RESEARCH ARTICLE

STOP, QUESTION, AND FRISK PRACTICES

Do Stop, Question, and Frisk Practices Deter Crime?

Evidence at Microunits of Space and Time

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Research Summary

Existing studies examining the crime impacts of stop, question, and frisks (SQFs) have focused on large geographic areas. Weisburd, Telep, and Lawton (2014) suggested that SQFs in New York City (NYC) were highly concentrated at crime hot spots, implying that a microlevel unit of analysis may be more appropriate. The current study aims to address the limitations of prior studies by exploring the impact of SQFs on daily and weekly crime incidents in NYC at a microgeographic level. The findings suggest that SQFs produce a significant yet modest deterrent effect on crime.

Policy Implications

These findings support those who argue that SQFs deter crime. Nonetheless, it is not clear whether other policing strategies may have similar or even stronger crime-control

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outcomes. In turn, the level of SQFs needed to produce meaningful crime reductions are costly in terms of police time and are potentially harmful to police legitimacy.

Stop, question, and frisks (SQFs) have long been used by the police as a method of crime control. SQFs are also known as Terry stops because the 1968 Supreme Court decision in *Terry v. Ohio* (1968) gave officers the right to stop and detain a person when there was reasonable suspicion that he or she was in the act of committing a crime or about to commit a crime (see Jones-Brown, Gill, and Trone, 2010). In New York City (NYC), the use of SQFs has received particular attention both from its proponents and detractors (Harcourt and Ludwig, 2006; Kelling and Sousa, 2001; McArdle and Erzen, 2001). One reason for this is that the number of SQFs increased dramatically in the first decade of this century (Weisburd, Telep, and Lawton, 2014). In all, 160,851 SQFs were recorded in NYC in 2003, but the number of SQFs reached more than 685,000 in 2011. Successful court challenges to the strategy and changes in NYC politics during the past few years have brought a reversal in the use of the tactic by the New York City Police Department (NYPD). Only approximately 190,000 SQFs were recorded in 2013—a 72% decline from its peak in 2011—and just 47,000 in 2014. Nonetheless, the returning police chief in NYC, William Bratton, recently argued, "You cannot police without [SQFs]. If you did not have it, you'd have anarchy" (Bratton, 2014). Although SQFs are being curtailed, they are still considered an important part of crime-prevention activities of the NYPD.

The potential unintended negative consequences of SQFs are widely recognized and have become extremely controversial, leading some scholars to argue that the practice is likely doing more harm than good (see Fagan, Geller, Davies, and West, 2010). The approach has been criticized for targeting the young, minorities, and specific neighborhoods of NYC (see Gelman, Fagan, and Kiss, 2007; Ridgeway, 2007; Stoud, Fine, and Fox, 2011). Opponents have argued that the pervasiveness of SQFs in NYC in the 2000s could not be justified on constitutional grounds and that the abuse of the policy by the NYPD should be considered a crime (Charney, Gonzalez, Kennedy, and Leader, 2010). In *Floyd v. City of New York* (2013), the U.S. Federal Court of the Eastern District of New York ruled that SQFs as carried out in NYC were unconstitutional and appointed a special monitor to institute substantive reforms (see Center for Constitutional Rights, 2015; Goldstein, 2013). Notwithstanding this court ruling and criticism of the policy more generally, the police and proponents (whether advocating a maximalist or a minimalist approach) argue that SQFs provide an effective method for reducing crime (Meares, 2014). Indeed, with a recent rise in shootings and homicides in NYC in 2015, the media has considered the decline in SQFs as a key culprit (e.g., Destefano, 2015). Accordingly, a key question in the debate about SQFs is whether the tactic deters crime.

Weisburd et al. (2014) suggested that SQFs are likely to reduce crime because they are concentrated at microgeographic crime hot spots. They found that crime was very concentrated in NYC as it is in other large urban areas (see Weisburd, 2015). Just 5% of street segments produce 50% of the crime on street segments, whereas 5% of intersections produce more than 50% of crime at intersections. SQFs were even more highly concentrated than crime. Just 5% of street segments produce 77% of SQFs on segments, whereas 5% of intersections produce more than 50% of SQFs at intersections. The strong correlations between crime and SQFs at street segments (rho above .594) and intersections (rho above .614) provide empirical evidence to suggest that the NYPD used the tactic as a hot spots policing strategy (Weisburd et al., 2014). In turn, strong empirical literature has now established that if the police target crime hot spots, then they can deter crime (Braga, 2007; Durlauf and Nagin, 2011; Nagin, Solow, and Lum, 2015; Telep and Weisburd, 2012; Weisburd and Eck, 2004). Nagin (2013) argued that the underlying model of deterrence created by hot spots policing is one in which the police act as sentinels, whereby their presence discourages offending through increasing certainty of apprehension.

