

# A Critical Reanalysis of Maryland State Police Searches\*

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## **Abstract**

Knowles, Persico and Todd (2001) develop a theoretical model of police search outcome and a test statistic for racial bias in police searches. We argue that their empirical analysis of the Maryland State Police search data is questionable for several reasons. We find that non-stationarity of the data preclude the use of standard statistical inference techniques, and that proper statistical graphics are better suited to capturing the complexities of the racial bias issue.

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## 1. Introduction

Pursuant to a 1992 racial profiling lawsuit filed against them, the Maryland State Police (MSP) were ordered to maintain comprehensive records on all motor vehicle searches occurring along interstate I-95, starting in January 1995. The data were collected to determine if the MSP were using race as a basis for vehicle searches. Based on a limited analysis of these data, the American Civil Liberty Union (ACLU) claimed that the MSP were racially profiling drivers, and a bitter legal dispute between the two groups ensued. Since then, racial profiling has received much local and national press coverage.

The lawsuits against the MSP were the result of several highly-publicized incidences of police searches of vehicles driven by African-Americans. The first case, that of Robert L. Wilkins, led to the first lawsuit filed against the MSP. Part of this settlement was that data collection by the MSP along I-95 be compulsory. These are the data (up to January 1999) that this paper seeks to reanalyze.

Based on this and other incidents, the ACLU repeatedly (and successfully) sued the MSP. Accompanying witness testimony, there was a benchmarking analysis performed by psychologist John Lamberth, then a faculty member at Temple University. In benchmarking analysis, the percentage of African American motorists stopped or searched is compared to some relevant population percentage (benchmark). In this case, Lamberth (1996) found that while African-American motorists comprise 30% of the drivers stopped and 73% of the drivers searched by the MSP, they only make up about 17% of the drivers on I-95 between

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Baltimore and Delaware. Moreover, in its 2001 case against the MSP, the ACLU singled out two troopers (by name) who searched vehicles of African-American motorists *exclusively*. Other troopers were identified as searching African American motorists' vehicles at rates exceeding 80%. (Note, however, that the total number of searches conducted by these troopers is not mentioned in the report.)

Aside from the analysis of Lamberth, other scholars have analyzed the MSP data. For example, see Becker (2004), Dharmapala and Ross (2004), Gross and Barnes (2002), and Barnes (2005). The earliest scholarly treatment in the economics journals appears to be Knowles, Persico, and Todd (2001), hereafter KPT. KPT develop a theoretical model of police search outcomes and a test statistic for racial bias in police searches, which they apply to the MSP data. The basic idea is that, in equilibrium, unbiased police searches will result in equal guilt rates across races. A lower guilt rate for African-Americans (Caucasians) is indicative of discrimination against African-Americans (Caucasians). Using a Pearson type test statistic, they determine that the overall guilt rate for blacks (32%) and that of Caucasians (34%) are not statistically different.

The present article is a critical reanalysis the MSP data collected between January 1995 and January 1999 (those analyzed by KPT). In doing so we uncover some pitfalls of drawing statistical conclusions from these data, and we argue that previous work has overlooked certain aspects of the data which we believe to be important in order to analyze them properly. We show that complex dynamics in these data preclude the use of standard statistical inference techniques, and simple graphics capture far better the many facets of the issue. In

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particular, we argue that most statistical analyses rely on inadequate assumptions and that, ultimately, racial profiling is far too complex an issue to be characterized by a few descriptive statistics that supposedly represent the truth. We conclude these data themselves cannot properly address the issue of racial profiling, and we make recommendations on ways to improve the outcomes of future police profiling studies.

We reveal some interesting patterns in the data that have gone overlooked and that may inform our understanding of racial profiling. A final section states our conclusions and recommendations.

