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# The Relationship Between Crime and Stop, Question, and Frisk Rates in New York City Neighborhoods

*Richard Rosenfeld and Robert Fornango*

The current study builds on prior research in an analysis of the relationship between monthly violent and property crime rates in New York City census tracks and the New York City Police Department's highly contentious stop, question, and frisk (SQF) policy. We find that higher doses of SQF are associated with small crime reductions generally and specific crime reductions for stops of blacks, Hispanics, and whites. But the way the policy was implemented precludes strong causal conclusions. Now that a federal court has intervened and SQF is undergoing change, the court monitor, New York Police Department, and city officials should partner with researchers in experimental evaluations to determine the optimal mix and dosage of enforcement strategies that safeguard the rights and liberties of citizens while enhancing public safety.

*Keywords* police stops; crime rates

During its peak years, the New York Police Department's (NYPD) stop, question, and frisk (SQF) policy generated extensive controversy. Most of the criticism focused on the policy's effect on the rights and liberties of minority residents. Correspondingly little attention, at least by the critics, was devoted to the policy's effect on crime. The 2013 federal court ruling in *Floyd v. City of New York* that found SQF unconstitutional as practiced and instituted substantive reforms did little to enlighten policymakers or the public regarding

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the impact of SQF on crime rates. The federal court judge in fact disallowed evidence pertaining to the policy's putative crime-reduction effects, observing:

> I emphasize at the outset, as I have throughout the litigation, that this case is not about the effectiveness of stop and frisk in deterring or combating crime. This Court's mandate is solely to judge the constitutionality of police behavior, not its effectiveness as a law enforcement tool (quoted in Schneiderman, 2013:27n).

We examine the relationship between SQF and violent and property crime rates in New York City between 2006 and 2011. We evaluate the effect of all stops and of those resulting in an arrest. We also compare the impact on crime of stops and arrests by the race and ethnicity of suspects, which has received little attention in prior research. The analyses are carried out on monthly crime and arrest data aggregated to census tracts and incorporate controls for socio-demographic, spatial, and unobserved temporal effects on crime rates. In these ways the current study improves on prior research, but methodological challenges to drawing strong causal conclusions remain. The most important limitation of this or any other research on SQF in New York is that the policy was not implemented as a field experiment in which the SQF "treatment" was systematically varied across the city. Therefore, the terms "effect" and "impact" are used simply for terminological convenience when referring to our own and others' research on the relationship between SQF and crime.

## Background

We present a brief overview of the rationale, scope, and growth of New York's SQF policy, which has been described in detail elsewhere (Jones-Brown, Stoudt, Johnston, & Moran, 2013; Schneiderman, 2013; White & Fradella, 2016). The policy implements and documents so-called Terry Stops by the NYPD. Based on the 1968 U. S. Supreme Court decision in *Terry v. Ohio*, police officers may detain and question citizens they have "reasonable suspicion" have committed or are about to commit a crime. Officers may frisk the outer clothing or conduct a more extensive search of individuals they believe to be armed and dangerous (Jones-Brown et al., 2013, pp. 1-2). NYPD officers have been required to document each Terry Stop they make since 2003. The number of such stops quadrupled from 160,851 in 2003 to 685,724 in 2011, and then began to fall. The NYPD recorded 532,911 stops in 2012 and 191,558 in 2013, a 72% drop from the 2011 peak.[1] The precipitous decline in documented police

1. The figures are from the New York Civil Liberties Union (http://www.nyclu.org/content/stop-and-frisk-data) taken, in turn, from the NYPD's SQF website (http://www.nyc.gov/html/nypd/html/analysis_and_planning/stop_question_and_frisk_report.shtml).

stops prompted the *New York Times* to proclaim that SQF is "all but gone" (Bostock & Fessenden, 2014).

Each year through 2012, about 6-7% of recorded stops resulted in an arrest and a summons was issued in another 5-6% of stops. Close to half of the individuals arrested between 2009 and 2012 were not prosecuted or their cases were dismissed, adjourned in contemplation of dismissal, or resulted in acquittal; just .10% of stops resulted in a conviction for a violent crime (Schneiderman, 2013, p. 3). In 2012, officers frisked the suspect in over half of all stops, but firearms were recovered in just .14% of them (Jones-Brown et al., 2013, pp. 18-23). The low yield in arrests and prosecutions from SQFs led critics to charge that the *Terry* standard of reasonable suspicion was not met in a large fraction of the stops recorded by the NYPD because "nearly nine out of 10 stopped-and-frisked New Yorkers have been completely innocent" (New York Civil Liberties Union, 2014). SQF critics also maintained that the low rate of firearm recovery indicates that the policy was ineffective at achieving its paramount objective of removing illegal guns from the street and reducing violent crime. But city and police officials drew the opposite conclusion from the same facts. The police found few firearms, they said, because SQF deters criminals from carrying guns in public (Rosenfeld & Fornango, 2014).

Perhaps the most persistent and influential criticism of SQF is that it is, or was during its heyday, racially biased. The court found in *Floyd v. City of New York* that the city "adopted a policy of indirect racial profiling by targeting racially defined groups for stops based on local crime suspect data" and that city officials had "turned a blind eye" to discriminatory policing practices (Goldstein, 2013). There is no question that SQFs were far more frequent in minority communities than in predominantly white areas of the city and that blacks and Hispanics were disproportionately stopped under the policy—if "disproportionate" is defined according to their presence in the population. But SQF defenders counter that minorities were not unduly targeted given their over-representation as crime victims and perpetrators (e.g., Mac Donald, 2013). That argument, in turn, must contend with evidence that minority communities experienced higher SQF rates even controlling for their crime rates (Gelman, Fagan, & Kiss, 2007; Rosenfeld & Fornango, 2014). The debate over racial bias notwithstanding, evaluations of the impact of SQF on crime should take account of the large and persistent race and ethnic differences in the application of the policy.