The finding that SQFs are targeted at places where crime is concentrated suggests that the deterrence model of focusing police resources is relevant to understanding SQF effects. Yet, little evidence exists to suggest that SQFs contributed to the crime declines in NYC. A key problem in identifying the impact of SQFs on crime is to untangle two potential causal mechanisms. The first is that SQFs are a response to crime. Accordingly, if you observe more SQFs at a given place, then it is likely because crime has been reported at that place. The second is that SQFs deter crime. In this case, by drawing from the evidence on hot spots policing and recent deterrence theories, we hypothesize that SQFs brought to a place will reduce the level of crime. Distinguishing these two causal chains is difficult if they are occurring within short time periods. For example, if we examine crime over a year, then we can expect that there have been cycles during which SQFs are a response to crime and where the tactic would lead to drops in crime. This set of relationships may repeat itself multiple times. Estimates of causality in such situations are not believable (Loftin and McDowall, 1982; Marvell and Moody, 1996; Yang, 2007) because it is difficult to disentangle this loop effect statistically.

To examine the effect of SQFs on crime, our first step is to understand whether SQFs and crime are following a long-term equilibrium relationship at a microgeographic level. Our finding, which will be reported next, that they are strongly co-integrated, leads us to two analytic approaches that try to solve the problem that arises as a result of the temporal confounding of crime and SQFs. The first uses an instrumental variables approach based on Bartik's (1991) instrument to examine impacts of SQFs on weekly crime data across NYC over a 6-year period. The second uses space–time interaction models (Diggle, Chetwynd, Häggkvist, and Morris, 1995) to examine the impacts of an SQF on crime over five days and within 500 ft of the occurrence of an SQF. Both approaches suggest a significant deterrent impact of SQFs on crime. In concluding, we discuss the findings and limitations of these analyses.

Data

One reason for the lack of empirical analyses of SQFs in NYC is that the data for conducting such studies are not publicly available. Although information on SQFs and their location have been published since 2003 (Rosenfeld and Fornango, 2014; Smith and Purtell, 2007, 2008; Weisburd et al., 2014), crime data at the microgeographic level are restricted and the NYPD has not routinely released such data to researchers. Our research was funded by a grant to the John Jay College from the Open Society Foundations. As part of that effort, the NYPD agreed to provide information on crime at a microgeographic level. We had access to data on both the exact locations of SQFs and all non–traffic-related crime incident data that occurred in NYC, geocoded at the address level for the years 2006 to 2011.[1] The SQF data are publicly available through the NYPD's website. The data set provided included *x–y* coordinates of SQFs and crime incidents, as well as their dates of occurrences. All events were geocoded by the NYPD, and a hit rate of 97.5% and 97.1% was obtained for crime incidents and SQFs, respectively.

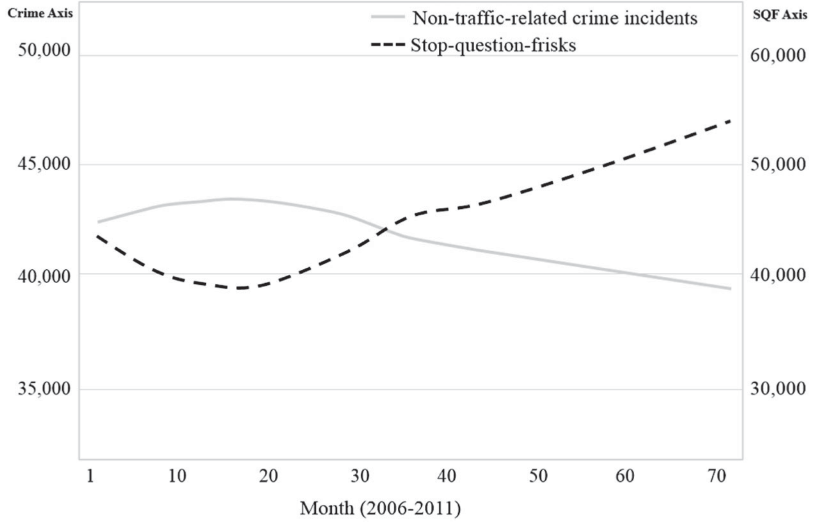
Data on crime incidents are routinely used to examine crime by researchers and the police. Incident reports are generated by police officers or detectives after an initial response to a request for police service. If the officer believes that response to be founded, then an incident report is recorded. In this sense, incident reports are more inclusive than arrest reports but less inclusive than calls for service. According to the New York Civil Liberties Union's website, "[e]very time a police officer stops a person in NYC, the officer is supposed to fill out a form to record the details of the stop" (New York Civil Liberties Union, 2012: para. 16). Officers fill out the forms by hand, and then the forms are entered manually into a database. We are told by the NYPD that SQFs are consistently reported from 2003, but we do not have any data to evaluate this assertion. We discuss reporting questions and their possible implications for our findings in more detail in the Discussion section.

