UNITED STATES DISTRICT COURT
SOUTHERN DISTRICT OF NEW YORK

DAVID FLOYD, *et al.*,

Plaintiffs,

-against-

THE CITY OF NEW YORK, *et al.*,

Defendants.

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08 Civ. 01034 (SAS)

DECLARATION OF JEFFREY FAGAN

JEFFREY FAGAN declares as follows pursuant to 28 U.S.C. § 1746:

1. I am the Isidor and Seville Sulzbacher Professor of Law at Columbia University Law School, Professor of Epidemiology in the Mailman School of Public Health at Columbia University, a Senior Research Scholar at Yale Law School, and Director of the Center for Crime, Community and Law at Columbia Law School. I am a Fellow of the American Society of Criminology. I have been retained by the Plaintiffs in this action as a testifying expert.

2. I submit this declaration in Opposition to Defendants' Motion to Exclude Plaintiffs' Proposed Expert Reports, Opinions and Testimony of Jeffrey Fagan.

3. As part of my work as a testifying expert, I conducted a series of statistical analyses of the New York Police Department's (NYPD) UF250 Stop-and-Frisk Data for the years 2004-2009. The results of these analyses and the statistical methods and data which I used are described in detail in my expert report, dated October 15, 2010 ("First Report"), my supplemental expert report, dated December 3, 2010 ("Supplemental Report") and below.

4. Based on my statistical analyses, I have the following conclusions:

a. The racial composition of a precinct, neighborhood, and census tract is a statistically significant, strong and robust predictor of NYPD stop-and-frisk patterns even after controlling for the simultaneous influences of crime, social conditions, and allocation of police resources.

b. NYPD stops-and-frisks are significantly more frequent for Black and Hispanic residents than they are for White residents, even after adjusting for local crime rates, racial composition of the local population, police patrol strength, and other social and economic factors predictive of police enforcement activity.

c. Blacks and Latinos are significantly more likely to be stopped by NYPD officers than are Whites even in areas where there are low crime rates and where residential populations are racially heterogeneous or predominantly White.

d. Black and Hispanic individuals are treated more harshly during stop-and-frisk encounters with NYPD officers than Whites who are stopped on suspicion of the same or similar crimes.

e. More than 170,000 stops, or 6.41% of all stops (6.71% of non-radio run stops, and 5.26% of radio runs), recorded by NYPD officers between 2004 and 2009 were Unjustified.

f. For more than 400,000 stops, or approximately 15%, the corresponding UF250 forms do not provide sufficient detail to determine the stops' legality.

g. The NYPD's reliance on information provided by officers on UF250 forms to assess whether stops are based on reasonable articulable suspicion is an ineffective way to regulate the constitutionality of officer stop-and-frisk practices.

h. NYPD officers' frequent and indiscriminate use of the "furtive movement" and "high crime area" stop factors, and the lower hit rates for stops based on those factors, suggest that neither of those stop factors, as used by NYPD officers, is a valid marker of reasonable articulable suspicion and that both factors are often used to provide an erroneous post-hoc justification for stops made without reasonable articulable suspicion.

i. The rates of weapon and contraband seizure in stops-and-frisks conducted by the NYPD are no better, and often times are lower, than hit rates found in cases involving random police checkpoints, where stops are made at random, without reasonable suspicion or probable cause. This low rate of seizures of weapons or contraband in stops-and-frisks by NYPD personnel suggests that the NYPD's stop and frisk program produces gun and contraband seizure rates that are no greater than would be produced simply by chance.

j. Between 2004 and 2009, UF250 forms are progressively less likely to articulate a valid and specific "suspected crime" that motivates stop activity. The rate at which invalid values are entered into the field increases from approximately 1% in 2004 to over 35% in 2009. As the

prevalence of invalid "suspected crime" entries increases over time, so too does the number of stop factors indicated.

k. The increase in the use of several of the more discretionary Stop Circumstances and Additional Circumstances (e.g., "furtive movements", "evasive response to questioning") on the UF250 form among officers over time, independent of the actual crime or stop patterns, indicates that the documentation of reasonable articulable suspicion among officers become a patterned behavior that is increasingly shared as part of a narrative or *script* of suspicion.

