The Impact of Police Stops on Precinct Crime Rates in New York City, 2003 – 2010

Richard Rosenfeld
University of Missouri – St. Louis

Robert Fornango
Arizona State University

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> . . . a police officer may stop a person in a public place located within the geographical area of such officer's employment when he reasonably suspects that such person is committing, has committed or is about to commit either (a) a felony or (b) a misdemeanor defined in the penal law, and may demand of him his name, address and an explanation of his conduct.[1]
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> What is the causal relationship, if any, between public safety and police use of stop, question, and frisk practices (Jones-Brown et al. 2010:21)?

The New York Police Department's policy of "stop, question, and frisk" has been criticized during the last decade as unfairly targeting innocent persons, racially biased, and ineffective in reducing crime (Gelman, Fagan, and Kiss 2007; Jones-Brown et al. 2010). In this paper we focus on the issue of crime reduction. To our knowledge, only a single study has examined the impact of New York's stop, question, and frisk (hereafter SQF) policy on crime rates (Smith and Purtell 2008).[2] We describe the growth of SQF in New York City over the past decade, review the methods and results of Smith and Purtell's (2008) study, discuss the research challenges associated with evaluating the effects of SQF on crime rates, and present the data, methods, and findings of the current study. In contrast with the prior research, we find few significant effects of SQF on robbery and burglary rates in New York during the period 2003 to 2010. The differing results may be due to corresponding differences in the methods and data used in the two studies. We conclude by cautioning against relying on the results of just two empirical investigations for policy evaluation given the significant analytical challenges to

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[1] Excerpted from New York state Criminal Procedure Law (CPL) § 140.50, quoted in Jones-Brown, Gill, and Trone (2010:2-3)

[2] A related study by the same researchers evaluated the crime-reduction effects of the New York Police Department's "Operation Impact," which involved heightened police patrols of high-crime areas in New York City (see Smith and Purtell 2007).

estimating the effects of SQF and similar enforcement policies on crime and the rights and liberties of citizens.

BACKGROUND

The New York Police Department's SQF policy dates to at least the turn of the current century, but like other police departments the NYPD has been conducting "Terry Stops" for decades. After the Supreme Court's decision in *Terry v. Ohio* (392 U.S 1, 1968), police officers have been permitted to stop, question, and pending other conditions, search persons on the basis of "reasonable suspicion" that they have committed or are about to commit a crime. Comprehensive records on SQF activity in New York have been publicly available only since 2003, following a settlement reached in *Daniels et al. v. City of New York et al.* filed in 1999 by the Center for Constitutional Rights.[3] Under terms of the settlement, NYPD officers are required to file a written report on each stop they make when the stop involves the use of force, a frisk or more extensive search is conducted, an arrest is made, or the person stopped refuses to identify him or herself (Jones-Brown 2010:4). Since not all police stops meet these conditions and it is likely that some officers do not file the report even when required, the SQF records probably under count the number of police stops that occur each year. In addition, because some persons may be stopped more than once, the public records cannot be used to determine the number of different persons stopped (or stop prevalence). Finally, it is likely that records are incomplete during the period the reporting system was being implemented, so some of the growth in stops in 2004 and perhaps later may result from a more complete enumeration of police stops. Even so, the underreporting of police stops in the early years of required documentation is unlikely to

[3] See http://ccrjustice.org/ourcases/past-cases/daniels,-et-al.-v.-city-new-york.

account fully for the dramatic growth in police stops in New York since 2003 (Jones-Brown 2010; Smith and Purtell 2008).

Figure 1 displays the number of documented stops made by the NYPD per 10,000 precinct population between 2003 and 2010.[4] The stop rate shown for each year is the precinct average rate weighted by precinct population.[5] The bolded line represents the total stop rate per 10,000 residents. Also shown are the stop rates for black, Hispanic, and white suspects per 10,000 black, Hispanic, and white residents, respectively.

Figure 1 about here

The total rate of documented police stops in New York City tripled between 2003 and 2010 to 713 per 10,000 population from 193 per 10,000 population, for an average yearly rate of increase of 14.6%. Stops of Hispanics and blacks rose at an even higher annual rate (15.2% and 20.7%, respectively.) By contrast, the average yearly stop rate for white suspects rose by 9.1%, and all of that increase occurred between 2003 and 2006. The rate of increase in stops of Hispanics also fell somewhat after 2006, whereas the stop rate for black suspects increased almost monotonically over the eight-year period. In 2003 black suspects were stopped at a rate 2.6 times greater than Hispanics and 5.4 times greater than whites. By 2010, those stop ratios had risen to 3.2 and 9.3, respectively. In 2010 the NYPD recorded nearly three stops for every 10 black New York City residents. These results suggest that assessments of the effect of police stops on city crime rates should be based not only on the total growth in stops but also on the large and widening race-ethnic differences in stop rates between 2003 and 2010.

Over the same period that police stops were increasing in New York City, crime rates were falling. Figure 2 displays weighted average robbery and burglary rates per 10,000 precinct

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[4] The SQF data are from the NYPD website (http://www.nyc.gov/html/nypd/html/analysis_and_planning/stop_question_and_frisk_report.shtml.)

[5] The average precinct population during the period under investigation was just over 110,000.

population between 2000 and 2010.[6] Burglary rates fell by half over the period and robbery rates dropped almost as much.

Figure 2 about here

The juxtaposition of rising stop rates and falling crime rates prompts the question of whether increases in police stops contributed to declines in crime. NYPD and city officials claim that SQF has reduced crime, whereas SQF critics argue that the policy has been ineffective (Rivera, Baker, and Roberts 2010). The critics point to the relatively few arrests that result from SQF as evidence that the policy does not reduce crime because the large majority of persons stopped are "innocent" of wrongdoing (see Jones-Brown 2010:11). They also point to the small fraction of stops that yield a weapon (about one percent) as "clearly contradict[ing]" the NYPD's claim that SQF "keeps weapons off the street" (Center for Constitutional Rights 2011).