Figure 1 shows the number of SQFs and the number of crimes in NYC for each month that we studied. Overall, SQFs increased during this period and crime decreased. This trend has led some observers to assume that SQFs are the cause of crime declines in NYC (Chauhan, 2011; Eligon, 2012; Zimring, 2012). However, drawing such strong causal conclusions from time series data of this type is unwarranted because the relationship could be a result of many external factors influencing both SQFs and crime (Greenberg, 2014).

1. The geocoding hit rate for these data is higher than the 85% suggested threshold for a minimal reliable geocoding rate (Ratcliffe, 2004). Crime incidents involving rape and other sex crimes were not included in the analyses because the *x–y* coordinates were redacted by the NYPD (1.2% of incidents). A street segment is defined at a street from intersection to intersection. Pedestrian walking paths, freeways, Amtrak/train tracks, on- and off-ramps to highways, shorelines, and private drives were excluded from the street centerline file. Because only *x–y* coordinates were provided by the NYPD, a total of 25 segments had to be excluded from the present sample because of concerns about potential magnet phone locations on these segments.

FIGURE 1

Total Crime Incidents and SQFs in NYC by Month



Notes. Trends smoothed using the loess command in *R*.

By using space–time interaction models, we can examine crime and SQFs according to their exact spatial location. But in the co-integration and regression analyses presented next, we have to choose a specific geographic unit. Our focus is on the impacts of SQFs on crime at street segments. Most crime (80.3%) in NYC was reported to have occurred at these geographic units. We did not merge intersection crime to street segments because they are not attached to any unique street segment (see Weisburd, Bushway, Lum, and Yang, 2004; Weisburd, Groff, and Yang, 2012). However, in assessing the deterrent impact of SQFs at the street segment, we hypothesized that SQFs occurring on a given intersection would influence all streets tied to that intersection. In turn, the fact that 55.5% of SQFs occur on intersections in our data means that such events are particularly important in assessing their impacts on crime. As such, SQFs on intersections are merged for our analysis so that each street segment has a total number of SQFs equal to the street and its two adjoining intersections. In Table 1, we follow this approach and report on the weekly and monthly averages of crimes and SQFs at a microgeographic level (street segments).

On average, street segments in the city had much less than one crime per month (and 0.08 crimes per week). However, for the hottest 1% of street segments in terms of crime (which produce 25% of crime at street segments), the average was more than seven crimes (and 1.7 per week). Similarly, the average street segment had approximately one SQF a

TABLE 1

Summary of SQFs and Crime Incidents at the Street Segment Level

Variable	Mean	(SD)
Monthly ($n = 72$ months)		
Crime Incidents		
Average number of crimes on segments	0.37	0.02
Average number of crimes on top 1%	7.20	0.35
SQFs		
Average number of SQFs on segments	1.27	0.23
Average number of SQFs on top 1%	15.50	2.51
Weekly ($n = 312$ weeks)		
Crime Incidents		
Average number of crimes on segments	0.08	0.01
Average number of crimes on top 1%	1.66	0.08
SQFs		
Average number of SQFs on segments	0.29	0.02
Average number of SQFs on top 1%	3.58	0.25

Notes. SD = standard deviation. Data are aggregated at the monthly ($n = 72$) and weekly ($n = 312$) time units for the years 2006 to 2011. Summary is for all street segments in NYC ($n = 87{,}254$) and the top 1% of these street segments ($n = 873$) with the highest number of crime incidents during the study period. Totals include SQFs occurring on the street segment and its intersections, whereas crimes are only those occurring on the street segment.

month (0.29 per week) during the period of study, whereas the hottest 1% of segments had on average 15 SQFs a month (3.58 per week).

Co-Integration of SQFs and Crime

We first set out to examine empirically whether SQFs and crime are co-integrated in a standard time period at a microgeographic level. Co-integration is a statistical term that refers to two time series that follow an equilibrium relationship and move together so closely over time that when one series increases or decreases, the other series will change to maintain the equilibrium between the two (Britt, 2001; O'Brien, 1999). To determine the long-term effects of SQFs on crime, we must first establish the fact that the two trends are integrated of the same order. Specifically, if the crime trend is stationary while the SQFs trend wanders around with no tendency to return to its original level (i.e., having a unit root), then it is unreasonable to believe that one has a long-term permanent impact on the other. We focus in the co-integration analyses only on the worst 1% of crime hot spots in NYC (873 street segments) over the 72-month period studied. If there is a relationship between SQFs and crime, then it should be more apparent and meaningful among high-activity streets.[2] We used panel unit root and panel co-integration tests to determine whether a long-term

2. One street did not have any SQFs, and it was omitted from the subsequent analysis.

TABLE 2

Panel Unit Roots Test Results with Individual Effects

Variable	Levin, Lin, and Chu (probability)	Im, Pesaran, and Shin (probability)	ADF Fisher Chi-square	PP Fisher Chi-square
Crime	−174.282***	−175.075***	22,361.00***	23,806.10***
Δ Crime	−228.011***	−280.800***	22,484.40***	16,088.80***
SQF	−136.275***	−141.299***	18,644.80***	19,912.40***
Δ SQF	−260.994***	−291.687***	21,167.90***	16,127.00***

Notes. The null hypothesis for the test is nonstationarity (i.e., unit root exists) of the trends. Δ indicates the trend of the first differences.
***$p < .001$.