5. In addition, I have reached the following conclusions concerning statistical evidence which Defendants may offer at trial in this case:

a. The RAND Corporation's 2007 analysis of racial disparities in the NYPD's stop-and-frisk practices is so methodologically flawed that it cannot constitute reliable statistical evidence of the absence of racial bias in NYPD stop-and-frisk activity.

b. The statistical studies on the crime control effects of the NYPD's Operation Impact Program and stop-and-frisk, which are attached as Exhibits D and E to Smith's expert report, suffer from several methodological flaws, which are listed at pages 22-25 and 27-32 of my Supplemental Report, which render his analyses extremely methodologically unreliable.

A. NYS OAG Report On the NYPD's Stop-and-Frisk Practices

6. As reflected in my Curriculum Vitae, attached as Appendix A to my First Report, I have authored or co-authored a number of different statistical studies of the New York Police Department's stop-and-frisk practices. An updated version of my CV is attached hereto as Exhibit A. One such study that I co-authored which was not listed in Appendix A to my First Report but which does appear in my updated CV was the study I conducted for the Civil Rights Bureau of the New York State Office of the Attorney General in 1999. In this study, I statistically analyzed the NYPD's data on approximately 175,000 stops and frisks conducted by its officers between January 1998 and March 1999, focusing specifically on racial disparities in stop rates and the extent to which stops complied with the Fourth Amendment. The results of my analyses are reflected in Chapter 5 of the Attorney General's report entitled *The New York Police Department's "Stop and Frisk" Practices": A Report to the People of the State of New York from the Office of the Attorney General* (1999) ("AG's Report"), which is attached as Exhibit 117 to the Declaration of Darius Charney in Opposition to Defendants' Motion for Summary Judgment (Dkt # 132).

7. In analyzing the extent to which NYPD officers' stops met Fourth Amendment standards, the recorded stops were classified into three categories based on the combinations of stop rationales that officers recorded on the UF250 stop-and-frisk forms which they completed following a stop. The three categories were: (i) stops in which the rationale as stated on the UF250 form established reasonable articulable suspicion, (ii) stops in which the rational as stated on the UF250 form did not establish reasonable articulable suspicion, and (iii) stops where (a) the rationale as stated on the UF250 form did not provide enough information to determine the if the stop was based on reasonable articulable suspicion or (b) the legal status of the rationale was unclear under relevant caselaw. *See* AG's Report at 136-145.

8. The statistical method described in paragraph 7 is an example of what is commonly referred to by social scientists as an event classification model. This model is widely used by crimonologists, policing researchers, and other social scientists to categorize nominal data-such as data on individual police stops- based on shared definitional criteria or attributes in to conceptual categories for comparison and statistical analysis. *See, e.g.,* Earl Babbie, *The Practice of Social Research* 421 (12th ed. 2010); Delbert Elliot and David Huizinga, *Social Class and Delinquent Behavior in a National Youth Panel: 1976-1980*, 21 CRIMINOLOGY 149 (1983).

B. Handwritten Notes on UF250 Forms

9. Any attempt to code and analyze unique handwritten narrative details which officers may have entered on the UF-250's when they checked off the "Other" stop circumstance on page 1 of the form could suffer from multiple sources of error. First, translation of incomplete sentences, shorthand notation with no obvious plain text meaning, and other uninterpretable scribbling would introduce a level of subjectivity into the analysis that would render any meanings of these strings as unreliable.

10. Unlike the Stop Circumstances check boxes, these handwritten notations have no known inter-rater reliability. That is, the same or a similar utterance may have very different intended meanings depending on, among other things, the situation and experience of the officer. Analyses that attributed the same meaning to such similar utterances would risk errors since there is no way to ascertain agreement among different officers as to the meanings of these utterances. No such dilemma exists among the check box circumstances, where training and feedback can and should create a shared meaning of these established categories.