### 2. Some Specific and General Criticisms

Our primary criticism of the KPT results is that the timing of several events in the case created incentives for the MSP to alter their search behavior during the period when the data were being collected, and it is unlikely that the data were generated from the traditional sampling mechanism modeled by independent and identically distributed random variables.<sup>1</sup> While a simple graphical examination of the data (see the plot hereafter and the next section) is, in the view of the authors, enough evidence to support this claim, it is also possible to devise simple tests which show that the iid assumption is unlikely to hold. For example, given a search and that a driver is either African-American (black) or Caucasian (white), the race of the driver follows a Bernoulli distribution. Consider the probability  $p$  that the driver is black. One could calculate exact confidence intervals for this probability at the

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<sup>1</sup> Gross and Barnes (2002) give evidence that the behavior of the MSP changed over time, but fail to point out that it is unlikely that we are observing a traditional sampling mechanism that lends itself to standard testing.

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level  $1 - \alpha$  for two separate and distinct periods in the data. If those confidence interval are disjoint then  $p$  is significantly different in the two periods, with a probability of being equal at most  $\alpha$ . A calculation on the data shows that the probability that  $p$  was constant over the two periods is less than  $10^{-7}$ , clear evidence that the standard sampling model does not apply.

The implication of this non-stationarity is that KPT's test of racial bias is not valid.<sup>2</sup> One could argue that some correlation in the data induces a larger variance and therefore may make the KPT valid. This is quite speculative, for one can also construct models with stronger correlation which makes a Pearson test as large or as small as desired.

A secondary point is that KPT use an *incorrect* limiting distribution for their test statistics. Indeed their statistic is not the usual Pearson chi-square test, for they replace the expectation of the guilt rate conditional on the race by the guilt rate estimated over the whole data set; it can be shown that the limiting distribution of their test statistic is that of a weighted sum of independent chi-squares, which is different than what they intended. However, even with the correct test statistic and an exact distribution, the data still indicate the MSP were not racially biased in searches (under the standard sampling model with independent and identically distributed observations), so in the framework of KPT, this is only a minor methodological issue.

On a more philosophical note, one also wonders if the appropriate measure should correspond to KPT's "equality of guilty rates" or *approximate* equality of these rates. It is

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<sup>2</sup> In a similar vein, Hernandez-Murillo and Knowles (2004) point out that when "statistics conflate searches involving different levels of police discretion, standard tests for racial bias are not applicable."

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conceivable that slight differences would be tolerated, which immediately raises the question of what is an admissible difference? In the same vein, why is KPT's 5% error rate acceptable? It certainly is a useful rule of thumb in some fields; however, if a society is unwilling to tolerate racial profiling, why is it willing to risk being wrong 5% of the time? Furthermore, we are reminded that classical testing theory is asymmetrical in the hypotheses and designed to favor the null. Therefore, from the ACLU vantage point, we should not test whether the guilt rates are equal, but whether they are different. A fine point which seems to have been overlooked, is that if we accept the basic framework of Knowles, Persico and Todd, then one can use an exact test (with critical values obtained via simulation) to show that the observed guilt rates are compatible with a difference of 10% between them at the 5% level. (Results are available from the authors.) Put another way, since the Caucasian guilt rate appears to be around 30%, this means that the guilt rate for African-Americans could be as low as 20%, which is 1/3 less (not a small difference) than that of Caucasians. Hence, from the ACLU perspective, one could argue that the data show a difference of at least 10 percentage points between the African-American and the Caucasian guilt rates. Put in more technical terms, given the amount of data, the power of the test is rather weak in distinguishing differences of about 10 percentage points.

However, in our view, the non-stationarity of the data prevents all classical testing from being used. One could of course envision using specific time series modeling techniques, but it is hard to evaluate the effect of the modeling on the conclusions, and it is not certain that proper modeling, capturing all the complexity of these data, is even feasible. For all these

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reasons, we feel that a good description of the data is more enlightening than traditional testing. Moreover, we feel that simple plots capture the complexity of and broaden the scope for participation in the discussion about racial profiling. Our techniques allow us to form hypotheses, and leave the readers to decide, based on their own social and political beliefs, whether the MSP used racial profiling or not, and if the legal actions were successful in correcting any unacceptable behavior on the part of the MSP. This relativistic conclusion may be unpractical, for one needs to reach a consensus on the racial profiling issue. However, it is a realistic conclusion: one should simply compare the conclusions of Lamberth's study, with those of KPT, and with those of Barnes (2005), and keep in mind that a simple switch of the null and alternative hypotheses leads to different conclusions based on the same data and the same test statistics, or Gross and Barnes (2002) to realize that the choice of the metric to gauge the problem seems to determine the conclusion.

