., 2002).

⁶⁵ The percent reflects a count of all drivers observed who were African American or white. Drivers judged to be Hispanic or Other are excluded from the computation of the rate.

Here, however, we found that the pattern is not a steady incline but rather a curvilinear pattern of initial incline (up to 7-8 mph above the speeding thresholds), and then a decline to where the proportion of threshold speeders who are African American approaches roughly 20 percent (the same percent of drivers observed on the highway who are African American). Note that Lange and colleagues did not present measured speeds above 89 mph, whereas we are presenting speeds up to 96 mph (at least where the speed limit is 70 mph and the speed threshold is 85 mph). It should be noted, however, that we do not measure speeds very accurately above 90 mph.⁶⁶ Thus, it is possible that the Lange data, if presented for higher speeds, might show a pattern similar to ours.

⁶⁶ Note that Lange and colleagues present their data only for a speeding threshold of 15 mph above the posted speed limit. They report a continual increase in the percent of speeders who are African American up to 89 mph. We use three different speed threshold measures, and all three show the same pattern: a decline in the percent African American after 7-8 mph above all three speeding thresholds. The value of “11 or more” mph above the speeding threshold for a 15 mph threshold is, of course, equivalent to 26 mph above posted speed (which varies from 55 to 70 in our observational study). Thus, the highest speed that could be included in our graph is 96 (70 + 15+11).

References for Appendix G

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