11. Second, there is no method to ascertain when and how these utterances are recorded in terms of the consistency from stop to stop. The recording of these utterances are

subject to influences that may render them fragile and sensitive to the conditions in which the utterances are recorded, including the officer's level of emotional arousal or other mental state factors at varying time points after the conclusion of the encounter. This matters for the reliability of these utterances, since the meaning of an utterance recoded during a field stop or immediately afterward may differ from its meaning if an officer completes the UF-250 form at some further point in time after the stop is concluded when emotional arousal or cognitive sharpening has faded.

12. Accuracy of recall is an enduring validity threat in the compilation of such data, with threats such as telescoping and cognitive distortions introduced when an interaction has been particularly salient, or when the officer is in a state of arousal from an encounter with a suspect. *See* Jennifer Roberts, Edward P. Mulvey, Julie Horney, John Lewis and Michael L. Arter *A Test of Two Methods of Recall for Violent Events*, 21 JOURNAL OF QUANTITATIVE CRIMINOLOGY 175-193 (2005); Laura Campos & Maria L. Alonso-quecuty, *The Cognitive Interview: Much More than Simply "Try Again"*, 5 PSYCHOLOGY, CRIME & LAW 47 (1999)

13. Therefore, there is no statistically reliable way to discern reliable statistical categories from these handwritten notes, or any sample of them, that would generate meaningful information about 2.8 million stops. Any attempt to do so would invite a host of potential biases and errors, and would render any conclusions statistically meaningless.

C. Classification of Stops Recorded on UF250 Forms

14. As set forth on page 50 of my First Report, as part of my RAS analysis, I divided all of the 2.8 million stops that I analyzed into six broad categories based on the combination of Stop Circumstances ("CS's") checked off on page 1 of the UF250 and "Additional Circumstances" ("AC's") checked off on page 2 of the UF250, and then used computer coding

instructions to classify stops in each of these categories as either "Justified", "Unjustified" or "Indeterminate". A copy of a blank UF250 form is attached hereto as Exhibit B. These six categories, and their classifications, were as follows:

a. Category 1: Stops in which at least one of the following three stop circumstances (casing, actions indicative of engaging in a drug transaction, actions indicative of engaging in violent crime) on page 1 of the UF250- which I collectively described as "justified" stop factors- was checked off. Stops in this category were classified as Justified.

b. Category 2: Stops in which at least one of the following six stop circumstances on page 1 of the UF250 (carrying object in plain view commonly used in commission of a crime, suspicious bulge/object, actions indicative of acting as a lookout, fits description, furtive movements, wearing clothes/disguises commonly worn in commission of a crime)- which I collectively described as "conditionally justified" factors- and at least one of the "additional circumstances" on page 2 of the UF250 were checked off. Stops in this category were classified as "Justified."

c. Category 3: Stops in which only one or more additional circumstances on page 2 of the UF250 were checked off. Stops in this category were classified as "Unjustified."

d. Category 4: Stops in which only one conditionally justified factor and no additional circumstance was checked off. Stops in this category were classified as "Unjustified."

e. Category 5: Stops in which two or more of the conditionally justified factors but no additional circumstances were checked off. Stops in this category were classified as "Indeterminate."

f. Category 6: Stops in which the "Other" stop circumstance on page 1 of the UF250 was checked off either by itself, or together with one or more additional circumstances on page 2 of the UF250. Stops in this category were classified as "Indeterminate."

15. On page 50 of my First Report, I inadvertently stated that "Stops are of indeterminate legality if the circumstance or circumstances listed are (all) conditionally justified, and no additional circumstances are indicated." The report should have stated, as reflected in the computer coding instructions and in paragraph 14 above, that stops based on only one conditionally justified stop circumstance and no additional circumstance(s) were coded as "Unjustified," while stops based on two or more conditionally justified circumstances but no additional circumstances were coded as "Indeterminate." Despite the error in the text of my report, the computer coding instructions for stops based on only one conditionally justified circumstance, were and still are correct.