### 3. Graphical Reanalysis

Since their main finding is summarized earlier in this paper, we do not reproduce KPT's results here. Instead, we attempt to shed some new light on the debate by identifying and exploiting the time dependency of the data. Due to the small number of "Hispanics" and "Others" in the data, we focus only on search differences between African-Americans (henceforth, blacks) and Caucasians (henceforth, whites).<sup>3</sup> Figure 1 illustrates the time dependency of the data. It is a monthly plot of various quantities upon which we now comment. Our comments follow the graphics in the figure from bottom to top. The time

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<sup>3</sup> We do, however, consider "hispanics" and "others" in a graphical analysis of the largest drug seizures.

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axis at the bottom of the figure starts on January 1995, the beginning of the data collection, and ends on January 31, 1999. Various events are indicated along the axis. The unit of time is one month. "Years" are indicated by both the light gray vertical lines and the labeling at the top of the figure.

The bottom display presents the number of searches, per month. As one can see, the intensity of the searches varied over time. The first three months were likely a transition period for the MSP to implement the data collection. As noted by KPT, one sees that the intensity of the searches, that is, the number of searches per month, varied greatly and seems to suggest three separate and distinct periods. The first period goes from the beginning of the data collection effort until about March 1996, where search intensity was high. In the second period, starting around March 1996, search intensity falls, until about the beginning of 1998, around the time that officer Hall ordered his troops to file reports on all searches. The third period is marked by higher search intensity and runs from the beginning of 1998 until the end of the data series. One could associate the lower search intensity of the second period with intentional under-reporting of the police, but other aspects of the data (to be seen later) do not support this presumption. It is also interesting to note that in the third period, the intensity of the searches is lower than in the first period. Why did the intensity fluctuate? It could be due to reorganization of the police force, or a change in MSP behavior under pressure of various legal actions, or, perhaps, there is some other explanation. We cannot tell from the available data.

Continuing to examine the search intensity panel, the darker bars indicate the number

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of searches where the location could not be accurately determined. As one can see, it is surprising that those records correspond to searches occurring mostly in two clusters of time. There are a few searches of undocumented location in April, May, and June of 1995, however most of the searches of undocumented location are grouped together between November 1995 and March 1997. We do not know the reason for this change in police reporting, but it is striking. Was it because the MSP changed its reporting forms? Could it be due to a turn-over in the police force, with new officers not well instructed on the use of the forms? Could this be intentional mis-reporting? We have not found any rationale in the data for this clustering.

The next panel for consideration is the percentage of African-American searched (this corresponds to KPT's Figure 2, panel a). The trend over time shows a reduction in this percentage and, perhaps, a reduction in any perceived racial disparity. Note that the time-evolution in this panel shares some similarities with the time-evolution of the search intensity panel: as search intensity decreased in the early phases of the data collection, the percentage of black searches declined. It is also interesting to note that the highest proportion of blacks observed in the data set occurred prior to 1997. In fact, 65% of searches of vehicles driven by African-Americans were recorded in the first half of the data. While it is clear that the MSP were originally targeting blacks in searches, one has to acknowledge that the practice stopped, likely due to the legal actions.

A monthly analysis of the guilt rate by race provides additional information and is the subject of the next panel (just above the "percentage of African-Americans searched" panel).

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According to Knowles, Persico and Todd, racial bias is revealed by racial disparities in guilt rates conditional on search; a higher guilt rate for blacks indicates a search bias against whites, and a lower guilt rate for blacks indicates a search bias against blacks. Therefore, we can examine the ratio of the guilt rates of blacks to whites to reveal racial bias. This ratio is plotted monthly in logarithmic scale. It is truncated at  $1/8$  and  $8$ . It appears that Knowles, Persico and Todd are correct to assert that over the entire data series there is no significant difference of guilt rates, but this may have not been the case over time.<sup>4</sup> With the noticeable exception of 3 months in 1995, the guilt rates were quite similar until about March 1996. Whites had a higher guilt rate in April 1996, after which blacks had a higher guilt rate until about December 1996 (indicating bias against whites). Then a period of large fluctuation in the guilt rate follows. Starting in August 1997, the guilt rates are nearly equal.