It is true that most documented police stops in New York City do not result in an arrest. Figure 3 displays the percentage of NYPD stops (weighted precinct averages) resulting in an arrest between 2003 and 2010. During the eight-year period, the police made an arrest in 6.6% of stops.[7] Interestingly, the percentage of stops resulting in an arrest dropped by nearly half to 4.6% from 8.6% between 2003 and 2006 and rose thereafter. We know of no explanation for the drop off in arrests during the first half of the period for which the data are available, but part of the decrease could be related to increased recording of "innocent" stops during the first years of required recordkeeping. The key question for our purposes, however, is what the percentage of stops resulting in arrests reveals about the effectiveness of SQF in reducing crime.

Figure 3 about here

[6] The precinct crime data were provided by the NYPD.
[7] Roughly the same percentage of stops resulted in the issuance of a summons (not shown; see Jones-Brown 2010:10).

Whereas SQF critics argue that the policy is ineffective because so few suspects are arrested or found carrying weapons or contraband, police officials argue just the opposite. The fact that few arrests are made, they contend, reflects the deterrent value of the policy. Knowing there is a strong likelihood they will be stopped by the police, the argument goes, deters would-be offenders from carrying weapons and contraband or otherwise engaging in criminal activity. How the police square this position with the legal necessity of "reasonable suspicion" a crime is underway or about to occur before a stop may be made remains an important and contentious issue. But the deterrence argument is not specious on its face. Averting crime is certainly a desirable feature of police enforcement activity (see Durlauf and Nagin 2011). Arrests may contribute to crime reduction, but the absence or low frequency of arrests does not necessarily indicate that SQF is ineffective if the threat of arrest prevents crime. From a research perspective, however, demonstrating the counterfactual – crimes that would have occurred in the absence of SQF – is a major challenge.

Ideally, one would evaluate the efficacy of SQF through random assignment of the policy across areas of the city. Crime rates in areas receiving the SQF "treatment" could then be compared with those without the treatment, both before and after the treatment was applied. In principal, random assignment insures that other conditions affecting crime are effectively held constant (but see Sampson 2010). The NYPD did not randomly introduce SQF in some precincts or patrol areas and withhold it from others. The policy was implemented citywide and, as noted, the documented SQF data do not encompass the entire period police have detained, questioned, and searched suspects. As shown below, the rate of SQF activity is strongly correlated with crime rates and other precinct-level characteristics (see, also, Gelman et al. 2007).

A number of methodological problems may arise from the absence of random assignment, including the problem of *endogeneity*. Technically, endogeneity exists when the predictor of interest, in this case a measure of SQF, is correlated with the error term of the equation used to estimate the effect of the predictor on the outcome, in this case crime rates (Wooldridge 2010). In practice, endogeneity is often produced by *simultaneity* between the predictor and outcome, meaning that the predictor is determined, in whole or part, by the outcome. We do not have to speculate about possible simultaneity in the relationship between SQF and crime in New York City. Police officials have been very explicit that they pursue the policy most vigorously in high-crime neighborhoods (Rivera et al. 2010). Failure to adequately address simultaneity and other sources of endogeneity can bias the estimates of the effect of SQF on crime (Murray 2006).

In their evaluation of the effects of SQF on crime in New York City, Smith and Purtell (2008) investigated monthly time series of police stops and precinct crime rates for the period February 1997 through December 2006. They conducted an interrupted time-series analysis of the rates of seven offenses in mixed effects panel models. To address the simultaneity in the relationship between police stops and crime, they lagged the stop rate one month behind the crime rates (Smith and Purtell 2008:19-20). Their analyses also include linear and quadratic trend indicators. They estimated the effects of police stops on citywide trends in the seven offenses and the effects of stops in "hot spot" precincts subject to heightened patrols through Operation Impact.[8]

Smith and Purtell (2008) characterize the results of their analysis as "mixed." They found statistically significant and negative effects of the lagged stop rates on rates of robbery, burglary, motor vehicle theft, and homicide and no significant effects on rates of assault, rape, or

[8] The results for the Operation Impact analyses are not summarized here.

grand larceny. They also found evidence of "declining returns to scale" (i.e., diminishing effects over time) of the effects of police stops on most of the offenses they analyzed but increasing returns to scale for robbery.

Given the methodological challenges to identifying the effect of SQF on crime in New York City using observational data, the approach taken by Smith and Purtell is not unreasonable, but it does have several notable limitations. Their estimates are not conditioned by the effects of other precinct characteristics on crime rates and perhaps also SQF, such as economic deprivation, race and ethnic heterogeneity, and residential stability, which prior research has found to be highly correlated with crime rates (e.g., Bursik and Grasmick 1990; Peterson and Krivo 2010). Failure to control for such conditions can result in omitted variable bias. Second, they did not control for spatial dependence in crime rates (i.e., the relationship between crime rates in adjacent or proximate geographic areas), another source of possible omitted variable bias. Third, their models do not incorporate period fixed effects, which adjust the estimates of SQF effects for unobserved time-varying sources of heterogeneity in crime rates. Fourth, they estimate the effects on crime rates of only a single measure of SQF, the total rate of police stops. Given the evidence presented earlier of substantial race-ethnic differences in the rate of police stops, it seems advisable to investigate the possibility that stops of individuals of different race-ethnic backgrounds may have differing effects on crime. Fifth, if the SQF critics are correct, stops resulting in an arrest may have a greater impact on crime than those involving "innocent" suspects, which suggests that separate analyses should be conducted of stops that produce an arrest.

Finally, and most importantly, we question Smith and Purtell's (2008) method for addressing the simultaneity in the relationship between police stops and crime. Their method simply entails lagging the stop rate one period behind the crime rate. They report that alternative lag structures (from two to six months) produced non-significant results (Smith and Purtell 2008:20n). There are at least two problems with this procedure for identifying the relationship between police stops and crime. First, Smith and Purtell do not include lags of the outcome, the crime rate, in their equations. But if police stops are a function of crime rates, failure to estimate the effects of prior crime rates on police stops and current crime rates amounts to serious omitted variable bias. The conventional approach in the panel-model context is to employ a "cross-lagged" design in which both the outcome and the explanatory variable of interest are lagged (e.g., a classic illustration is Greenberg, Kessler, and Logan 1979). Second, a single, one-period lag of either the explanatory variable of interest (police stops) or of the outcome (crime rate) can produce spurious results. The fact that successive lags of police stops yield non-significant effects of police stops on crime rates in Smith and Purtell's analysis suggests that the significant effects produced by including only the one-period lag may be artifacts of failure to condition the estimates of police stops on additional lags of this explanatory variable.