TABLE 3

Panel Co-Intergration Test Results

Crime and SQFs	Panel Statistics	Group Statistics
Variance ratio	11.508**	—
Rho statistic	−217.576***	−258.116***
Philips-Perron statistic (nonparametric)	−135.596***	−190.760***
ADF statistic	−74.317***	−112.814***

Note. The null hypothesis for the test is no co-integration between the trends.
**$p < .01$. ***$p < .001$.

relationship exists between monthly SQF and crime trends of street segments (Hamilton, 1994; Pedroni, 1999).[3]

The panel unit root test estimates for crime and SQF trends are presented in Table 2, as well as the results of the trends after taking the first differences.[4] These analyses suggest that the trends are stationary in both level and first differences. The individual unit root tests that we conducted as a sensitivity analysis reveal the same story. The panel co-integration estimates presented in Table 3 indicate that the results across different statistics are consistent.[5] All of the statistics reject the null hypothesis (all group tests are significant at .01

3. The following conditions must be met for the trends to be co-integrated: (a) the Augmented Dickey–Fuller test needs to indicate that the trends are integrated of the same order (Granger, Clive, Huang, and Yang, 2000); (b) if SQFs are integrated of order two, then crime can be co-integrated with the SQF trend only when crime is also a second-order trend (Hamilton, 1994; O'Brien, 1999); and (c) the difference between the two time series needs to be stationary (Miller and Russek, 1990).

4. The first difference of a time series is the series of changes from one period to the next (i.e., $x_t - x_{t-1}$, the difference between two consecutive years).

5. For the panel statistics, the first-order autoregressive term is assumed to be the same across all the cross sections. As for the group panel statistics, the parameter is allowed to vary across the cross sections (in

levels), suggesting a stable and long-term relationship exists between crime and SQFs. The evidence indicates strongly that crime and SQFs are co-integrated at the street segment level.

These findings led us to consider approaches that tried to disaggregate the SQF and crime relationship not only at the microgeographic level but also within tight bands of time. Next, we present two different approaches to identifying the impacts of SQFs on crime over short time periods.

Findings: Bartik's Instrumental Approach

The first analysis employs an instrumental variables approach based on Bartik's (1991) instrument. Bartik capitalized on nationwide labor trends that allowed him to focus on exogenous shifts in regional labor demand (as opposed to shifts in regional labor supply). As described next, we use borough-level SQF trends as an instrument to identify exogenous SQF shocks at the street segment level.

We measure the effect of SQFs conducted by the NYPD on non–traffic-related crime incidents in NYC each week during a 312-week period (from January 2006 to December 2011) across all 87,254 street segments. We estimate the following linear probability model:

$$\Pr(\text{Crime}_{it}) = \alpha \text{SQF}_{it-1} + x_{it}\,\beta + (v_i + u_{it})$$

The estimate of interest is α, the effect of the number of SQFs that occurred last week on a street segment and its two intersections on the probability that a crime occurs this week on the street segment.[6] We control for x_{it}, observed demographic characteristics of the population surrounding the given street segment, as well as the number of SQFs at the precinct level.[7,8] The error term $(v_i + u_{it})$ corresponds to all characteristics that affect crime but are unobserved by the researcher. The first term v_i captures unobserved characteristics of the street segment that remain constant across time. The second term u_{it} applies to unobserved characteristics that affect crime and vary across time and location.

The primary concern in estimating the deterrence effect (α) is that SQF_{it-1} might be correlated with characteristics in the error term. We have already mentioned that areas with higher crime rates tend to have more SQFs. Thus, without controlling for street segment fixed effects, we are likely to estimate a positive and significant coefficient on SQF_{it-1}. This

our case, street segments) (Ramirez, 2006; Ramirez and Sharma, 2008). On the one hand, if the panel statistics lead to rejection of the null, then crime and SQFs are co-integrated for all streets. On the other hand, if the null is rejected by the group panel statistics, then co-integration exists for at least one street.

6. These data are organized at the weekly level, and thus, we cannot determine whether the SQF occurred before or after the reported crime. We therefore focus our analysis on the impact of an SQF this week on crime in the following week.