16. In Appendix D of my expert report, I inadvertently stated that stops based solely on "Actions indicative of engaging in drug transactions is not coded as an unconditionally justified stop factor." The computer coding is correctly reflected on page 50 of my First report, where I explain that all stops based on "Drug Transactions" were classified as "Justified." This typographical error in Appendix D does not affect my analysis or results.

17. As stated in paragraph 14, my computer coding instructions originally classified stops based on two or more conditionally justified stop circumstances as "Indeterminate." This

was due to an inadvertent computer coding error, which I have since corrected. Stops based on two or more conditionally justified stop circumstances are now coded as "Justified," regardless of whether an officer also checked off one or more additional circumstances. Under the corrected computer coding instructions, the percentage of stops classified as "Indeterminate" is approximately 15.46%. The percentage of stops classified as "Unjustified" is unchanged from my First Report: 6.71% of non-radio run, 5.26% of radio run, and 6.41% of all stops.

18. According to my review of the 2004-2009 UF250 data, in 4846 stops, 0.17% of all stops, "carrying object in plain view used in commission of a crime" was the only CS checked off on the UF250 form and no AC's were checked off.

D. The Basis for My "Improper Use" Opinion

19. In my report, I concluded that New York police officers' frequent and indiscriminate use of the "furtive movement" and "high crime area" stop justifications raised doubts about whether those factors validly reflect actual RAS. In reaching this conclusion, I started with what is known as a "counterfactual assumption," which is an accepted and frequently used analytic strategy. The counterfactual assumption in this case is a hypothesis that two variables are related, and I conducted statistical tests to determine if there was such a relationship between the indication of "high crime area" and the actual crime rate in the area where a stop took place. *See* Donald T. Campbell, *Reforms as Experiments,* 24 AMERICAN PSYCHOLOGIST 409 (1969). The assumption was that if the "high crime area" stop factor is correctly used by police officers, then it would appear more often on UF250s for stops conducted in high crime areas than UF250s for stops conducted in lower crime areas. However, as I explained on page 53 of my First Report, I found that "high crime area" was checked off as a stop factor in roughly the same percentage, between 53 and 58% of stops conducted in precincts with high crime, low crime, and average crime rates. In addition, as reflected in the Table

attached hereto as Exhibit C, similar results are found at the census tract level. This lack of correlation between the crime rate (the independent variable) and the likelihood that stops are justified with "high crime area" (the dependent variable) supports my conclusion that officers are not correctly using the "high crime area" stop circumstance.

20. In addition, I compared the "hit" rates (i.e, percentage of stops that resulted in an arrest) for stops based on furtive movements and/or high crime area with stops in which neither of those stop factors was present. *See* First Report at 52. Through this comparison, I found that the hit rates in non-high-crime area stops were 22% higher than they were for stops where high-crime area was marked as a factor, and that the hit rates for non-furtive movement stops were 18% higher than for stops in which furtive movement was marked as a factor. *Id.*

E. Multivariate Regression Analysis Benchmarks and Data

21. This is not the first time that I have used the combination of a local population variable and the local crime rate as benchmarks to analyze racial disparities in NYPD stop-and-frisk rates. In fact, I have previously used this benchmark in four published, including two that peer-reviewed, statistical studies that I co-authored which analyzed claims of racial bias in the NYPD's stop and frisk practices. These four studies are:

a. Amanda Geller and Jeffrey Fagan, *Pot as Pretext: Marijuana, Race, and the New Disorder in New York City Street Policing*, 7 JOURNAL OF EMPIRICAL LEGAL STUDIES 591 (2010), a copy of which is attached hereto as Exhibit D (Peer-Reviewed)

b. Fagan, J., *et al*, "Street Stops and Broken Windows Revisted: Race and Order Maintenance Policing in a Safe and Changing City", in S. Rice and M. White, eds., *Race, Ethnicity and Policing: Essential Readings* 309

(2010), a copy of which is attached as Exhibit G to the Declaration of Heidi Grossman (Dkt # 180).

c. Andrew Gelman, Jeffrey Fagan, and Alex Kiss, *An Analysis of the New York City Police Department's Stop-and-Frisk Policy in the Context of Claims of Racial Bias*, 102 JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION 819 (2007), a copy of which is attached hereto as Exhibit E. (Peer-Reviewed)

d. Jeffrey Fagan and Garth Davies, *Street Stops and Broken Windows: Terry, Race and Disorder in New York City*, 28 FORDHAM URBAN L.J. 457 (2000), a copy of which is attached hereto as Exhibit F.