In summary, Smith and Purtell's (2008) evaluation of the effect of police stops on crime rates in New York City is limited by the absence of (1) substantively important covariates, (2) controls for spatial dependence in crime rates, (3) period fixed effects, (4) analysis of race-ethnic specific police stops, (5) analysis of stops resulting in arrests, and (6) lags of the crime rate and prior lags (beyond one month) of police stops. The current analysis of SQF effects on precinct crime rates addresses each of these limitations. We estimate the effects of total police stops and stops of black, Hispanic, and white suspects on yearly precinct robbery and burglary rates over the period 2003 to 2010. Our models include controls for other precinct conditions, including economic disadvantage, immigration, residential instability, racial composition, vacant housing, and precinct divorce rates. We also condition our estimates on the spatial dependence of precinct crime rates on those of nearby precincts and separately analyze the effects of SQF arrest rates on precinct crime rates. Our estimates are obtained from dynamic panel models containing period fixed effects and multiple lags of both the SQF indicators and crime rates.

We use Arellano-Bond linear panel models in our analysis (Arellano and Bond 1991; Roodman 2006). These models are designed to be applied to data with many panels (75 police precincts in this investigation) and comparatively few time periods (eight years). They also are recommended when at least one of the explanatory variables is assumed to be endogenous and the investigator does not have "excellent instruments waiting in the wings" (Roodman 2006:21) that can be used to identify the causal relationship between the endogenous variable(s) and the outcome. An instrument is a variable that is not included among the explanatory variables, is correlated with the endogenous variable, and is not correlated with the error term of the equation (Murray 2006). We were unable to find a suitable substantive variable meeting these conditions to identify the causal relationship between the SQF measures and crime rates. The Arellano-Bond estimator combines further lags of the outcome and endogenous explanatory variable(s) with differences of the strictly exogenous variables in a potentially large set of instruments that, when valid, produces consistent estimates of the effects of endogenous predictor(s) on the outcome.

Appropriate use of Arellano-Bond estimator requires the investigator to make many decisions regarding model specification, often without firm statistical or substantive guidance (Roodman 2006). We have sought to make our specification choices as transparent as possible and build our models in a step-by-step cumulative fashion to reveal the influence of each choice on the results. Although we do not believe our specification decisions are arbitrary, reasonable disagreements may exist regarding those decisions and the corresponding results. In addition to employing different methods for evaluating the effect of SQF on crime rates than those in Smith and Purtell (2008), our analysis is based on yearly data, which permits the inclusion in our models of substantive covariates available only on an annual basis (see Geller 2011). This as well as the other differences in data and methods should be kept in mind when comparing the results of the two studies.

DATA AND METHODS

The current study analyzes annual rates of robbery and burglary per 10,000 residents in 75 New York City police precincts between 2003 and 2010.[9] We limit our analysis to these two crime types because they are serious and comparatively well-measured offenses for which Smith and Purtell (2008) found significant SQF effects.[10] We examine the effects of several SQF measures on precinct robbery and burglary rates: the rate of total stops, stops of black, Hispanic, and white suspects, and stops resulting in an arrest, all per 10,000 precinct residents (source at fn4). We evaluate two measures of SQF arrests, the percentage of police stops resulting in an arrest and the arrest rate per 10,000 precinct population. For comparison and in line with prior research we also estimated the effects of total misdemeanor arrests (not limited to SQF) on precinct robbery and burglary rates (Harcourt and Ludwig 2006; Kelling and Sousa 2001; Messner et al. 2007; Rosenfeld, Fornango, and Rengifo 2007). Our models also include measures of precinct economic disadvantage, immigration rate, and residential stability. Each

[9] We dropped the precinct covering Central Park from the analysis because it has no residential population. The crime data were provided by the NYPD.

[10] Space (and time) limitations precluded analysis of all seven of the offenses analyzed by Smith and Purtell (2008). We did conduct limited analyses of precinct homicide rates but the Arellano-Bond models did not fit the homicide data well and the results were not substantively meaningful (all results not shown are available from the authors on request). See Langan and Durose (2004) and Rosenfeld and Lauritsen (2010) for assessments of the measurement reliability of New York City robbery and burglary trends.

measure consists of the factor scores from a principal components analysis of twelve social and economic indicators taken from the New York City tract-level files of the 2000 decennial census and the 2005-2009 American Community Survey (ACS).[11] The disadvantage measure reflects high loadings on a single factor of the percentage of families with incomes below the poverty level, the percentage of families receiving public assistance, median family income, the percentage of female-headed families with children under age 18, and the unemployment rate of males age 16 and over. The immigration measure reflects high loadings of the percentage of Hispanic, other-race (nonwhite, nonblack), and foreign-born residents. The residential stability factor reflects high loadings of the percentage of owner-occupied households and the percentage of the population residing at the same residence one year ago. The percentage of black residents, the percentage of vacant housing units, and the divorce rate did not load strongly on these three factors and were entered into the models as separate indicators. The non-Hispanic black, non-Hispanic white, Hispanic, and total precinct population data used to form the crime and SQF rates are also from the 2000 census and 2005-2009 ACS and were annualized and aggregated to the precinct level (see fn 11).

For each precinct we created spatial lag measures of robbery and burglary by averaging the respective weighted crime rates across all adjacent precincts. To account for differences in the number of adjacent areas across precincts, we use a row-standardized spatial weights matrix so that the sum of weights equals one for each precinct. All variables (except the disadvantage, immigration, and stability factor scores) were transformed to their natural logs to reduce

[11] We regressed the 2005-2009 ACS data on the 2000 census data and a linear counter to obtain annualized measures of the covariates. The tract-level data were aggregated to the precinct level for analysis. Population characteristics of tracts spanning multiple precincts were calculated according to the proportionate geographic area in each precinct.

skewness and yield coefficients representing the percentage change in the outcome given a one percent change in the predictor.