7. Demographic characteristics such as race, income, and so on were obtained from the U.S. Census Bureau.

8. We do not have access to data on the number of hours worked by police officers and therefore proxy for this with the number of SQFs conducted at the precinct level.

outcome would likely be a result of proactive police deployment and not deterrence. We therefore run a fixed-effects estimation comparing crime at each street segment with crime rates at that same segment on weeks with different SQF rates.

Even when looking at within-street segment (and attached intersection) changes in SQFs, an increase in SQFs at a given street or intersection could be, for instance, a result of a chain of burglaries occurring across time at that precinct. This would result in a positive bias on α as SQFs were conducted as a response to increased crime risks. We therefore construct a simplified Bartik instrument focusing on SQF trends occurring outside of the precinct:

$$\text{Bartik}_{it} = \frac{S_t - S_{t-1}}{S_{t-1}}$$

where S_t refers to the number of SQFs conducted in the borough of precinct i, excluding precinct i.[9] The instrument provides a causal interpretation of the coefficient α as long as the increase in SQFs at the borough level was driven by an exogenous factor (e.g., a surge in officer hiring, a change in policing strategy, increased funding for overtime hours).[10] We think using the entire borough (excluding the precinct in which the SQF occurs) reduces the potential for confounding spatial effects, such as the influence of crime shocks, in areas immediately surrounding the intersection.

Table 4 provides estimates of α in the Bronx (which has both a high number of SQFs and high crime rates) by using a linear probability model based on Bartik's instrument. Without controlling for differences between locations, we find in specification (i) that the occurrence of an SQF last week increases the probability of a crime at this street segment by 3.8 percentage points, implying that SQFs significantly increase crime. This measured effect disappears when including location fixed effects in specification (ii), suggesting that the measured impact in specification (i) was a result of more SQFs occurring in crime-prone areas. A significant deterrence effect only appears after correcting for loop-effects using the Bartik instrument in specification (iii), where we find that an SQF this week results in a 2-percentage-point decrease in the probability of crime. To gain a sense of the magnitude of this change, we apply this estimate to the 8.8% average weekly crime rate in the Bronx. Thus, we find that each additional SQF last week reduces the probability of crime this week by approximately 23% (2/8.8).

9. The inclusion of street segment controls in the analysis compares the same street segment in periods with low versus high rates of SQFs. In this type of specification, α captures the effect of an additional SQF last week at the same street segment, where any unobserved characteristics of that location that remain constant over time are controlled for in the specification. The Bartik instrument intends to capture the general trend of SQFs occurring in street segment *i*'s borough. This allows us to focus more on random SQFs that are driven by borough policy or practices and not on a specific crime risk at location *i*. Importantly, it also would seem to be uncorrelated with general crime risks within street segment *i* as this information is excluded in the calculation of the instrument.

10. Although we cannot alleviate the concern that the increase in SQFs was a result of a crime increase throughout the entire borough, we would still expect a smaller bias than that present without the instrumenting strategy.

TABLE 4

Estimates of the Effect of an SQF on Crime This Week in the Bronx Using Various Specification Methods

	Specification		
	(i)[a]	(ii)[b]	(iii)[c]
Variable	B (SE)	B (SE)	B (SE)
SQFs Conducted Last Week	0.038***	0.002***	-0.020***
	(0.001)	(0.000)	(0.006)
Year and Month Fixed Effects	Yes	Yes	Yes
Location Fixed Effects	No	Yes	Yes
Bartik instrument	No	No	Yes

Notes. $N = 12{,}617$. Standard errors account for clustering at the intersection level. SE = standard error.
[a] Additional controls: the number of SQFs in the precinct this week as proxy for general police activity and census block characteristics (percentage of population with high-school diplomas, unemployment rate, income, and percentage of homes that are owner occupied).
[b] Additional control: the number of SQFs in precinct this week.
[c] This specification instruments for SQFs conducted last week at the given intersection using $\text{Bartik}_{i\,(t-1)}$. We also instrument for the number of SQFs occurring this week at the precinct level with instrument $\text{Bartik}_{i\,t}$.
$^{***}p < .001$.

TABLE 5

Findings from the Bartik Instrument Across All NYC Boroughs

	Manhattan	Bronx	Brooklyn	Queens	Staten Island
Variable	B (SE)	B (SE)	B (SE)	B (SE)	B (SE)
SQFs Conducted Last Week	–0.010†	–0.020***	–0.020***	–0.007	–0.073**
	(0.005)	(0.006)	(0.005)	(0.006)	(0.032)
Year and Month Fixed Effects	Yes	Yes	Yes	Yes	Yes
Location Fixed Effects	Yes	Yes	Yes	Yes	Yes
Bartik Instrument	Yes	Yes	Yes	Yes	Yes
N (Street Segments)	8,419	12,617	20,691	34,046	11,481
Average Crime Rate per Week	0.145	0.088	0.081	0.035	0.026
	(0.352)	(0.283)	(0.273)	(0.185)	(0.160)

Notes. SE = standard error. Standard errors account for clustering at the intersection level. SQFs conducted last week at the given intersection are instrumented for using $\text{Bartik}_{i\,(t-1)}$ .We also instrument for the number of SQFs occurring this week at the precinct level with instrument $\text{Bartik}_{i\,t}$.
$^{\dagger}p < .10$. $^{**}p < .01$. $^{***}p < .001$.