The rationale underlying SQF, at least as articulated by its proponents, is deterrence (White & Fradella, 2016, pp. 92—93). Heavy police presence along with stops of suspects according to *Terry* standards, the argument goes, deters criminals from carrying guns and committing crime. Some investigators have held that SQF incorporates a strong dose of "hot spots" policing, in so far as police stops tend to be concentrated in high-crime areas of the city (Weisburd, Telep, & Lawton, 2014; Weisburd, Wooditch, Weisburd, & Yang, 2015). The deterrence rationale assumes that individuals calculate, however imperfectly, the odds of apprehension and punishment when deciding to commit a crime

(Nagin, 2013). The lower the odds, the more likely a crime will occur. What, then, should be made of the low yield of arrests and convictions from SQFs? Is that an indication of the policy's weak deterrent effect on crime or its effectiveness in averting crime? When alternative interpretations of the same facts are plausible, empirical evidence is needed to resolve debate. A comprehensive evaluation of SQF should determine the effects of SQF arrests, as well as stops, on crime rates.

Finally, as with other aspects of the debate over SQF, its impact on crime remains highly controversial, even though proponents and critics of the policy drew on largely the same facts to support their position. The proponents argued that proof of the policy's effectiveness is manifested in New York's sustained crime drop (Mac Donald, 2013). The critics allege that lower crime rates were the product of other factors, and as evidence point to continuing crime reductions even as SQFs have decreased in recent years (Jones-Brown et al., 2013). Neither argument is convincing. SQF could have contributed to crime reductions in New York that also resulted from the forces pushing down crime rates in other cities (Baumer & Wolff, 2014; White & Fradella, 2016). And the effects could have been sufficiently modest that crime rates in New York would have continued to drop even as SQFs decreased. If we allow that changes in crime rates are produced by multiple causes, the extreme positions of SQF's defenders and critics alike are untenable.

## Prior Research

SQF's impact on crime requires careful and sustained attention from researchers. A small but growing number of studies have investigated the policy's crime-reduction effects. Smith and Purtell (2007, 2008) found that monthly crime rates were lower in New York police precincts with higher levels of SQF. They did not incorporate controls for other influences on crime, however, nor did they examine the effect of SQF arrests separately from those of stops or race-ethnic differences in stops and arrests. Rosenfeld and Fornango (2014) incorporated these measures in their research and found, contrary to Smith and Purtell (2007), that police stops had negligible effects on robbery and burglary rates.

Like Smith and Purtell's research, however, Rosenfeld and Fornango's study is limited by the use of police precincts as the units of spatial analysis. New York City police precincts, with over a 100,000 residents on average, are the size of small American cities and arguably are too large and heterogeneous to capture the effects of SQF in more localized areas. In addition, they estimated the effect of SQF stops and arrests on annual crime changes. As a result, their study is unable to detect effects of SQF on crime that persist for less than a year. Finally, Rosenfeld and Fornango's study investigated just two crime types.

Recent studies by Weisburd et al. (2015) and MacDonald, Fagan, and Geller (2016) overcome many of these limitations. Weisburd et al. (2015) examined the impact of SQFs on small geographic "hot spots" where crimes are heavily concentrated. Using sophisticated regression methods, the researchers found that SQFs reduced the volume of crime in the hot spots during the next few weeks or days, depending on the analysis, without appreciable crime displacement to adjacent areas. MacDonald et al. (2016) examined the effects of increases in SQF on both crime and arrests in "impact zones" in New York. Impact zones were high crime areas within NYPD precincts to which officers were deployed to make investigative stops.[2] MacDonald and colleagues compared monthly changes in multiple offense types (e.g., violent crimes, property crimes, drug crimes, weapons offenses) in the impact zones with those in other areas of the same precinct in models that assessed crime levels before and after the formation of the impact zones over the period 2004 to 2012.

MacDonald et al. (2016) found significant crime reductions in the impact zones, especially in robbery and burglary, without crime displacement to adjacent areas. They also found significant increases in arrests, though they did not estimate the effect of arrests on crime. They did find, however, that police stops based on probable cause of criminal activity accounted for the entire effect of the increase in SQF in the impact zones. They found no significant effects on crime of "general suspicion" stops (e.g., those based on a suspect's "furtive movements").

This recent research is notable for its strengths in comparison with previous studies of SQF and crime in New York. Both studies are based on small geographic and temporal units of analysis and use innovative methods to address simultaneity in the relationship between SQFs and crime. The latter is particularly important because the crime-reduction effects of SQFs are potentially confounded by the greater use of SQFs where crimes are highly concentrated.

But the Weisburd et al. (2015) and MacDonald et al. (2016) studies are also limited in several respects. The results of the Weisburd et al. (2015) investigation are based on estimates of SQF effects on total crimes, which are heavily weighted by property offenses. To shed empirical light on the controversy surrounding the impact of SQF on violent crime, it is necessary to distinguish its effects on violent and property offenses. The MacDonald et al. (2016) study does not incorporate controls for other conditions that may affect urban crime and also may be related to SQFs, such as socioeconomic disadvantage, residential instability, and population heterogeneity (Bursik & Grasmick, 1993; Peterson & Krivo, 2012; Sampson, 2012). Neither study includes separate estimates of the crime-reduction effects of SQFs that result in an arrest or separate estimates of the effects of stops and arrests of suspects of different race or ethnic groups. A reasonable hypothesis is that stops resulting in an arrest should have stronger crime deterrent effects than stops of "innocent" persons.