As noted above, the Arellano-Bond estimator requires that the investigator make multiple decisions regarding model specification. Important choices involve the lag structures of the dependent and endogenous explanatory variables, the number of elements to include in the instrument matrix, and the selection of a set of exogenous explanatory variables. We employ post-estimation tests of autocorrelation in the model residuals and the exogeneity of the instrument matrix, but they do not substitute for thoughtful model building (Roodman 2006). We chose to proceed by constructing successively more complex models, all of which treat the SQF measures as endogenous. We begin by regressing the precinct crime rates on the SQF measures. A second model then adds the substantive covariates. In successive models, a linear trend indicator and year dummy variables are added. Our final models include the spatial lag of the crime rate, which we chose to treat as endogenous. After experimenting with differing lag structures for the crime rate and the endogenous measures, we decided to include three lags of the crime rate and two lags of the SQF and spatial lag measures in all models. This choice was based largely on inspection of the Wald statistic, maximizing the number of observations free to vary, and limiting the number of instruments used in the estimation.[12] We assessed the robustness of our results to departures from these specification criteria (e.g., including two lags of the crime rates and three lags of the endogenous measures), but found few substantively meaningful differences in results. All of our models were fit with xtabond2 implemented in Stata 11.2 (Roodman 2006).

[12] No hard-and-fast rule exists for determining the optimal size of the instrument matrix, but a common rule of thumb is to not allow the number of instruments to exceed the number of panels (Roodman 2006). Where necessary, we met this standard by restricting the maximum number of lags of the crime rates and endogenous measures used as instruments in the equations.

RESULTS

Table 1 displays the correlation matrix, means, and standard deviations for the variables in the analysis of the robbery and burglary rates of 75 New York City police precincts between 2003 and 2010 (N = 600). As expected, positive correlations exist between the crime rates and the total number of police stops per 10,000 precinct population, reflecting the targeting of police stops in areas with high crime rates and, therefore, the endogeneity of police stops with respect to crime rates. The productivity of police stops, measured by the percentage of stops resulting in arrest, however, is weakly and negatively associated with precinct crime rates: proportionately fewer SQF arrests occur in precincts with higher crime rates. The rate of SQF arrests per 10,000 population is positively associated with precinct robbery rates but unrelated to burglary rates. The race-ethnic specific stop rates exhibit generally weak associations with precinct crime rates. Robbery and burglary rates tend to be higher in more disadvantaged and less stable precincts, those with relatively large proportions of black residents, those with higher vacancy rates, those located nearby high-crime precincts, and those with higher misdemeanor arrest rates.

Table 1 about here

Police stops are significantly correlated with several other characteristics of police precincts in addition to their robbery and burglary rates, including socioeconomic disadvantage ($r = .466$), immigration ($r = -.258$), percentage of black residents ($r = .457$), and the vacancy rate ($r = .416$). These results underscore the importance of controlling for other precinct differences in multivariate analyses of the effect of police stops on crime rates.

The results of our analysis of the effects of police stops on precinct robbery and burglary rates are shown in Tables 2 and 3. Because two lags of the police stop rates are employed in the analyses, the number of observations is reduced from 600 to 450. The first column of results in each table displays the effects of contemporaneous and two lags of police stops on precinct crime rates with no other variables in the equation. Beginning with the results for robbery, in this specification we observe a significant, negative effect of police stops lagged one period on robbery rates (b = -.341, p < .05). A ten percent increase in police stops during the previous year is associated with a 3.4% decline in robbery rates in the current year. This result is consistent with the hypothesis that police stops reduce crime and with the results obtained by Smith and Purtell (2008).

Tables 2 and 3 about here

The second column of Table 2 displays the results with the measures of other precinct conditions added to the equation. In this specification the effect of police stops lagged one period on robbery rates remains negative and significant but the magnitude of the effect is reduced by nearly half (b = -.184, p < .05). Significant effects are also found for precinct disadvantage, immigration, stability, and racial composition, and a marginally significant and negative effect is observed for the vacancy rate. The third column of the table adds the linear trend to the equation. Conditioning the estimates on the linear trend in robbery rates reduces the effect of police stops lagged one period to marginal significance (b = -.102, p < .10). With year effects (column 4) and the robbery spatial lag (column 5) added to the equation, the effect of lagged police stops on precinct robbery rates is no longer statistically significant, although a small positive association between concurrent police stops and robbery emerges in the final specification shown in Table 2 (b = .131, p < .05). Immigration and residential stability remain negatively associated with precinct robbery rates in this model and the spatial lag term is positively associated with precinct robbery rates.

In contrast with the robbery results, we find no significant effect of police stops lagged one period on burglary rates in any of the models shown in Table 3. A marginally significant negative effect of police stops lagged two periods does emerge in the final model ($b = -.097$, $p < .10$), but the substantive meaning of this result is unclear. We can think of no credible reason why today's burglary rate would be reduced by SQF activity two years ago, and in any case the effect, if it exists, is quite weak. The final model in Table 3 also indicates that burglary rates are higher in less stable precincts with larger proportions of black residents.[13]

We evaluated our final models with two post-estimation tests. The first is the Arellano-Bond test for serial correlation in the first-differenced model residuals. The existence of significant serial correlation in the first-differenced residuals at lag order greater than one implies model misspecification (Roodman 2006). We found no significant serial correlation in the first-differenced residuals of our final robbery and burglary models. The Sargan test evaluates the validity of the over-identifying restrictions of the model. Valid results imply exogeneity of the instrument matrix (Roodman 2006). The Sargan test indicates that the over-identifying restrictions of the final robbery model are valid. The test result for the final burglary model, however, indicates invalid over-identifying restrictions. This result is a consequence, at least in part, of our decision to set the number of lags of the burglary rate, police stop rate, and burglary spatial lag term to be used as instruments in the equation to a maximum of five. With four maximum lags of the outcome and endogenous variables as instruments, the Sargan test reveals valid over-identifying restrictions. No significant serial correlation of the lagged residuals is found in this specification and the substantive results remain unchanged.

[13] To insure that the number of instruments did not exceed the number of panels in the final models of Tables 2 and 3, we limited the maximum lags of the dependent and endogenous variables to be used as instruments to five.