22. I also used a local population-crime benchmark in the analysis which I conducted for the AG's Report. *See* Declaration of Darius Charney sworn to and submitted to the Court in opposition to Defendants' Motion for Summary Judgment, Ex. 117.

23. As I explained in my First Report, one of the reasons I chose to use the local population-crime rate joint benchmark to conduct my negative binomial regression analyses is because these analyses were designed to test the extent to which the racial composition of a precinct, neighborhood, or census tract- separate and apart from its crime rate- predicts the stop-and-frisk rate in that precinct, neighborhood, or census tract. *See* First Report at 30-31.

24. In addition, I chose to use the local population-crime rate benchmark instead of the crime suspect race benchmark in my multivariate regression analyses because the crime complaint report data which had been provided to me by Defendants at the time I was conducting these analyses showed that the suspect's race was unknown in a large percentage of all crimes reported to the NYPD (45%). As I explained in my First and Supplemental Reports, excluding

this large a portion of the crime data would have introduced significant sample selection bias to my analyses had I used the crime suspect race benchmark. *See* First Report at 17-18, 75-77; Supplemental Report at 12; *see also* Berk, R.A, *An Introduction to Sample Selection Bias in Sociology Data*. 49 AMERICAN SOCIOLOGICAL REVIEW 386 (1983); Berk, R.A., A. Li, and L.J. Hickman, *Statistical Difficulties in Determining the Role of Race in Capital Cases: A Re-analysis of Data from the State of Maryland*, 21 JOURNAL OF QUANTITATIVE CRIMINOLOGY 365 (2005); Heckman, J.J. , *Sample Selection Bias as a Specification Error*, 47 ECONOMETRICA 398 (1979).

25. In November and December 2011, approximately a year after I completed my multivariate regression analyses, I received a copy of revised 2009 and 2010 NYPD arrest and criminal complaint report data produced by Defendants. Based on my review of this data and the Declaration of Defendants' Expert Dennis Smith submitted in support of Defendants' motion to exclude me ("Smith Decl.") and attached exhibits, I have learned that while suspect race is now known in a higher percentage of reported crimes, particularly reported violent crimes, the percentage of crimes where suspect race is still unknown, almost 40% across all crime categories, is still high enough that excluding those crimes from a mutlivariate analyses would result in sample selection bias. *See* Berk, R.A, *An Introduction to Sample Selection Bias in Sociology Data*. 49 AMERICAN SOCIOLOGICAL REVIEW 386 (1983); Berk, R.A., A. Li, and L.J. Hickman, *Statistical Difficulties in Determining the Role of Race in Capital Cases: A Re-analysis of Data from the State of Maryland*, 21 JOURNAL OF QUANTITATIVE CRIMINOLOGY 365 (2005); Heckman, J.J. , *Sample Selection Bias as a Specification Error*, 47 ECONOMETRICA 398 (1979). Moreover, using suspect race data for only violent crimes as a benchmark would be extremely uninformative given that, as stated in my First Report, suspected violent crimes are the

basis for only about 14% of all stops-and-frisks conducted by the NYPD. First Report at 75, Appendix C5.

26. I have also reviewed the description and results of the regression analysis conducted by Smith using the crime suspect race data as a benchmark, which are summarized in Smith Decl. ¶¶ 30-31 and Exhibit I. However, because, I understand, Defendants have not provided the computer code Smith used nor specified whether he used suspect race data for violent crimes or all crime categories, it is not possible for me to assess the validity of his results.