2. In their 2007 study, Smith & Purtell also assessed SQF effects on crime in impact zones.

Whether stops of Non-Hispanic blacks and Hispanics have stronger crime-control benefits than stops of Non-Hispanic whites is open to speculation.[3]

With the exception of the study by Rosenfeld and Fornango (2014), prior research has not sought to isolate the effects on crime of stops of blacks, Hispanics, and whites. By contrast, extensive research documents race differentials in police contact and treatment. Studies of nationally representative data have disclosed sizable race differences in traffic stops, with black motorists more likely than white motorists to be stopped by the police (Lundman & Kaufman, 2003) and more likely to be searched during the stop, despite being less likely than whites to possess contraband (Engel & Calnon, 2004). National and local studies have also found that blacks are more likely than whites to have negative interactions with the police and to report police misconduct, including unwarranted stops (e.g., Brunson, 2007; Weitzer & Tuch, 2004). Finally, a large body of survey research suggests that citizens' perceptions of procedural injustice in law enforcement reduces compliance with the law (see Tyler, Goff, & MacCoun, 2015, for a review).

SQF proponents might argue that stops of minority suspects should have a greater impact on crime given the disproportionate criminal involvement of minority groups. By contrast, critics of the policy might contend that stops of blacks and Hispanics will have less of an impact on crime than stops of whites, because they result from race-ethnic profiling rather than crime-relevant considerations. The research on procedural justice, however, suggests that unjust treatment by the police could well lead to more and not less crime. The current study cannot fully resolve these complex considerations. But it can show whether the sharp race-ethnic differentials in exposure to SQF in New York have the same or differing effects on neighborhood crime rates, an important step along the way to the more extensive research that is needed.

## Data and Methods

This study evaluates the relationship between SQF stops and arrests and monthly violent crime (homicide, aggravated assault, robbery) and property crime (burglary, larceny, and motor vehicle theft) rates per 1,000 population in New York City census tracts.[4] The residential population is not always a reliable basis for measuring crime risk, especially in heavily commercialized areas frequented by large numbers of nonresidents (MacDonald et al., 2016, p. 5; see, also, Chamlin & Cochran, 2004). Norming the crime counts by tract

3. Hereafter, Non-Hispanic blacks and whites are referred to as "blacks" and "whites."

4. The crime data were provided by the NYPD. Rape is excluded because sexual violence is not well measured in police data. The SQF data are from the NYPD's SQF database (http://www.nyc.gov/html/nypd/html/analysis_and_planning/stop_question_and_frisk_report.shtml). For measurement reliability, analyses are restricted to census tracts with 100 or more inhabitants.

population is necessary, however, because the estimation procedure used in the current analysis is not suitable for count outcomes.

The analysis covers the 72-month period from January 2006 to December 2011. We chose this time frame to capture the substantial growth in SQFs over time and to reduce the influence of artifactual changes attributable to increased recording of SQFs during the early years of the policy.[5] The average population of New York City census tracts with 100 or more residents is 3,884 ($n = 2{,}111$). These spatial units are more homogeneous in population characteristics than are police precincts and are much closer in size to common conceptions of "neighborhood." They are much larger, however, than the micro spatial units—street segments and adjoining intersections—used in Weisburd et al. (2015). One advantage of using census tracts as units of analysis is that the estimates of SQF effects on crime can be conditioned on a large number of social, economic, and demographic covariates. Another advantage is that the resulting estimates of SQF effects can be aggregated to gage the citywide effects of the policy over time. Estimating the contribution of crime reductions on high-crime street blocks to citywide crime reductions is less straightforward. The disadvantage of basing the estimates on census tracts is the within-unit heterogeneity in both crime and SQFs at the tract level. Both the benefits and costs of census tracts as units of analysis should be kept in mind when interpreting the results of the current study and when comparing them with those found in prior research based on both larger and smaller spatial units.

The independent variables of primary interest are the monthly rate of SQFs per 1,000 tract population and the proportion of SQFs that result in an arrest. Each of these measures is also computed separately for stops and arrests of Hispanic, black, and white suspects. Our models also include measures of economic disadvantage, race-ethnic composition, immigration, and other variables that may be correlated with both crime rates and SQFs; spatial lag terms to control for the influence of crime rates in nearby areas; and period effects to control for unobserved time-varying influences on crime rates.

The control variables are from the census tract files of the 2005-2009 and 2007-2011 American Community Survey (ACS). Measures of socioeconomic disadvantage include the poverty rate, median household income, the male unemployment rate, the percentage of the population receiving public assistance, the percentage of households with children under the age of 18 headed by females, and the percentage of the population without a high school degree. Measures of residential instability include the residential vacancy rate, the percentage of households that are owner-occupied, the percentage of the population residing in the same household one year ago, and the divorce rate. The immigration measures include the foreign-born percentage of the

5. This decision was based on discussions with an NYPD crime analyst.

population and the percentage of households that are "linguistically isolated."[6] Population heterogeneity measures include the percentage of the population that is Hispanic, the percentage of the population that is black, and the percentage of the population neither white nor black. Finally, we include in the analysis a measure of the sex-age composition of the population—the percentage of the population that is male and between the ages of 15 and 24—and the tract population size.

The two ACS files contain data averaged over five-year periods. To obtain monthly estimates of the covariates, we applied polynomial curve fitting to the source data, as described in the Appendix. Given the large number of possible covariates and strong relationships among many of them, we included all of them in a factor analysis with orthogonal rotation. The analysis produced two factors with eigenvalues greater than 1.00, which we label Economic Disadvantage and Immigration. The variables with high rotated loadings (>|.50|) on the Economic Disadvantage factor are the poverty rate (.870), high school drop out rate (.801), median income (−.792), public assistance rate (.763), female-headed households (.759), owner-occupied housing (−.707), and percent Hispanic (.683). The variables with high loadings on the Immigration factor are the percent foreign born (.748), percent linguistically isolate households (.712), and percent of the Non-Hispanic population neither white nor black (.664). The other ACS variables are entered as separate predictors in the regression models.[7]

The violent and property crime rates, SQF stops, SQF arrests, and all but two of the control variables are logged (base e) to correct for skewness. The Economic Disadvantage and Immigration factors are retained in original metric. The variables were transformed by first adding 1 and then taking the natural log. Logging the variables produces an intuitive interpretation of effect size: A one percent change in the predictor yields a b percent change in the outcome, where b is the coefficient on the predictor.[8]

We control for the influence of crime rates in surrounding areas by including spatial lags of the outcome crime rate. Spatial lags were defined using 1st order queen weighting, resulting in a weighted average crime rate for each of the adjacent census tracts (see Anselin, 1988; Fornango, 2010). As with the

6. The Census defines linguistically isolated households as those in which "no member 14 years old and over (1) speaks only English or (2) speaks a non-English language and speaks English 'very well'" (http://www.census.gov/hhes/socdemo/language/about/faqs.html).