We also estimated the effects of race-ethnic specific police stops on precinct robbery and burglary rates. The results (not shown) largely duplicate those for the effects of total stops on the precinct crime rates. We found significant, negative effects of stops of black suspects lagged one period on robbery rates, with the substantive covariates and the linear trend indicator in the model, and no significant effects after the year effects and robbery spatial lag term were added. The effect of lagged stops of Hispanic suspects becomes non-significant after the substantive covariates are added to the model. We found no significant effects in any of the robbery models for police stops of white suspects. Stops of black and Hispanic suspects (but not whites) lagged two periods behind the burglary rate have weak and marginally significant effects on precinct burglary rates, the same rather puzzling result found for total police stops in the final burglary model shown in Table 3. Again, it is unclear why police stops conducted two years earlier would affect the current burglary rate.

We estimated the same robbery and burglary models with the two measures of SQF arrests substituted for the stop rates (not shown). No significant effects of either the fraction of stops resulting in an arrest or the SQF arrest rate per 10,000 precinct population were found in any of the model specifications. Because prior research has found small but significant effects of misdemeanor arrests on precinct crime rates in New York City (Messner et al. 2007; Rosenfeld et al. 2007; but see Harcourt and Ludwig 2006), we examined the effects of misdemeanor arrests per 10,000 population on precinct robbery and burglary rates using the same sequential modeling strategy as before. The results are shown in Tables 4 and 5.

Tables 4 and 5 about here

As with the SQF indicators, we treat the misdemeanor arrests and crime spatial lags as endogenous in these models.[14] No significant effects of misdemeanor arrests are found in any of the burglary models (see Table 5). The misdemeanor arrest rate does have significant and sizable effects on robbery rates in the first three models shown in Table 4. The results for model 3, which includes the substantive covariates and linear time trend, imply that a 10% increase in the contemporaneous misdemeanor arrest rate reduces precinct robbery rates by 6.8%. The arrest rate lagged one period, however, has a significant positive effect on robbery rates ($b = .426$, $p < .05$). We doubt that last year's misdemeanor arrests increase this year's robbery rate and suspect that this result reflects remaining endogeneity of arrests with respect to robbery. In any event, when the period effects and robbery spatial lag are added to models 4 and 5 neither the concurrent nor the lagged misdemeanor arrest rates exhibit a significant relationship with precinct robbery rates. Post-estimation tests of the final robbery and burglary models reveal no significant serial correlation in the model residuals and indicate that the over-identifying restrictions are valid.[15]

The results of our analyses of the SQF indicators and misdemeanor arrest rates on precinct crime rates depend on how the models are specified. We believe that the inclusion of other precinct characteristics (disadvantage, instability, immigration, etc.) and linear time trends is a reasonable and appropriate specification decision. Adding period fixed effects, however, may be open to question. Baumer (2011) points out that including period dummy variables in panel models may swamp out meaningful time-varying effects on the outcome. We therefore re-

[14] We retain 600 observations in the misdemeanor arrest models because we have complete data for the 2000 - 2010 time series for both arrest and crime rates.
[15] We restricted the lags of the crime rate and endogenous variables in these models to a maximum of three to insure that the number of instruments would not exceed the number of panels.

estimated all of our final models omitting the year effects. The revised estimations produced no substantive changes in the results.

DISCUSSION

The controversy over the NYPD's SQF policy has been based on assumptions, but little systematic evidence, regarding the policy's effect on crime. The single prior study that examines the crime-reduction effects of SQF produced mixed evidence of its effectiveness but did find that more police stops lead to fewer robberies and burglaries (Smith and Purtell 2008). The results of the current study, by contrast, show few significant effects of several SQF measures on precinct robbery and burglary rates. The difference in results is likely due to corresponding differences between the two studies in data and methods. Each study has limitations that preclude definitive conclusions regarding the crime-reduction effectiveness of SQF. Our hope is that they will prompt future research that rigorously evaluates, within the limits of the policy's design and implementation, the effects of SQF and related enforcement practices on crime rates in New York City and elsewhere.

We have pointed to several limitations of Smith and Purtell's (2008) investigation that raise questions about the robustness of their results. They did not condition their estimates of SQF effects on the conjoint effects of other correlates of crime. Their analysis examines only one SQF measure, the total stop rate, and neglects to consider additional measures, such as race-ethnic specific police stops and SQF arrests that may – or may not – affect crime rates. Most importantly, we question the validity of the method they used to identify the causal relationship between SQF and crime. They simply lagged the rate of stops one period behind the crime rate. In our view, this procedure does not eliminate possible sources of statistical bias and may amount to “over-fitting” their models to a lag-structure that produces significant effects. On the other hand, a strength of Smith and Purtell’s (2008) study is their use of monthly crime and stop data, which allows for assessment of the short-run impact of police stops on crime.

The current study seeks to overcome many of these limitations. Our models include estimates of the effects of several precinct characteristics that are correlated with both crime and SQF, including socioeconomic disadvantage, racial composition, and the crime rates of adjacent areas. We examined the effects on crime rates of the total stop rate, race-ethnic specific stops, and SQF arrests. We also compared the crime-reduction effects of SQF arrests with those of total misdemeanor arrests. Finally, our estimates of SQF effects are obtained from dynamic panel models that condition the estimates on transformations and multiple lags of crime rates and the SQF measures. We believe this approach is necessary to address the endogeneity of SQF with respect to crime.

A potentially serious limitation of our analysis is the use of annual measurements of SQF, crime rates, and the substantive covariates, which could lead to underestimates of sub-annual SQF effects on crime, especially if the crime-rate “returns to scale” that Smith and Purtell (2008) found occur rapidly. Future research should examine sub-annual as well as annual data on SQF and crime to investigate this possibility. A significant challenge will be to obtain valid monthly or other sub-annual indicators of crime covariates.

Both the current study and prior research are based on data aggregated to New York City police precincts, with an average population of over 100,000 residents. Such large and heterogeneous geographic areas are probably not ideal for analyses of the impact of SQF on crime, especially if SQF activity is highly concentrated in specific neighborhoods, blocks, street segments, or other crime “hot spots” (see Telep and Weisburd 2011). This common limitation could lead to biased estimates of SQF effects on crime.[16] Future research should not only evaluate the effects of SQF in sub-annual time intervals but also its effects in smaller crime locales that may disproportionately affect the crime rates of police precincts or other large geographic areas.