Table 5 provides estimates from the main specification by using the Bartik instrument across all boroughs in NYC.[11] These findings reinforce the results for the Bronx, which

11. Caution should be applied in the interpretation of the magnitude of these results as the overall proportional change is dependent on the base crime rate in each borough.

suggests that SQFs occurring last week have a deterrent effect on crime this week. Across all boroughs, the coefficient for the impact of SQFs on crime is negative, reflecting a deterrent outcome. Nonetheless, effects vary among the NYC boroughs with the larger estimates in the Bronx, Brooklyn, and Staten Island. We measure the largest impact of SQFs in Staten Island, which has a relatively low crime rate compared with other boroughs. Thus, each additional SQF on a street segment in Staten Island this week is associated with a 7.3-percentage-point decline in the probability of a crime occurring at that segment the following week. SQFs have a smaller impact in Manhattan ($p < .10$) and Queens ($p =$ n.s.). These results suggest that with a total of 686,000 SQFs in a given year (the highest rate of SQFs during the study period), we would expect a reduction of 11,771 crimes or a 2% decrease in crime at the city level.[12]

Findings: Bivariate Spatiotemporal K-Function

The second analysis, a bivariate space–time Ripley's K-function (Diggle et al., 1995), measures the effect of SQFs on non–traffic-related crime incidents at a daily level in the Bronx, New York.[13] We focus only on one borough because of the computational complexity of this approach. We already have evidence of a significant deterrent effect for SQFs in the Bronx from the Bartik's instrument models, suggesting that a focus on this area is also reasonable for identifying a space–time interaction. The Bronx, as noted previously, has a relatively high crime rate and a high number of SQFs. It also has a relatively small geography (larger only than Manhattan), which simplifies the computations for our analyses. The bivariate spatiotemporal K-function allows us to develop an even more discrete analysis of SQFs and crime within limited time periods (5 days) and within tight geographic distances (500 ft).

This technique uses x–y coordinates to measure the strength of the space–time relationship between SQFs and crime across time (a 5-day period after the SQF) and space, starting at the location where the SQF occurred (referred to as 0 ft) and extending to a 500-ft radius from this location. The approach is conceptually similar to prior deterrence decay studies (Sherman, 1990; Sherman, Rogan, et al., 1995; Weisburd et al., 2006) but differs in that it yields information about effects across a finite geographic distance rather than relying on units of analysis that vary in size and shape between regions (e.g., street

12. We use the 2011 distribution of SQFs between NYC boroughs: 20% of SQFs occurred in the Bronx, 33% in Brooklyn, 21% in Manhattan, 22% in Queens, and 4% in Staten Island. For instance, this distribution implies that 137,200 out of 686,000 SQFs will be conducted in the Bronx. At an average SQF deterrence rate in the Bronx of 0.02, our model predicts a decrease in 2,744 crimes. This calculation conducted for each borough results in a total crime reduction of 11,771.

13. In this subset, offense severity for the crime incidents is as follows: 27% felonies, 61% misdemeanors, and 12% violations. Crime types for the incidents are as follows: 34% personal (e.g., robbery and assault); 43% property (e.g., burglary, arson, and criminal mischief); 16% drugs, alcohol, prostitution, and public order; and 7% other (e.g., miscellaneous penal law, unauthorized use of vehicle, and other state laws).

segments) or artificially demarcated boundaries that can be redrawn (e.g., school districts and census tracts). It is also robust to any potential underreporting of SQFs that is not severely confined to specific places or times (French et al., 2006).

The estimation of the bivariate space–time Ripley's K-function, known as $\hat{D}_0(\mathrm{s}, \mathrm{t})$, is the correlation between SQFs and crime incidents that remains after correction for purely spatial (see also Ripley, 1976) and purely temporal (see also Gavin, Hu, Lertzman, and Corbett, 2006) correlations. The computation of $\hat{D}_0(\mathrm{s}, \mathrm{t})$ involves three elements:

1. $\hat{K}(\mathrm{s})$ uses information on *where* events occurred to compute an adapted Ripley's K-function (two spatial point patterns are used to measure the degree of clustering rather than a single point pattern, which is more commonly employed when using Ripley's K-function).
2. $\hat{K}(\mathrm{t})$ uses information on *when* events occurred to measure the degree to which the two separate types of events cluster in time.
3. $\hat{K}(\mathrm{s}, \mathrm{t})$ uses information on *where* and *when* events occurred to estimate clustering in time as well as space.