7. Orthogonal rotation forces the correlations among the factors to zero. To assess the sensitivity of the results to this assumption, we also performed a factor analysis with oblique rotation, which allows the factors to be correlated. The results are virtually identical to those reported.

8. Log transformation and the addition of 1.0, or some other positive constant, to a measure before taking the log can induce unwanted changes to the original distribution. We checked the resulting distributions of the log-transformed measures in our analysis and found that the transformations had the intended effect: the influence of extreme values was reduced and the zeros were retained in the transformed measures (ln(1) = 0), without otherwise distorting the distributions of the original measures. Results available on request from the lead author.

other variables, the spatial lags were transformed by taking the natural log after adding 1 to the original value. The regression models also include fixed effects for month to condition the estimates on unmeasured influences on the crime rates common to all census tracts over time, which could include other citywide crime-control strategies the NYPD implemented between 2006 and 2011.

## Estimation Methods

Our estimates of the effects of SQFs on violent and property crime rates are obtained from Arellano-Bond linear panel models (Arellano & Bond, 1991; Roodman, 2006). These dynamic panel models are designed for situations in which one or more of the regressors is assumed to be endogenous (i.e., is influenced by the outcome) and the number of panels exceeds the number of time points in the data. Both conditions hold in the current study. Tract-level SQF and crime rates are positively correlated,[9] and the estimates are based on 2,111 panels (census tracts) and 72 months. Multiple lags of the outcome, the endogenous predictor, and the exogenous predictors, in both levels and differences, are used as instruments to identify the effect of the endogenous predictor on the outcome.[10] The instrument set can grow quite large. A common rule-of-thumb is to not allow the number of instruments to exceed the number of panels in the analysis (Roodman, 2006). This condition is also met in the current study.

Two specification tests are used to evaluate the models. The first assumes that the error terms exhibit no second-order autocorrelation. The second is the Sargon test of the validity of the instruments, which evaluates the over-identifying restrictions of the model. Valid results imply that the instrument set is exogenous (Cameron & Trivedi, 2009; Roodman, 2006; Rosenfeld & Fornango, 2014, Appendix). The results of both post-estimation tests are reported here. The models were fit with the user-written xtabond2 program by Roodman (2006), implemented in Stata 13.1.

As Rosenfeld and Fornango (2014) point out, the post-estimation tests do not substitute for thoughtful model building. The investigator must make a priori decisions about the lag structure of the outcome and endogenous predictor, the size of the instrument set, and selection of the exogenous explanatory variables. After experimenting with differing lag structures of the crime rates

9. The correlation (r) between the total crime rate and the SQF rate in census tracts is .679.
10. An "instrument" is a variable that, when controlled in a regression equation, reduces suspected endogeneity of one or more of the predictors. The logic behind using past values of the outcome and predictors as instruments is that they cannot be influenced by current or more recent values of the outcome, thereby increasing confidence that significant effects of the predictors represent causal influences on the outcome. As discussed below, however, strong causal inferences cannot be drawn from observational data of the kind used in this study.

and SQF variables, we chose to lag both by two periods. Additional lags produced few substantive differences in the results. This decision also was based on inspection of the Wald statistic to maximize model fit and selection of the number of instruments necessary to satisfy the post-estimation tests. We caution that, absent firm statistical or substantive guidance, reasonable differences may exist regarding model selection based on the Arellano-Bond estimator.

## Results

Table 1 presents descriptive statistics for the variables included in the analysis. The average number of property crimes per 1,000 tract population per month was nearly double the monthly rate of violent crimes between 2006 and 2011. The NYPD recorded an average of nearly seven SQFs per 1,000 tract population per month (hereafter termed "stops") during the 72-month period. Blacks were stopped at a rate double the rate for Hispanics and nearly seven times the rate for whites. About seven percent of the stops resulted in an arrest (hereafter "arrests"), and the arrest rate was roughly the same for stops of Hispanics, blacks, and whites.

The pooled data exhibit sufficient density and variability in stops and arrests to permit reliable estimation of their effects on crime. The number of stops per census tract ranges from zero to 838. The median number of stops is 11 and the mean is 27, indicating substantial skew in the count and rate of stops across New York census tracts with more than 100 residents. The proportion of stops resulting in an arrest ranges from zero to one, with a mean of .068 and a median of zero. The arrest measure, of course, is restricted to only those census tracts in which at least one stop occurred during the observation period. One cautionary note regarding data sparseness is the small number of stops of whites. Stops of whites range from zero to 142, but average just over two stops per census tract, compared with an average of approximately 11 stops of blacks and seven stops Hispanics. The comparatively small number of SQFs involving white suspects should be kept in mind when interpreting the regression estimates reported below.