Finally, if it is to inform the ongoing debate concerning both the efficacy and disproportionate impact of SQF on racial and ethnic minorities, the best future research will combine evidence on both outcomes in a single research design. No utilitarian calculus exists, nor is one desirable, that can disclose the optimal number of innocent persons that the police should detain, question, or search in order to reduce crime. The public, in New York City and elsewhere, wants the police to be effective *and* just in their day-to-day interactions with citizens; there is no optimal trade-off (Skogan and Frydl 2004; Stoudt, Fine, and Fox 2011). By this standard, the police must find ways to reduce crime that safeguard the rights and liberties of those they suspect of criminal activity. In the end, SQF as currently practiced in New York City may or may not meet this difficult but essential policy challenge. Research that encompasses both the crime-reduction effectiveness and possible collateral consequences of SQF should help the NYPD, city officials, and the public decide whether to retain the policy in its present form, introduce significant modifications, or abandon it.

[16] Smith and Purtell (2007, 2008) did investigate the effect of a hot-spots strategy, Operation Impact, on crime rates, but their analysis simply partitions police precincts according to whether they contained an "impact zone" and is not based on within-precinct micro-spatial data.

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Table 1. Correlation Matrix and Descriptive Statistics (N = 600)[a]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
(1) Robbery	---															
(2) Burglary	.608*	---														
(3) SQF_{Total}	.539*	.144*	---													
(4) $SQF_{Arrest\ Pct}$	-.092*	-.111*	-.240*	---												
(5) $SQF_{Arrest\ Rate}$	.456*	.069	.803*	.386*	---											
(6) $SQF_{Hispanic}$	.182*	.122*	.714*	-.095*	.620*	---										
(7) SQF_{Black}	.019	.124*	.510*	.003	.487*	.869*	---									
(8) SQF_{White}	.335*	.151*	.712*	-.185*	.563*	.430*	.284*	---								
(9) Disadvantage	.667*	.186*	.466*	-.088*	.390*	-.080	-.236*	.504*	---							
(10) Immigration	-.101*	.066	-.258*	.060	-.208*	-.052	-.074	-.121*	-.209*	---						
(11) Stability	-.322*	-.370*	-.038	-.257*	-.194*	-.318*	-.455*	.173*	-.048	.041	---					
(12) Pct Black	.644*	.130*	.457*	-.159*	.336*	-.138*	-.413*	.278*	.646*	-.356*	.247*	---				
(13) Divorce	.267*	.016	.133*	.177*	.235*	-.028	-.124*	.004	.177*	-.240*	-.136*	.373*	---			
(14) Vacancy	.276*	.194*	.416*	.091*	.451*	.487*	.482*	.180*	.053	-.407*	-.418*	.137*	.235*	---		
(15) $Robbery_{Spatial\ Lag}$	.691*	.380*	.190*	-.009	.175*	.003	-.095*	.059	.489*	-.170*	-.394*	.446*	.364*	.303*	---	
(16) $Burglary_{Spatial\ Lag}$	.378*	.575*	-.172*	.022	-.150*	-.034	.020	-.152*	.021	.077	-.447*	.003	.100*	.212*	.627*	---
(17) Mis. Arrest	.767*	.438*	.687*	.080*	.702*	.414*	.305*	.513*	.608*	-.217*	-.434*	.467*	.368*	.464*	.449*	.158*
Mean	3.280	3.307	6.136	1.757	3.288	6.341	7.228	5.148	-.080	-.230	.147	2.523	2.080	2.069	3.375	5.669
St. Dev.	.632	.461	.799	.516	.841	.727	.947	.957	.937	.872	.869	1.321	.166	.376	.479	.730

[a]All variables transformed to natural logs except Disadvantage, Immigration, and Stability.
*p < .05 (two-tailed)

Table 2. Arellano-Bond Panel Estimation Results for Precinct Robbery Rates in New York City, 2003 – 2010[a]

	(1)	(2)	(3)	(4)	(5)
Police stops	.019	.073	.085	.112	.131*
	(.086)	(.071)	(.061)	(.075)	(.061)
Police stops$_{-1}$	-.341*	-.184*	-.102+	.078	.103
	(.064)	(.062)	(.054)	(.073)	(.065)
Police stops$_{-2}$	.059	.044	.119*	.109*	.007
	(.044)	(.034)	(.031)	(.044)	(.040)
Disadvantage	---	.250*	.193+	.094	.026
	---	(.120)	(.105)	(.104)	(.099)
Immigration	---	-.122*	-.373*	-.286*	-.215*
	---	(.057)	(.057)	(.058)	(.055)
Stability	---	-.335*	-.330*	-.280*	-.224*
	---	(.094)	(.082)	(.087)	(.083)
Pct black	---	.255*	.341*	.201*	.089
	---	(.108)	(.093)	(.093)	(.091)
Divorce rate	---	-.260	.585*	.158	-.293
	---	(.242)	(.230)	(.231)	(.217)
Pct vacant	---	-.415+	.191	.194	.288
	---	(.203)	(.191)	(.195)	(.185)
Trend	---	---	-.107*	.030	-.049*
	---	---	(.013)	(.026)	(.018)
Year effects	---	---	---	---[b]	---[b]
	---	---	---	---[b]	---[b]
Spatial lag	---	---	---	---	.529*
	---	---	---	---	(.135)
Spatial lag$_{-1}$	---	---	---	---	.725*
	---	---	---	---	(.155)
Spatial lag$_{-2}$	---	---	---	---	.016
	---	---	---	---	(.108)
Wald chi^2	343.25*	585.92*	847.79*	928.97*	1094.03*
Observations	450	450	450	450	450

[a] Three lags of robbery rate estimated (not shown). Police stops and robbery spatial lag specified as endogenous.
[b] Year effects not shown

*p < .05 +p < .10 (two-tailed)

Table 3. Arellano-Bond Panel Estimation Results for Precinct Burglary Rates in New York City, 2003 – 2010[a]