This raises several interesting questions. Is the increase in the black guilt rate in May 1996 related to Officer Hall's memo concerning unequal treatment by race? Following the arguments of Knowles, Persico and Todd, this implies that in May of 1996 the MSP started to discriminate against white drivers. This change occurred around the time when black search intensity declined. Were police only inclined to search blacks when guilt was nearly certain? In other words, in altering the treatment of blacks but not that of the whites, the MSP may have achieved higher guilt rates for the blacks, implying bias toward whites in the KPT framework. Was the change in guilt rates an attempt to level the playing-field across races?

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<sup>4</sup> KPT did disaggregate the data into three subperiods, and did conclude that guilt rates were statistically equal within each subperiod. However, our graphic disaggregates the data to an even finer level (monthly) to "test" equality of guilt rates.

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The next panel for discussion is the overall guilt rate. It is consistently high compared to guilt rates in other states. Typically, a 10% guilt rate is considered good, but the MSP are almost consistently above this benchmark. This consistency, combined with the previously discussed panel and put in the framework of Knowles, Persico and Todd, implies that the MSP were, in fact, biased against the whites, and that the effect of the legal remedies, intended to correct bias against blacks, ultimately led to bias against the other group! Also, as the number of searches went up in June 1998, the guilt rate of the blacks went down. While one might consider this drop as supporting the claim that the MSP were under-reporting some searches in order to boost its success rate, our analysis of large seizures will show that this interpretation is, in all likelihood, incorrect. From the end of 1995 to the first quarter of 1998, the overall guilt rate trended up, indicating that the MSP were becoming more successful at finding drugs.

However, the next panel, the amount of drugs seized, tells a more nuanced story. It is based on a variable which, when properly recorded, contains the actual weight of the drugs recovered. The variable was not recorded in 8 cases. For 15 other cases, the amount is recorded in units other than weight, such as packs or vials of unknown volume. A few records contained data like "trace amount," "1 seed" or "1 cigarette (joint)". For these records, we set the amount to either 0 or 1 gram per seed, or 1 gram per cigarette. Therefore, the weight was either recorded by the officer or imputed for all but 32 records. The discussions that follow are robust to our imputations.

Since a few seizures were exceedingly large, we calculated the median amount recovered

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for each month. The searches were fairly successful until the end of 1996, with a couple of other successful months in 1997. However, essentially starting in 1997, most searches lead to only a small amount of drugs recovered. Based on our observations to this point, our impression is that as time passed the MSP restricted its searches to the most obvious cases, targeting users (as opposed to dealers) traveling with their drugs in plain sight. In doing so, it slightly increased its overall success rate. It also decreased the intensity of the searches. If this interpretation is correct, we do not know if this so called "depolicing" was intentional or if it was a unforeseen consequence of the legal actions taken against the MSP.

Without interviewing the MSP, we cannot formally test our hypothesis that the MSP focused its search efforts on the most obvious cases. However, we can add further support to its validity by examining the reason for the searches. In some cases there was little doubt that a search would recover drugs (e.g., cases where the police officer indicated that drugs were in plain view or there was an odor of burnt marijuana). There are also borderline cases, like "drivers eyes bloodshot" or "physical appearance indicates that driver recently smoked marijuana," and there are cases where no obvious suspicion is indicated, such as "nervousness." Therefore, we re-coded this variable into three categories "obvious," "not obvious" and "maybe obvious". Clearly the second category, "not obvious," corresponds to cases where the MSP exercise some subjectivity in the reason for search. Also, the third category, "maybe obvious," is marked by some level of police subjectivity. Figure 2 shows a time plot of the total percentage of "not obvious" (light shaded bars) and "maybe obvious" (dark shaded bars) searches, along with the median amount of drugs seized (vertical lines).

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It seems clear that there is a positive correlation over time between the level of subjective search and the median amount of drugs seized, and that there is a decrease in subjectivity (and the size of seizures) over time. This supports the hypothesis that in the third period of the data the MSP searched mostly "small-time" users with their drugs plainly in sight at the time of the stop. However, there is also a decrease in 1998 which is contemporaneous with series of memos sent by MSP Officer Hall to field officers regarding sensitivity to racial profiling, and for which we do not have a good interpretation.