### Multivariate Analyses

We first present the results of models that regress the violent and property crime rates on total stops and arrests along with the substantive control variables, spatial lags, and time fixed effects. The stops and arrests are uncorrelated ($r = -.042$) and are entered in the same models. As shown in Table 2, total stops have a significant negative contemporaneous effect on the violent crime rate and significant lagged negative effects from one and two months before. The results imply that SQF stops are associated with reductions in

**Table 1** Descriptive statistics for New York City census tracts, 2006-2011[1]

| Variable | Definition | Mean | St. Dev. | Minimum | Maximum | Obs. |
|---|---|---|---|---|---|---|
| Violent crime | Violent crimes per 1,000 pop. | .48371 | 2.4500 | 0 | 259.26 | 152,001 |
| Property crime | Property crimes per 1,000 pop. | .90831 | 4.1520 | 0 | 361.11 | 152,001 |
| Stops | SQF stops per 1,000 pop. | 6.9002 | 27.466 | 0 | 1783.9 | 152,001 |
| Black stops | SQF stops of blacks per 1,000 blacks | 14.628 | 63.306 | 0 | 4283.1 | 152,001 |
| Hispanic stops | SQF stops of Hispanics per 1,000 Hispanics | 7.5988 | 31.351 | 0 | 2408.2 | 152,001 |
| White stops | SQF stops of whites per 1,000 whites | 2.2129 | 11.404 | 0 | 843.675 | 152,001 |
| Arrests | Total arrests/SQF stops | .06836 | .13243 | 0 | 1 | 142,860 |
| Black arrests | Arrests/Black stops | .07120 | .16602 | 0 | 1 | 112,292 |
| Hispanic arrests | Arrests/Hispanic stops | .06905 | .17275 | 0 | 1 | 111,723 |
| White arrests | Arrests/White stops | .06808 | .20000 | 0 | 1 | 85,926 |
| Disadvantage | Factor score | .01332 | .95066 | −2.1912 | 3.2957 | 152,001 |
| Immigration | Factor score | .03366 | .81340 | −1.5731 | 3.3901 | 152,001 |
| % Black | % population non-Hispanic black | 24.822 | 30.358 | 0 | 98.819 | 152,001 |
| % Same res. | % same residence one year before | 88.907 | 7.5092 | 2.2104 | 100 | 152,001 |
| % Divorced | % 15 and older divorced | 7.7917 | 3.3529 | 0 | 89.549 | 152,001 |
| % Unemployed | % males unemployed | 10.485 | 7.3511 | 0 | 100 | 152,001 |
| % Vacant | % vacant housing units | 8.4433 | 6.0108 | 0 | 78.673 | 152,001 |
| % 15-24 | % males age 15-24 | 6.8799 | 3.2856 | 0 | 51.356 | 152,001 |
| Population | Number of tract residents | 3883.8 | 2146.2 | 100.06 | 26310 | 152,001 |

[1]Monthly values for census tracts with 100 or more residents. Variables in original metric.

**Table 2** Effects of SQF stops, arrests, and controls on monthly violent and property crime rates in New York City census tracts, 2006-2011 (No. of obs. = 128,869)[1]

| | Violent crime | Property crime |
|---|---|---|
| Violent crime$_{t-1}$ | .53354** (.20209) | – |
| Violent crime$_{t-2}$ | .11700 (.15321) | – |
| Property crime$_{t-1}$ | – | .38007* (.16882) |
| Property crime$_{t-2}$ | – | .02519 (.11345) |
| Stops | −.00544* (.00266) | .06913 (.05218) |
| Stops$_{t-1}$ | −.00552* (.00266) | −.02226* (.01062) |
| Stops$_{t-2}$ | −.00992** (.00280) | −.01517** (.00351) |
| Arrests[2] | .02155 (.01162) | .47162 (.83123) |
| Arrests$_{t-1}$[2] | −.00185 (.01208) | −.00099 (.02236) |
| Arrests$_{t-2}$[2] | .01120 (.01139) | .02832 (.01688) |
| Spatial lag | .08245** (.01004) | .13635** (.01426) |
| Disadvantage | .03724** (.00563) | .06248** (.01578) |
| Immigration | −.01338** (.00244) | −.03173** (.00504) |
| % Black | .00494** (.00115) | .00447 (.00390) |
| % Same residence | −.16769** (.02613) | −.31375** (.06450) |
| % Divorced | .00068 (.00256) | −.00797* (.00318) |
| % Unemployed | .00302 (.00180) | −.00675** (.00224) |
| % Vacant | .00808** (.00205) | .01362** (.00517) |
| % 15-24 | −.00980** (.00302) | −.01942** (.00527) |
| Population | −.05438** (.00782) | −.10069** (.02019) |
| Wald $\chi^2$ | 43589** | 75474** |
| Instruments | 104 | 100 |
| A-B AR(2)test[3] | $p = .739$ | $p = .563$ |
| Over-id. test[4] | $p = .539$ | $p = .379$ |

[1]Arellano-Bond regression models. Standard errors in parentheses. Period effects not shown. All variables logged (base e) except disadvantage and immigration factor scores.
[2]Proportion of stops resulting in arrest.
[3]Arellano-Bond test for second-order autocorrelation.
[4]Sargan test of over-identification restrictions.
**$p < .01$; *$p < .05$ (two-tailed).

violent crime that may persist up to three months. Total stops also have a significant negative effect on the property crime rate at one and two month lags but do not have a significant contemporaneous effect. SQF arrests have no significant effect on either violent or property crime rates. We also observe that several of the controls exert significant effects on the violent and property crime rates in New York census tracts. For example, both violent and property crime rates were higher in more disadvantaged neighborhoods, those adjacent to high-crime areas, and those with higher vacancy rates. Violent and property crime rates were lower in more residentially stable areas and those with higher levels of immigration.