27. However, neither of these two sets of crime suspect data would serve as a methodologically sound benchmark. As discussed above, the race of violent criminal suspects is not a valid proxy for the population most likely to engage in the kinds of behaviors that arouse reasonable suspicion in NYPD officers because so few stops (14%) are made by the NYPD based on suspicion of violent crimes. As for data on suspect race across all crime categories, the large portion of crimes in which suspect race is unknown (almost 40%) would result in sample selection bias if this data was used as the benchmark.

28. I have also reviewed the statistics contained in Smith Decl. Exhibit C, which compare, for each NYPD precinct, the racial demographics of persons stopped-and-frisked with the racial demographics of criminal suspects. This exhibit is extremely misleading because it does not state that the statistics on criminal suspects, expressed in percentages, are based only on those crimes in which the suspect's race is known, rather than all crimes reported in the precinct. Since only a portion of crime complaints or arrests include a valid suspect description, basing conclusions on percentages where the denominator (i.e., the base rate on which the percentage is computed) is unknown generates results that are uninterpretable. Smith conceals the number of crime complaints on which the percentages are based. If the denominator is limited because of

missing suspect race information in a large number of cases, these percentages are subject to the same selection bias flaws that were discussed in paragraph 24 above.

29. In his declaration, Smith also claims that my local population-crime rate benchmark fails to account for differing levels of criminal participation by race within a precinct. *See* Smith Decl. ¶¶ 14-15. However this critique ignores the fact that the very small geographic areas which Smith himself claims are the proper spatial units of analysis for NYPD stop-and-frisk activity- e.g., "hot-spots", impact zones, or the census tracts I used in my Supplemental Report.- tend to be racially homogenous *See* Lois Quinn and John Parasawat, *Racial Integration in Urban America: A Block Level Analysis of African American and White Housing Patterns*, *available at* http://www4.uwm.edu/eti/integration/integration.htm at Table 2 (showing that New York City ranks fourth among U.S. cities in its absence of racial heterogeneity at the census tract level). Moreover, I did in fact account for racial differences in criminal participation rates within precincts by performing a series of sensitivity tests on the negative binomial regression analysis that I conducted for my First Report, using interaction terms for precinct racial demographics and crime rate. Because these tests did not affect the results of my analysis in any meaningful way, I concluded it was unnecessary to report the results of these tests in my First Report.

30. Smith also repeats his critique from his expert report that I used the incorrect spatial and temporal units of analysis in my multivariate regressions. However, as set forth in my Supplemental Report, I ran the same multivariate regressions using smaller spatial units of analysis (neighborhoods and census tracts) which strongly resemble the spatial units advocated by Smith- e.g., "hot-spots", "impact zones"- and the results were virtually identical. *See* Supplemental Report at 9-20.

31. In addition, the temporal interval that I used in these regressions-one month- was sufficient to address problems of serial correlation which the social scientific literature instructs will likely occur if observations are spaced too closely in time, and to the related problem of autoregression that often occurs when observations are spaced too widely in time. *See* Badi Baltagi, *Econometric Analysis of Panel Data* (2001); Badi H. Baltagi and Qi Li, *Testing AR(1) Against MA(1) Disturbances In an Error Component Model*, 68 JOURNAL OF ECONOMETRICS 133 (1995); Wilbur John Coleman II, *Money and Output: A Test of Reverse Causation*, 86 THE AMERICAN ECONOMIC REVIEW 90 (1996). Moreover, the one-month time frame is, according to the deposition testimony of three senior level NYPD officials that I reviewed, consistent with the time interval that the NYPD itself uses to analyze and respond to crime patterns. *See* Supplemental Report at 4-5 n. 11.

32. Smith's claim that the reliability of my multivariate regression analyses is undermined by multicolinearity caused by the fact that all of my independent variables are highly correlated with race, *see* Smith Decl. ¶ 21, misunderstands the entire point of my regression models. I intentionally used a set of independent variables that correlate with race (crime, poverty, etc.) as well as race itself in order to determine whether there is a unique race effect on stop rates after controlling for these other factors. The relevant criminological and policing literature supports this approach. *See* Exhibit C; Ian Ayres, *Outcome Tests of Racial Disparities in Police Practices*, 4 JUSTICE RES. & POL'Y. 131, 134 (2002) (noting that "the outcome test intentionally harnesses omitted variable bias to test whether any excluded (unjustified) determinant of decisionmaking is sufficiently correlated with the included racial characteristics to produce evidence of a statistically significant racial disparity"); Robert J. Sampson, *Moving to Inequality: Neighborhood Effects and Experiments Meet Social Structure*, 114 AMER J.