The next (second from the top) panel in Figure 1 shows the time of the day of each stop leading to a search. For each three-month period (quarter), we display an histogram of the time documented in the data. It is not clear if reported time is when the stop was made, ended, or some other time. Also, we do not have any data on the duration of the stops. This duration probably varies greatly according to the extent of the search and whether the driver is arrested. The main pattern, already observed in Barnes and Gross (2002), is that the MSP searched mostly during non-rush hours. There is a slight pattern of searching more at night starting from the second quarter of 1996 up to either the first or the third quarter of 1997. This coincides in part with the period where most records had no well-documented location. Overall, if we say that a search occurring between 8 p.m. and 6 a.m. occurs at night, while others occur at daytime (an obviously crude definition), there is no well-documented location in 40% of the searches occurring at night and no well-documented location in 30% of those occurring the daytime; this difference may make sense, for it is obviously more difficult for an officer to see a mile post at night than during the day.

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The top panel of Figure 1 shows where the searches occurred on I-95 over time; of course, this is restricted to the records for which the location could be readily observed. Throughout the period, most searches occur in the last 30 miles or so of I-95 (in the northern part of Maryland). However, there is an interesting variation between the first and last part of the data. Indeed, in 1995, 90% of the searches occurred in Cecil and Harford counties (in the north), while this proportion was 73% starting in 1997 and onward. We see that starting in late 1996, the MSP spread its searches more along I-95, probably under the pressure of the current and pending legal action.

Another interesting feature of Figure 1 is the impact of the various events (at the bottom of the figure) on search behavior. Memos by MSP Officer Hall in 1998 seemed to encourage an increase in searches and/or (perhaps) reporting. Various legal actions (documented in Gross and Barnes, 2002) are likely to have had an impact in the long-run, while there seem to be no obvious short-term effects, except for the legal action leading to the start of the data collection. The end of a specific police force, STIF, does not coincide with any obvious feature, but it could be that this end was in fact scheduled or discussed as early as March 1996, and this may have produced the drop in search intensity around that time.

Figure 3 shows the data associated with large seizures, those for which more than 100 grams of drugs, regardless of the type, were recovered. The horizontal axis is time (with the same scale as that of Figure 1). The vertical axis is the weight of drugs recovered in logarithmic scale. Hence, the coordinates of a point on this graphic indicate when the seizure took place and how much was seized. The observations pertaining to a "white" driver, an

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"Hispanic" driver or an "other" driver are circled, squared and crossed, respectively.

The patterns in Figure 3 are striking. There were very few large seizures recorded after the second quarter of 1997 (3/4/97 in the figure). Prior to then, large seizures account for 13% of all searches and 43% of successful searches. After this quarter, they account for only 3% of all searches and 9% of successful searches. This further supports the hypothesis that in the third period of the data the MSP targeted mostly "small-time" users and avoided aggressive searches of large dealers. In term of racial composition (illustrated by the boxes below the time-plot), large seizures from white drivers are more prevalent in the second half of the series (4% prior to 3/4/97 versus 17% after). The graphic also shows that blacks are over-represented in large seizures, regardless of what benchmark is used, however this is not to say that blacks are more likely to carry large amounts of drug. One possibility is that the MSP developed a better profile of black drug couriers than of white couriers. This interpretation, if correct, could reconcile both parties' views of the data and results. Blacks are simply over-represented in the large seizures, because police have a better understanding of what to look for from a black drug courier than a white drug courier. This could certainly be a differential treatment based on race, but it may simply be due to asymmetry of information and not due to discrimination or racial bias (in the most nefarious use of the terminology). From the guilt-rate perspective of Knowles, Persico, and Todd, the MSP were discriminating against whites, because they tended to be less successful with searching whites than blacks. From the ACLU perspective, the MSP were targeting mainly blacks because they were more successful at it (which is not to exonerate troopers whose record show that they were searching only blacks

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or whose behavior demonstrates, at best, a total insensitivity toward the dignity of human beings). From the MSP perspective they may not have felt that they were racially profiling blacks, because they were merely responding rationally to the informational asymmetry.