	(1)	(2)	(3)	(4)	(5)
Police stops	.146^{+}	.210*	.228*	.205*	.036
	(.080)	(.069)	(.067)	(.082)	(.074)
Police stops$_{-1}$	.023	.051	.048	.119	.087
	(.069)	(.064)	(.063)	(.084)	(.068)
Police stops$_{-2}$	-.041	-.041	-.036	-.045	-.097^{+}
	(.044)	(.038)	(.038)	(.060)	(.053)
Disadvantage	---	-.098	-.140	-.137	.071
	---	(.124)	(.126)	(.124)	(.123)
Immigration	---	.040	-.011	-.008	-.064
	---	(.058)	(.066)	(.066)	(.063)
Stability	---	-.383*	-.367*	-.371*	-.299*
	---	(.089)	(.089)	(.093)	(.086)
Pct black	---	.282*	.285*	.261*	.248*
	---	(.114)	(.113)	(.116)	(.111)
Divorce rate	---	.141	.256	.292	.339
	---	(.246)	(.258)	(.250)	(.236)
Pct vacant	---	-.215	-.030	-.091	-.017
	---	(.198)	(.226)	(.234)	(.223)
Trend	---	---	-.021	-.003	-.004
	---	---	(.014)	(.023)	(.018)
Year effects	---	---	---	---[b]	---[b]
	---	---	---	---[b]	---[b]
Spatial lag	---	---	---	---	.508*
	---	---	---	---	(.124)
Spatial lag$_{-1}$	---	---	---	---	-.109
	---	---	---	---	(.138)
Spatial lag$_{-2}$	---	---	---	---	-.023
	---	---	---	---	(.118)
Wald chi^2	342.7*	504.9*	515.8*	530.6*	602.0*
Observations	450	450	450	450	450

[a] Three lags of burglary rate estimated (not shown). Police stops and burglary spatial lag specified as endogenous.
[b] Year effects not shown

*$p < .05$ $^{+}p < .10$ (two-tailed)

Table 4. Arellano-Bond Panel Estimation Results for Precinct Robbery Rates in New York City, 2003 – 2010: Misdemeanor Arrests[a]

	(1)	(2)	(3)	(4)	(5)
Mis. Arrests	-1.025*	-1.010*	-.676*	-.074	.008
	(.141)	(.161)	(.157)	(.130)	(.113)
Mis. Arrests$_{-1}$	.419*	.100	.426*	.080	.143
	(.181)	(.178)	(.168)	(.128)	(.123)
Mis. Arrests$_{-2}$	-.300*	-.120	-.082	.136	.112
	(.116)	(.105)	(.101)	(.086)	(.091)
Disadvantage	---	.019	-.038	-.025	-.080
	---	(.099)	(.097)	(.080)	(.079)
Immigration	---	-.233*	-.191*	-.077	-.023*
	---	(.063)	(.058)	(.047)	(.045)
Stability	---	-.118^{+}	-.046	-.094^{+}	-.053*
	---	(.064)	(.064)	(.052)	(.051)
Pct black	---	.393*	.244*	.038	-.091
	---	(.105)	(.100)	(.082)	(.083)
Divorce rate	---	.772*	.539*	.222	.049
	---	(.229)	(.208)	(.170)	(.164)
Pct vacant	---	-.171	.071	-.136	.007
	---	(.128)	(.148)	(.128)	(.128)
Trend	---	---	-.039*	-.028*	-.021*
	---	---	(.010)	(.009)	(.009)
Year effects	---	---	---	---[b]	---[b]
	---	---	---	---[b]	---[b]
Spatial lag	---	---	---	---	.344*
	---	---	---	---	(.128)
Spatial lag$_{-1}$	---	---	---	---	.267^{+}
	---	---	---	---	(.137)
Spatial lag$_{-2}$	---	---	---	---	-.043
	---	---	---	---	(.128)
Wald chi^{2}	509.6*	704.6*	740.26*	1199.7*	1312.6*
Observations	600	600	600	600	600

[a] Three lags of robbery rate estimated (not shown). Misdemeanor arrests and robbery spatial lag specified as endogenous.
[b] Year effects not shown

*$p < .05$ $^{+}p < .10$ (two-tailed)

Table 5. Arellano-Bond Panel Estimation Results for Precinct Burglary Rates in New York City, 2003 – 2010: Misdemeanor arrests[a]

	(1)	(2)	(3)	(4)	(5)
Mis. Arrests	-.051	.025	.127	.261	-.007
	(.167)	(.166)	(.162)	(.160)	(.133)
Mis. Arrests$_{-1}$	.092	.219	.336^{+}	.063	-.173
	(.186)	(.177)	(.175)	(.175)	(.145)
Mis. Arrests$_{-2}$	.052	.075	.095	.171	.119
	(.134)	(.125)	(.119)	(.119)	(.112)
Disadvantage	---	-.066	-.105	-.084	.080
	---	(.103)	(.101)	(.096)	(.093)
Immigration	---	.111*	.065	.043	-.081
	---	(.055)	(.057)	(.056)	(.053)
Stability	---	-.169*	-.158*	-.200*	-.231*
	---	(.071)	(.069)	(.065)	(.060)
Pct black	---	.343*	.339*	.391*	.353*
	---	(.105)	(.101)	(.101)	(.097)
Divorce rate	---	-.194	-.217	-.222	.030
	---	(.227)	(.217)	(.207)	(.189)
Pct vacant	---	-.047	.132	.196	.401*
	---	(.143)	(.153)	(.152)	(.156)
Trend	---	---	-.037*	-.035*	-.019
	---	---	(.014)	(.013)	(.013)
Year effects	---	---	---	---[b]	---[b]
	---	---	---	---[b]	---[b]
Spatial lag	---	---	---	---	.702*
	---	---	---	---	(.113)
Spatial lag$_{-1}$	---	---	---	---	-.056
	---	---	---	---	(.135)
Spatial lag$_{-2}$	---	---	---	---	-.094
	---	---	---	---	(.121)
Wald chi^2	779.6*	1007.3*	1084.5*	1217.6*	1483.1*
Observations	600	600	600	600	600

[a] Three lags of burglary rate estimated (not shown). Misdemeanor arrests and burglary spatial lag specified as endogenous.
[b] Year effects not shown

*p < .05 $^{+}$p < .10 (two-tailed)

Figure 1. NYPD Stops per 10,000 Population by Suspect Race and Ethnicity, 2003 - 2010 (Weighted Precinct Averages)