The results of the race- and ethnic-specific analyses are shown in Table 3. Stops of blacks, whites, and Hispanics are significantly associated with violent

**Table 3** Effects of SQF stops and arrests, by race and ethnicity, and controls on monthly violent and property crime rates in New York City census tracts, 2006-2011[1]

| | Violent crime[1] | | | Property crime | | |
|---|---|---|---|---|---|---|
| | Black | Hispanic | White | Black | Hispanic | White |
| Violent crime$_{t-1}$ | .08184 (.55864) | .47807 (.28095) | −.04946 (.21056) | – | – | – |
| Violent crime$_{t-2}$ | .41332 (.44019) | .19141 (.22026) | .10947** (.02787) | – | – | – |
| Property crime$_{t-1}$ | – | – | – | .31214 (.26598) | .33709 (.28637) | .36151* (.18396) |
| Property crime$_{t-2}$ | – | – | – | .07907 (.16842) | .21134 (.20120) | .00566(.15626) |
| Stops | −.02424** (.00539) | −.02178** (.00542) | −.07793** (.01611) | −.07072** (.01118) | −.03303** ( .00783) | −.09208** (.01658) |
| Stops$_{t-1}$ | −.01955* (.00785) | −.02395** (.00523) | −.07518** (.01664) | −.05338** (.01100) | −.02742** (.00749) | −.08612** (.01582) |
| Stops$_{t-2}$ | −.02307** (.00674) | −.02721** (.00647) | −.07930** (.01761) | −.06342** (.01024) | −.03289** (.00775) | −.09487** (.01702) |
| Arrests[2] | .01063 (.01331) | −.00933 (.01394) | .00603 (.01148) | .00086 (.01777) | .00517 (.01367) | −.00054(.01624) |
| Arrests$_{t-1}$[2] | .00343 (.01318) | .00228 (.01311) | .00376 (.01195) | −.03603* (.01786) | .00199 (.01317) | .01027 (.01637) |
| Arrests$_{t-2}$[2] | .01167 (.01239) | −.00697 (.01391) | .00586 (.01158) | −.00124 (.02221) | .01628 (.01309) | .01337 (.01652) |
| Spatial lag | .12178** (.02855) | .08613** (.01460) | .19497** (.03347) | .17122** (.02441) | .12028** (.02012) | .17854** (.02047) |
| Disadvantage | .06564** (.01736) | .05829** (.01131) | .07084** (.01416) | .12999** (.02157) | .08661** (.01870) | .05833** (.00809) |
| Immigration | −.03401** (.00941) | −.00728** (.00260) | −.05025** (.01026) | −.07434** (.01286) | −.01733** (.00431) | −.05332** (.00784) |
| % Black | .01625** (.00486) | −.00091 (.00162) | .01093** (.00267) | 05281** (.00898) | .00598** (.00207) | .00558* (.00252) |
| % Same residence | −.25919** (.06796) | −.21940** (.04344) | −.91391** (.18892) | −.50384** (.08613) | −.35206** (.07845) | −.75561** (.11583) |
| % Divorced | −.00904* (.00421) | .01599** (.00478) | .02212** (.00535) | −.03826** (.00784) | .01269** (.00448) | .00590 (.00442) |
| % Unemployed | −.00189 (.00196) | −.01001** (.00290) | −.01043** (.00291) | .00350 (.00253) | −.01473** (.00405) | −.00781* (.00311) |
| % Vacant | .01941** (.00538) | .00555** (.00211) | .00006 (.00192) | .04769** (.00816) | .00976** (.00310) | −.00682* (.00270) |
| % 15-24 | −.02160** (.00695) | −.01144** (.00414) | −.02631** (.00660) | −.04824** (.00897) | −.01945** (.00565) | −.01104* (.00454) |
| Population | −.11997** (.03119) | −.09497** (.01718) | −.33256** (.06655) | −.23813** (.03926) | −.15876** (.03314) | −.34382** (.05298) |
| Wald $\chi^2$ | 43019** | 36609** | 52806** | 60446** | 66138** | 53943** |
| Instruments | 93 | 92 | 91 | 91 | 95 | 90 |
| A-B AR(2) test[3] | $p$ = .567 | $p$ = .921 | $p$ = .235 | $p$ = .917 | $p$ .763 | $p$ = .565 |
| Over-id. test[4] | $p$ = .964 | $p$ = .304 | $p$ = .193 | $p$ = .168 | $p$ = .232 | $p$ = .136 |
| No. of obs. | 86357 | 82069 | 49005 | 86357 | 82069 | 49005[4] |

[1]Arellano-Bond regression models. Standard errors in parentheses. Period effects not shown. All variables logged (base e) except disadvantage and immigration factor scores.
[2]Proportion of stops resulting in arrest.
[3]Arellano-Bond test for second-order autocorrelation.
[4]Sargan test of over-identification restrictions.
**$p$ < .01; *$p$ < .05 (two-tailed).

and property crime reductions in New York census tracts, and the effects may persist for three months. The estimated effects on crime rates of stops of whites are consistently larger than the effects of stops of blacks and Hispanics. The race- and ethnic-specific effects are also larger than the total SQF effects reported in Table 2. That could reflect stops of persons of a particular group in areas populated primarily by members of another race or ethnic group, resulting in high group-specific stop rates. This possibility should be kept in mind when interpreting the group-specific results in Table 3.

We observe no significant effects of race-ethnic specific arrests on crime rates, with the exception of a significant negative effect on violent crime for arrests of black suspects. In light of the number of coefficients estimated in these analyses, that result could be due to chance. By contrast, the consistency in the significant effects of total and race-ethnic specific stops on both violent and property crime rates indicates that those results are unlikely attributable to chance.

As before, more disadvantaged neighborhoods, those adjacent to high-crime neighborhoods, and, with one exception, those with higher vacancy rates experienced higher crime rates. Higher levels of immigration and residential stability are associated with lower crime rates. In these analyses and those reported in Table 1, other covariates are significantly associated with violent and property crime rates, although the direction of the relationships is not always obvious. For example, the percentage of 15-to-24 year-old males, a high-crime population group, is consistently associated with lower neighborhood crime rates. That result could reflect movement out of high-crime neighborhoods by families with children, an interpretation consistent with the finding that smaller census tracts tend to have higher crime rates.