Another point of contention is the prevalence of searches on the south-bound side of I-95. The MSP argued (without much evidence) that this is the direction of the drug flow. Gross and Barnes give supporting evidence of the police claim. Further evidence is contained in the data on searches leading to large seizures: 94% of the seizures of at least 100 g are on south-bound side, and 92% of the 26 seizure of at least 1 kg are on the south-bound side. This is to be compared with the two-thirds of all searches occurring on the south-bound side. Gross and Barnes (2002) suggest that the MSP are less likely to under-report large seizure of drug. If that assertion is correct, the data for large drug seizures and the bottom panel on Figure 1 suggest that most searches were indeed reported by the MSP (though, again, it is documented that some searches were not reported).

### 4. Conclusions.

Several features of the MSP data lead us to the conclusion that they are unsuitable for standard hypothesis testing. Our primary criticism of previous analyses is that they do not account for non-stationarity of the data and rely on statistics themselves relying on assumptions, whose choice is just as subjective as an interpretation based on the raw data properly plotted (if not more so). The non-stationarity may exist for a variety of reasons, but it is likely that changes in the behavior of the MSP were linked to the timing of various

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events in the data (e.g., the issuance of memos by police officials during the period of data collection). We do not profess to fully understand the true causes of the non-stationarity, however the time dependence of the data is often clear. This non-stationarity may also invalidate any attempts to model police behavior, using a specific functional form (e.g., regression) that does not account for it.<sup>5</sup> For these and other reasons (articulated above), we feel that a good description of the data has been more enlightening than rigorous testing or modelling.

Unfortunately, there is no clear solution for understanding racial profiling (or police bias) using these data aside from our simple descriptions, and these descriptions do not lend themselves to a definite answer to the question of whether or not the MSP were biased in search. We have also argued that the data support both the ACLU's and MSP's perspectives on the issue, which is not surprising given the quality (or lack of quality) of the data. However, we hope to have shed some light on issues that are important to the discussion of understanding police behavior that have gone, otherwise, unnoticed. We also hope to have sufficiently pointed out the deficiencies of too quantitative analyses for policy makers to consider them with skeptical eyes.

We hope that future attempts to understand these behaviors not be undertaken in the context of litigation, since it will undoubtedly cause police to alter their behavior more than if the data collection and analyses are done as part of "standard procedure." Indeed, the cost structure of their behaviors are markedly changed when legal actions are a perceived consequence. Analysts would be more likely to observe more typical police behavior in

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<sup>5</sup> Gross and Barnes (2002) use regression techniques to analyze these data.

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the context of a study that was part of a regular police policy on auditing data. Second, audit mechanisms should be designed to ensure that *all* police stops and frisks are recorded. Precisely how this would be accomplished is unclear, but its importance is underscored in the on-going debate between police and citizens groups about the validity of the reported data. Third, overall data accuracy must be improved. This can be accomplished by the use of electronic forms that reject improperly coded data (this was done in the City of Syracuse, NY in a 2005 study performed by the second author). Also, geo-positioning equipment in police vehicles could automatically record locations of stops and frisks, which were a problem in the MSP data. Finally, the data collected are only from the perspective of the police officer. A survey of the citizens stopped could be performed to better understand their perceptions of police behavior. This could be as simple a survey issued to all stopped (and unarrested) citizens which would be completed and mailed subsequent to the stop, or as complicated as a random selection of citizens from police data bases that is followed with a timely survey or telephone interview.