By using these elements, the spatial and temporal dependence between point patterns is assessed with the derived function:

$$\hat{D}_0\ (\mathrm{s},\ \mathrm{t}) = \frac{\hat{K}\ (\mathrm{s}, \mathrm{t}) - \hat{K}\ (\mathrm{s})\ \hat{K}\ (\mathrm{t})}{\hat{K}\ (\mathrm{s})\ \hat{K}\ (\mathrm{t})}$$

For the current study, $\hat{D}_0(\mathrm{s},\ \mathrm{t})$ is referred to as $\hat{D}_{SC}(\mathrm{s},\ \mathrm{t})$. A detailed description of the statistical method and model specification is available in Wooditch and Weisburd (2015).

The space–time analysis assesses the deterrent effect of an SQF by exploring how the likelihood of a crime after an SQF compares with (a) the likelihood of a crime on any given day at these locations when an SQF does not occur and (b) the likelihood of a crime immediately after a crime event that occurs in the absence of an SQF. This analysis is accomplished by comparing $\hat{D}_{SC}(\mathrm{s},\ \mathrm{t})$ with two informative null spatial patterns. The first benchmark $\hat{D}_{FS}(\mathrm{s},\ \mathrm{t})$ represents $\hat{D}_0(\mathrm{s},\ \mathrm{t})$ on any given day when an SQF did not occur.[14] Because police tend to respond to periods of high crime at these locations by increasing SQFs, the second benchmark $\hat{D}_{CC}(\mathrm{s},\ \mathrm{t})$ represents the likelihood of a crime

14. A data set of fake events (*x*–*y* coordinates and corresponding time of occurrence) were generated at locations where an actual SQF occurred during the study period. The generated events "occurred" every day a real SQF did not take place; however, because it is expected that police officers bring SQFs to areas where a crime has occurred and that there is an expected deterrent effect, fake events were not generated on days occurring close in time to a real SQF occurrence (7 days before and after). A subsample of 10,000 fake events was randomly selected from this distribution for each month, and $\hat{D}_{FS}(\mathrm{s},\ \mathrm{t})$ was computed with these events using actual crime incidents. This process was replicated 99 times (with replacement), and the final null distribution estimate of $\hat{D}_{FS}(\mathrm{s},\ \mathrm{t})$ was taken as the mean of the 99 subsample estimates, with confidence limits derived from the 2.5 and 97.5 percentiles of the subsample distribution.

event after the occurrence of a crime in the absence of an SQF at the locations where SQFs have occurred in the present sample. Inferences on the deterrent effect of SQFs will be made with these hypothesized models because they are more theoretically relevant than a distribution based on Monte-Carlo–style methods and do not suffer from incorrect Type I error rate performance (see Loosmore and Ford, 2006, for a discussion).