SOCIOLOGY 189, 216 (2008) (stating that ...."the goal of studying sorting and selection into neighborhoods of varying types is an essential element in the larger theoretical project of understanding neighborhood effects"); Close, B.R.and P.L. Mason.. *Officer Characteristics and Racially Biased Policing*, 3 REVIEW OF LAW AND ECONOMICS 263 (2007).

33. Moreover, multicolinearity, to the extent it was present in my analyses, would actually understate, rather than overstate, the statistical significance of the relationship between predictor (neighborhood racial composition) and outcome (stop rates) variables by inflating the standard error in the test statistic. *See* Donald E. Farrar and Robert R. Glauber, *Multicollinearity in Regression Analysis: The Problem Revisited*, 49 THE REVIEW OF ECONOMICS AND STATISTICS 92 (1967); Peter Kennedy, *A Guide to Econometrics*, 187 (1998). Thus, the fact that, even in the face of such limitations, the results of my analyses still showed statistically significant relationships between the predictor and outcome variables reinforces the strong correlation between neighborhood or precinct racial composition and stop rates.

34. Contrary to Defendants' claim, for each of my two multivariate regression analyses, I ran alternative models in which crimes were disaggregated into smaller homogenous groupings (e.g., murders and robberies together with other violent crimes, grand larceny and burglary with other major property crimes). *See* First Report at 32-47. This method of aggregating crimes is in turn consistent with the way that the FBI aggregates crimes. See id. at Appendix C3; United States Department of Justice, Federal Bureau of Investigation, *Uniform Crime Reports: Crime in the United States, 2010, Offenses Known to Law Enforcement*, available at http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/offenses-known-to-law-enforcement/offensesknownmain.pdf.

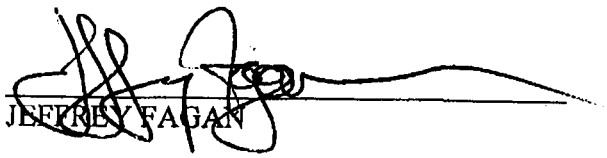
35. Defendants are correct that I logged the crime data. I did so in order to avoid what some in the field have referred to as the "tyranny of outliers," whereby certain data exerts undue influence on a statistical model. *See, e.g.*, Richard A. Berk, *New Claims about Executions and General Deterrence: Déjà vu All Over Again*, 2 JOURNAL OF EMPIRICAL LEGAL STUDIES 303 (2005). *See also* D.R. Cook and Sanford Weisberg, *Applied Regression Including Computing and Graphics* (1999).

36. Defendants' critique of my use of NYPD patrol strength data in my multivariate regressions is curious for two reasons. First, this is NYPD-created data created and provided to me by Defendants, for every precinct in the Department. Second, as I stated in my Supplemental Report, my multivariate regressions conducted at the precinct level showed that patrol strength was a relatively weak predictor of stop rates. Thus, whether or not the data itself was reliable has little or no impact on the overall reliability of my regression analyses.

37. Finally, Defendants' claim that the unemployment data I used for the regression analyses reflected in my Supplemental Report was not aggregated at a level that corresponds to the units of police action is false. As I stated in my Supplemental Report, data on unemployment and poverty was disaggregated by neighborhood and census tract which, as discussed above, closely resemble the so-called "hot-spots" and "impact zones" that Smith claims are the proper units of analysis for NYPD stop-and-frisk practices. *See* Supplemental Report at 16 n.49.

Pursuant to 28 U.S.C. § 1746, I declare under penalty of perjury that the foregoing is true and correct.

Dated: New York, New York
February 2, 2012



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