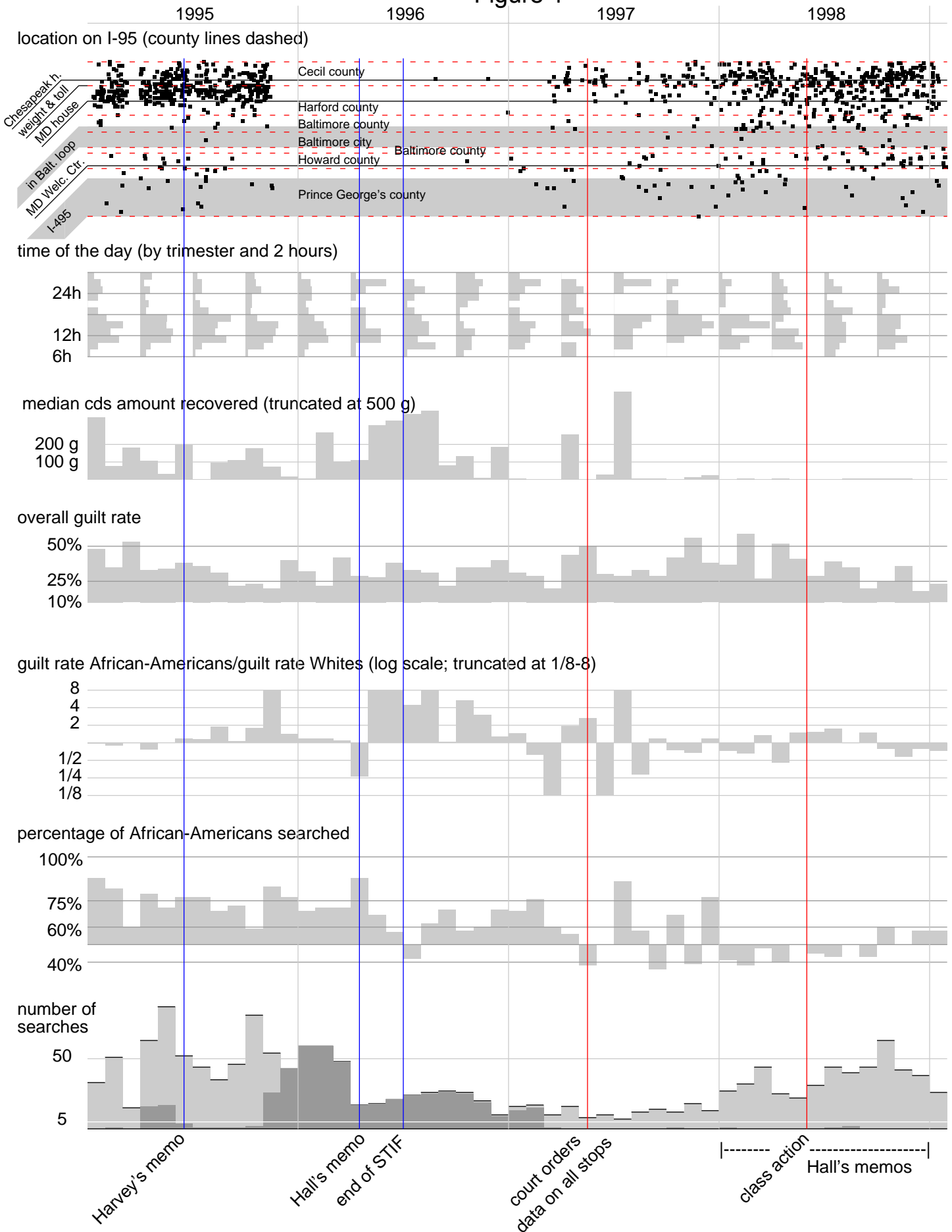
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Figure 1