The major results of the regression analyses are that (1) census tracts in which SQF stops were more frequent experienced significantly lower monthly violent and property crime rates than did those with lower stop rates during the period of rapid growth in the policy in New York; (2) with one possible exception, census tracts where stops resulted in higher proportions of arrests did not have significantly lower crime rates; and (3) these results withstand controls for a large number of substantive, spatial, and temporal covariates. Finally, the results are robust against tests for autocorrelation in the errors and exogeneity of the instruments in the regression models.

## Assessing Impact

Given the large number of observations on which the regression results are based, even small effects or weak relationships are likely to be statistically significant. Absent a standard for comparison, however, it is difficult to say whether the estimated effects of SQF on crime rates are large or small. We can, however, calculate the expected crime reductions in New York City neighborhoods, given the estimated effects of SQF stops on violent and property crime over the 72-month observation period. Based on the results shown in Table 2, a 10% increase in the SQF stop rate is associated with a contemporaneous decrease of -.054% in the monthly violent crime rate.[11] Additional reductions in violent crime are associated with the lagged effects of SQF stops. Summing the coefficients on the contemporaneous and lagged stop rates, the total reduction in the violent crime rate associated with a 10% monthly increase in the SQF stop rate is −2.090%. The reduction in property crime associated with a 10% increase in the lagged SQF stop rates (the contemporaneous effect is not significant) is −3.743%. Given an average number of police stops in New York City census tracts of approximately 27 per month and average numbers of violent and property crimes of approximately 2 and 3.5 per month, respectively, a 10% increase in stops would yield a reduction of .039 violent crimes and .131 property crimes per month.[12]

Monthly crime changes of this magnitude appear quite small, but they assume somewhat greater importance when calibrated according to observed changes in the frequency of SQFs over time. For example, as noted earlier, the number of SQFs plummeted by 72% through the end of 2013 from a peak in 2011. SQFs declined at an average rate of 2% per month over the 36-month period. Based on the regression results reported here, the expected monthly violent and property crime increases given a 2% drop in SQFs are .042 and .075%, respectively (i.e., twice the summed SQF coefficients). When accumulated over 36 months, these results imply an increase in violent crime rates of 1.51% and an increase of 2.70% in property crime rates, assuming no change in other factors affecting crime. Meanwhile, violent and property crime rates in New York registered small increases from 2011 to 2012, followed by small decreases in 2013, resulting in effectively no net change in crime rates over the period (https://www.fbi.gov/stats-services/crimestats). If the substantial reduction in SQFs pushed crime rates up slightly, other factors may well have exerted pressure in the opposite direction.

The monthly crime changes reported here are not necessarily trivial, but the key policy question regarding SQF is whether comparable reductions could have been achieved by less contentious means. Now that New York is well into what might be called its "post-SQF" era, and alternative crime-control strategies are tried, that question assumes added urgency.

11. Because a constant was added to the original measures before taking the log, these impact estimates should be treated as approximations.

12. The mean monthly stop rate is 6.900 stops per 1,000 tract residents. The mean monthly violent and property crime rates are .484 and .908 crimes per 1,000 tract residents, respectively. The mean number of residents per tract is 3,884 (see Table 1). Therefore, the mean number of stops per month is 26.800 (6.900 × 3.884), the mean number of violent crimes is 1.880 (.484 × 3.884), and the mean number of property crimes is 3.527 (.908 × 3.884). A 10% increase in the mean number of stops per month would yield 2.680 additional stops. Applying the summed coefficients reported in the text and Table 2, an increase of 2.680 stops would result in .039 (1.880 × .021) fewer violent crimes and .131 (3.527 × .037) fewer property crimes per month.

## Discussion

New York's SQF policy has been hotly debated and subject to court intervention. Both sides of the debate make assumptions about the effects of SQF on crime, but with little guidance from empirical research. Part of the reason for the evidence-free quality of the debate is undoubtedly the limited available research on the relationship between SQF and crime. The current study builds on the prior research by investigating the relationship between SQF and monthly crime rates in New York City census tracts between 2006 and 2011—a period of substantial growth in SQF. We find statistically significant reductions in both violent and property crime rates associated with the number of SQF stops per 1,000 tract population over the 72 months covered by the study. We also find that stops of white, black, and Hispanic suspects were all significantly associated with violent and property crime reductions.

The effects of stops of whites are larger than those for blacks and Hispanics, results consistent with criticisms that SQF stops of racial and ethnic minorities were motivated by bias rather than evidence of suspected criminal activity. Complicating that interpretation is the fact that stops of whites were no more likely than stops of blacks and Hispanics to result in arrest. The fact that stops of blacks and Hispanics were not associated with larger crime reductions than stops of whites, however, calls into question the claim that the group disparities in stops simply reflect corresponding group differences in criminal involvement (Mac Donald, 2013). The small number of monthly stops of whites across New York census tracts could result in some parameter instability, which should be considered when interpreting the race-ethnic specific results.

We used dynamic panel models to estimate the crime reductions associated with SQF stops and arrests. The models identify the effect of SQFs on crime using lagged values of the outcome and endogenous and exogenous predictors as instruments. The reported results are based on the best-fitting models with lag structures and instrument sets that withstood tests for autocorrelation and exogeneity of the instruments. But we could not estimate all possible models that may have met these selection criteria, and the results therefore are subject to some model uncertainty. The results imply that crime reductions associated with a monthly dose of SQFs are relatively small and short lived—they dissipate after two-to-three months. The policy must be applied continuously to maximize its crime-reduction effects over the long term.

Two issues ripe for future research are possible feedback loops in the relationship between SQF and crime and the impact of SQF on the daily round of residents where stops were heavily concentrated.[13] If police stops both respond to and reduce crime, as we have found, then fewer police stops should eventually occur in those areas where crime has decreased, possibly resulting over time in an uptick in crime. This possibility suggests that the

13. We thank an anonymous reviewer for both suggestions.

relationship between police stops and crime could be calibrated to achieve something of an optimal equilibrium in which the police make just the number of stops, but no more, to achieve maximum crime reduction. Given the requisite research, that should not be too much to ask of the planning and analysis staff of a big-city twenty-first century police department.