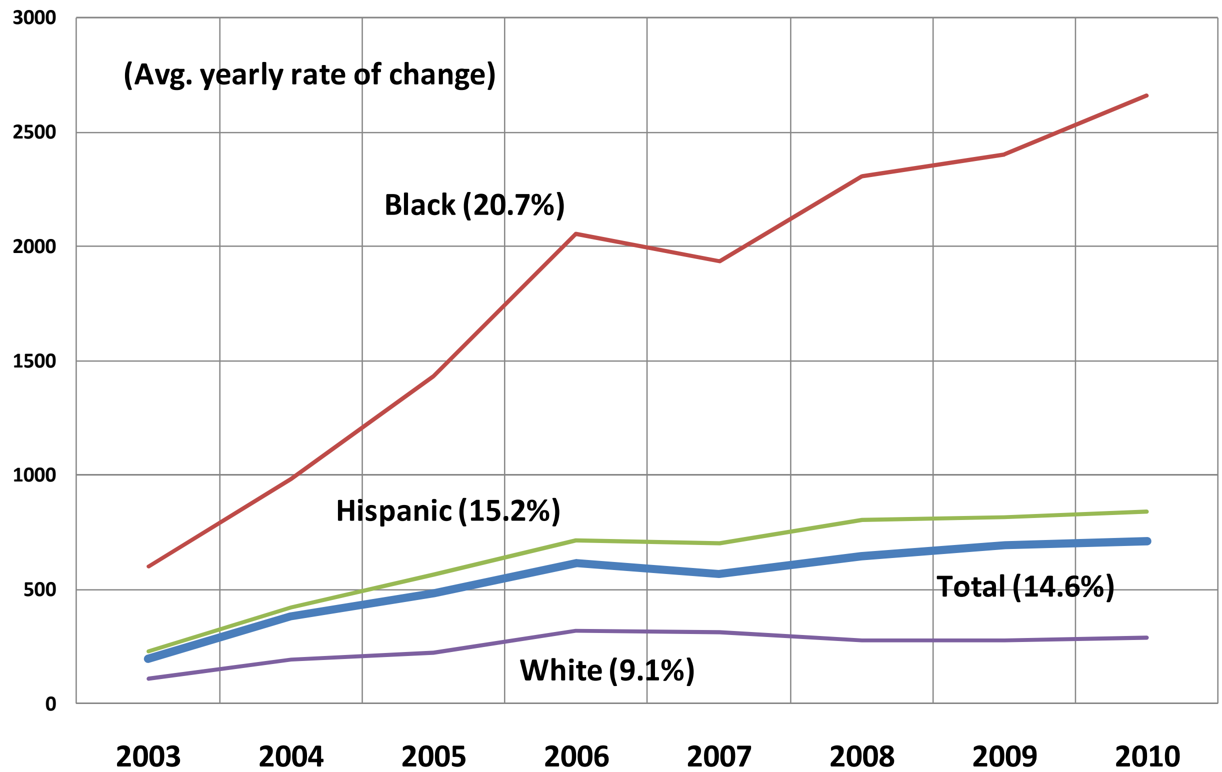

Figure 2. New York City Homicide, Robbery, and Burglary Rates per 10,000 Population, 2000 - 2010 (Weighted Precinct Averages)

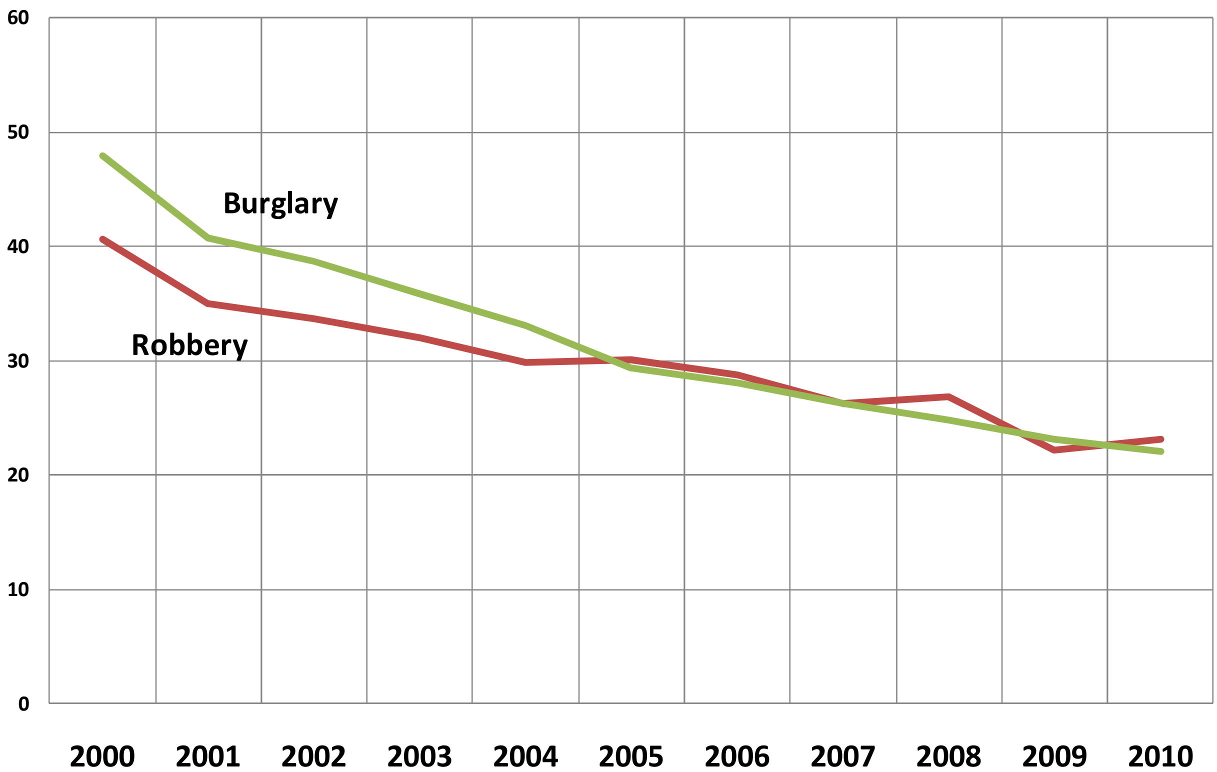

Figure 3. Percentage of NYPD Stops Resulting in an Arrest, 2003 - 2010 (Weighted Precinct Averages)