% of nonobvious searches and median amount of cds recovered

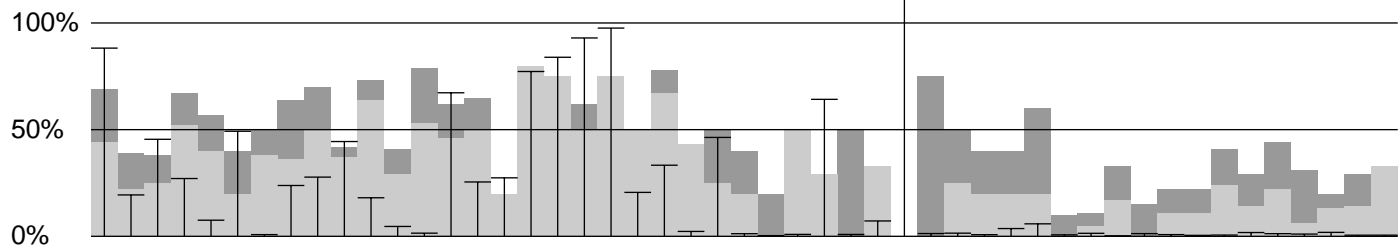
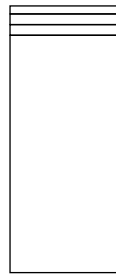
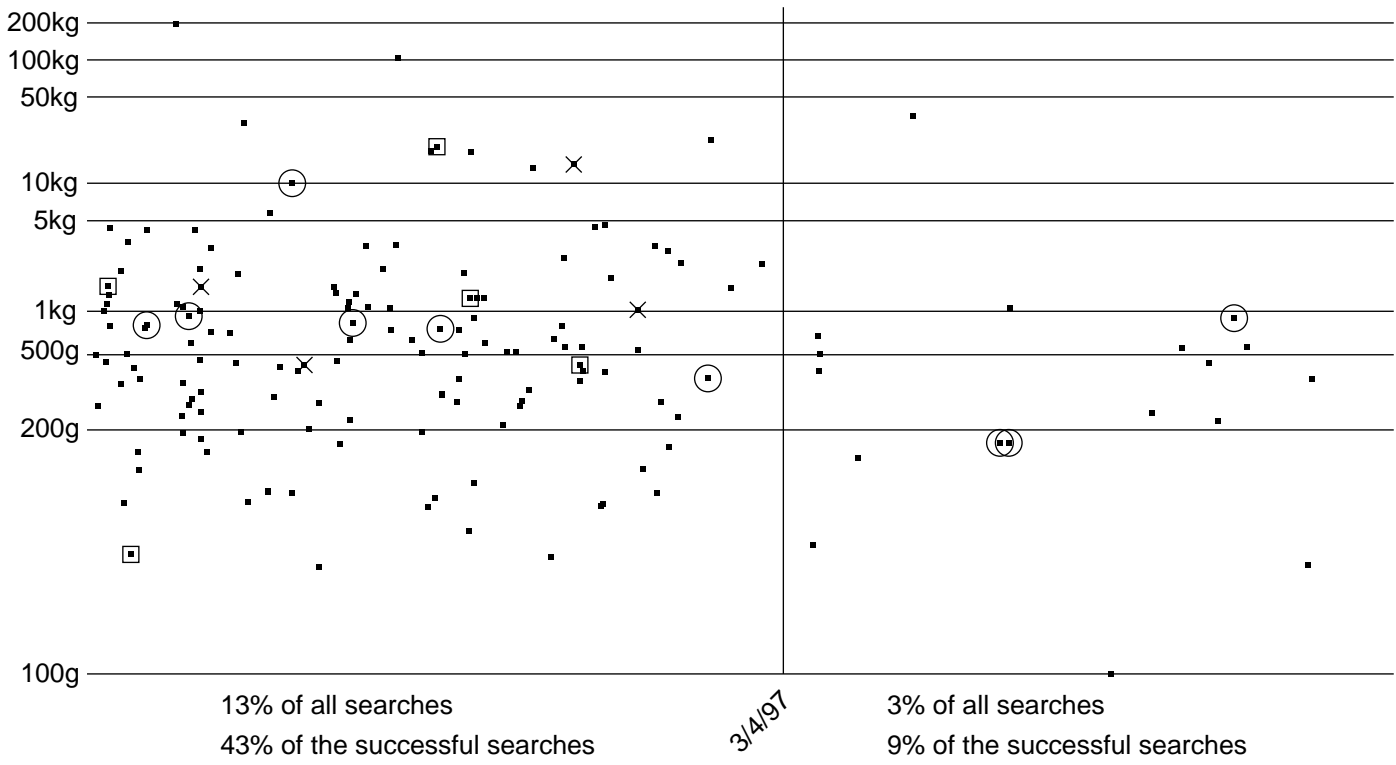


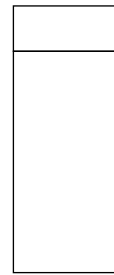
Figure 2

Figure 3

More than 100g recovered (logarithmic scale, white drivers circled, hispanics squared, other crossed)



A : 89 W : 4 H : 4 O : 3 %



A : 83 W : 17 H : 0 O : 3 %

A = Black; W = White; H = Hispanic; O = Others.