The second issue for subsequent research is whether SQF, especially during its expansion phase, may have reduced crime in part by causing some persons to avoid locations where the police were heavily concentrated, resulting in crime reductions in those areas and crime increases in other areas. The crime spatial lags in our models should effectively control for this possibility in adjoining census tracts, but cannot control for such crime displacement within census tracts. Weisburd et al. (2015) found little crime displacement around New York City street segments where SQF stops were concentrated, but additional research, including ethnographic studies, is needed to document the effect of SQF on how residents, both law abiding and criminal, conduct their daily lives.

With a single exception that may be a chance result, we observe no significant crime reductions associated with the fraction of SQF stops resulting in an arrest. An early study of the effect of police stops and arrests on crime reported a similar finding (Boydstun, 1975). On its face, the fact that SQF arrests produce no additional crime-reduction benefit implies that they have no greater deterrent or incapacitation effect on crime than SQF stops that do not end in arrest. That interpretation is in line with evidence that relatively few SQF arrests result in prosecution or conviction (Schneiderman, 2013). But if we assume that stops that do culminate in an arrest are more likely to meet the constitutional standard of reasonable suspicion than those that do not, another interpretation is possible. Bellin (2014, p. 1550) has argued that SQF "probably "worked" precisely because of the very aspects that render it unconstitutional"—notably, the mass, invasive, prison-like scrutiny of thousands of individuals *without* reasonable suspicion of criminal activity. In short, the effectiveness of SQF as a crime-fighting strategy is "inversely proportional to its constitutionality" (Bellin, 2014, p. 1548). Recall, however, that a recent study found that only probable cause stops, which presumably meet Constitutional requirements, were associated with crime reductions in New York's impact zones (MacDonald et al., 2016).

Some analysts have gone further and questioned the external validity, policy relevance, and ethics of evaluating the "effectiveness" of a crime-control program that has been ruled unconstitutional (e.g., Sweeten, 2015). After all, there are many morally offensive criminal justice practices—the Soviet Gulags come to mind—whose crime-control effectiveness is simply beside the point. Yet, few observers want the police to refrain from detaining persons they believe, with good reason, have committed or are about to commit a crime. As such, it is important to know whether police stops that pass constitutional muster reduce crime and whether alternative procedurally just methods are more or less effective.

Future research on the relationship between SQF and crime, in New York and other cities, should also build on the current results showing no greater, and perhaps smaller, crime reductions in stops of blacks and Hispanics than for stops of whites. In light of past research and the ongoing controversy regarding disparate police treatment of members of minority groups, policymakers and law enforcement officials need empirically sound guidance about the consequences for crime of the large racial and ethnic differences in police stops of whites and minorities.

The policy question is always, compared to what? Would NYPD officers have been just as effective in reducing crime through their sheer visibility, without stopping, questioning, and frisking hundreds of thousands of suspects? Would a strategy of making fewer stops of higher quality that resulted in far more arrests have been just as effective? We don't know. The reason we don't know is that SQF was implemented with little evident regard for finding out which aspects of the policy produced crime reductions and which had little or no effect on crime. We cannot even conclude with certainty that any aspect of SQF caused crime to decrease in New York.

Strong causal inferences require experimental data and methods, and even then some caution is warranted (Sampson, 2010). SQF was not an experiment. It was rolled out all at once, and no areas were withheld from the program for comparison purposes. As in all prior studies, we use statistical procedures to disclose relationships between SQF and crime rates, but we cannot conclude with the kind of certainty one might desire when evaluating a significant policy initiative that the observed changes in crime rates are attributable to corresponding changes in SQF. That level of certainty is possible only after careful and repeated experimentation. We are left with the much weaker inferences that can be drawn from observational data, the lapses into "causal speak" in this paper notwithstanding. At best, we can conclude that, based on the methods and data used in this study, SQF was statistically associated with crime reductions in New York City neighborhoods between 2006 and 2011.

Now that a federal court has ordered changes to SQF and the number of police stops has been dramatically reduced, an opportunity exists to better gage the policy's causal effects on crime. The policy reforms should be evaluated experimentally. Of course, all changes must meet constitutional standards for equitable and unbiased treatment of citizens, but we see no reason why, with careful monitoring, most cannot be subject to randomized controlled evaluation. For example, it should be possible to systematically vary reductions in police stops, or increases in police presence without corresponding increases in stops, across otherwise similar places to determine the optimal mix and dosage of enforcement strategies that safeguard the rights and liberties of citizens while enhancing public safety. Police-researcher partnerships in other cities provide a model for this kind of flexible, ongoing, and evidence-based approach to policy evaluation (e.g., Ratcliffe, Taniguchi, Groff, & Wood, 2011; Rosenfeld, Deckard, & Blackburn, 2014). We hope that one lesson police and city officials, in New York and elsewhere, draw from the controversy over

SQF is that the manner in which a policy is implemented can go a long way toward resolving questions regarding its effectiveness.

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## Appendix: Deriving Monthly Estimates of the ACS Covariates

We used polynomial curve fitting to obtain monthly estimates of the census covariates from three distinct point estimates. The 2005-2009 ACS data were used as the point estimate for the monthly 2006 data. The 2007-2011 ACS data were used as the point estimate for the monthly 2010 and 2011 data. For the 2007-2009 point estimates, the period of overlap between the two 5-year ACS data files, the average value of the 5-year estimates for each measure was calculated for each unit as the monthly source data. Having expanded these three point estimates to represent monthly values in their assigned years, we further refined the monthly data by fitting polynomial curves to the monthly source data. Linear, quadratic, and cubic polynomials were fit for each measure, in each geographic unit. Likelihood ratio tests were used to select the polynomial form that best fit the data for each geographic unit. Predicted values from the best fitting polynomial curve were obtained and used as monthly estimates of the socio-demographic characteristics included in the analysis.