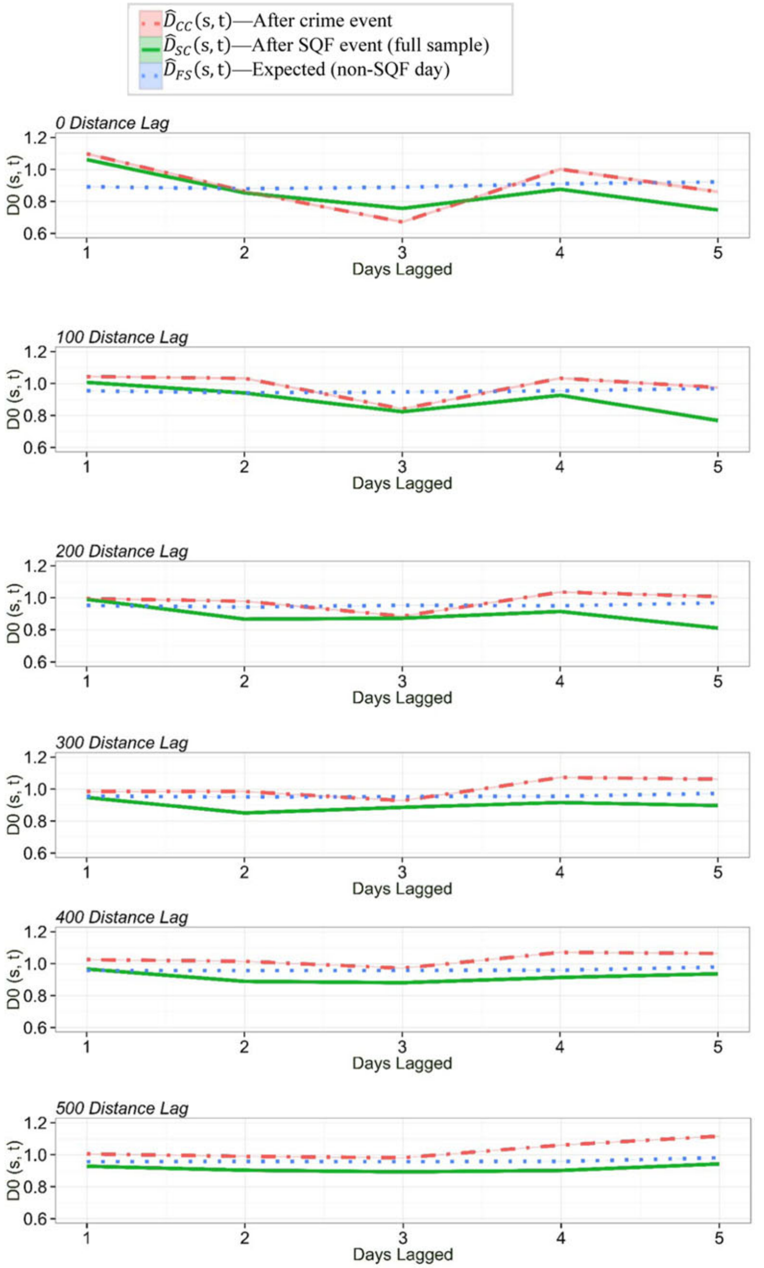
The effect of SQFs on crime is examined by using two samples. The first sample includes all SQFs from a 150-day period in 2006. This time frame had 45,027 crime incidents and 32,600 SQFs in the Bronx for a total of 77,627 events. Although this sample provides an overall view of the impacts of SQFs, it does not take into account that multiple SQFs can occur within the time and distance buffers we examine. We think this potential bias could be meaningful especially given findings that SQFs cluster at places (Weisburd et al., 2014). Even though removing co-occurring SQFs from the data set does not guarantee that their influence has also been removed, it nonetheless provides a more conservative indication of the average effect of a single SQF on crime levels. Accordingly, we develop a second model that examines a subset of SQFs in the Bronx during the same time period that were not contaminated by the influence of neighboring SQFs. To do this, points falling within a 250-ft buffer in the same 6-day period were removed from the data set. This process left a total of 4,349 SQFs that were not confounded by neighboring SQFs in time and space.

The findings using all SQFs in the 150-day period are presented in Figure 2. We first compare $\hat{D}_{SC}(\mathrm{s},\ \mathrm{t})$ with the first null spatial pattern $\hat{D}_{CC}(\mathrm{s},\ \mathrm{t})$. Although the likelihood of a crime at a location where an SQF took place is elevated the two days after its occurrence, crime levels remain significantly lower than if a crime occurred at these locations in the absence of an SQF. In fact, a crime is less likely to occur after an SQF than it is in the absence of an SQF for all time and distance scales, except in one instance where $\hat{D}_{SC}(\mathrm{s},\ \mathrm{t})$ is 0.756 and $\hat{D}_{CC}(\mathrm{s},\ \mathrm{t})$ is 0.670 ($\mathrm{CI}_{95} = 0.662 - 0.679$) (distance = 0 ft; lag = 3 days). An observed diffusion of benefits (Clarke and Weisburd, 1994) is more pronounced several days after the SQF at locations closer to where the SQF occurred (distance $\leq$ 300 ft), with the likelihood of a crime being 11.3% lower than after a crime event in the absence of an SQF (CI_{95} = 0.849 – 0.869) at the location where the SQF occurred on day 5 ($\hat{D}_{SC}$ (s, t) = 0.746; distance = 0 ft). We then compare $\hat{D}_{SC}(\mathrm{s},\ \mathrm{t})$ with the second null spatial pattern $\hat{D}_{FS}(\mathrm{s},\ \mathrm{t})$ and find that the likelihood of a crime after an SQF is 17.5% lower than crime levels on any given day at these locations (CI_{95} = 0.919 – 0.922) on day 5 ($\hat{D}_{SC}$ (s, t) = 0.746; distance = 0 ft). Comparisons with both null spatial patterns are consistent with the results of the Bartik's instrument analysis, which found that deterrent effects extend up to 1 week.

The findings using the subsample of SQFs that were not confounded in space or time, as presented in Figure 3, suggest a stronger deterrent effect. Accordingly, we think that confounding of the estimates in the entire sample is masking some of the deterrent impacts of SQFs. We first compare $\hat{D}_{SC}(\mathrm{s},\ \mathrm{t})$ of the subsample with the first null spatial pattern $\hat{D}_{CC}(\mathrm{s},\ \mathrm{t})$. At the location of where the SQF occurred, the likelihood of a crime event is approximately 30% lower than after a crime in the absence of an SQF

FIGURE 2

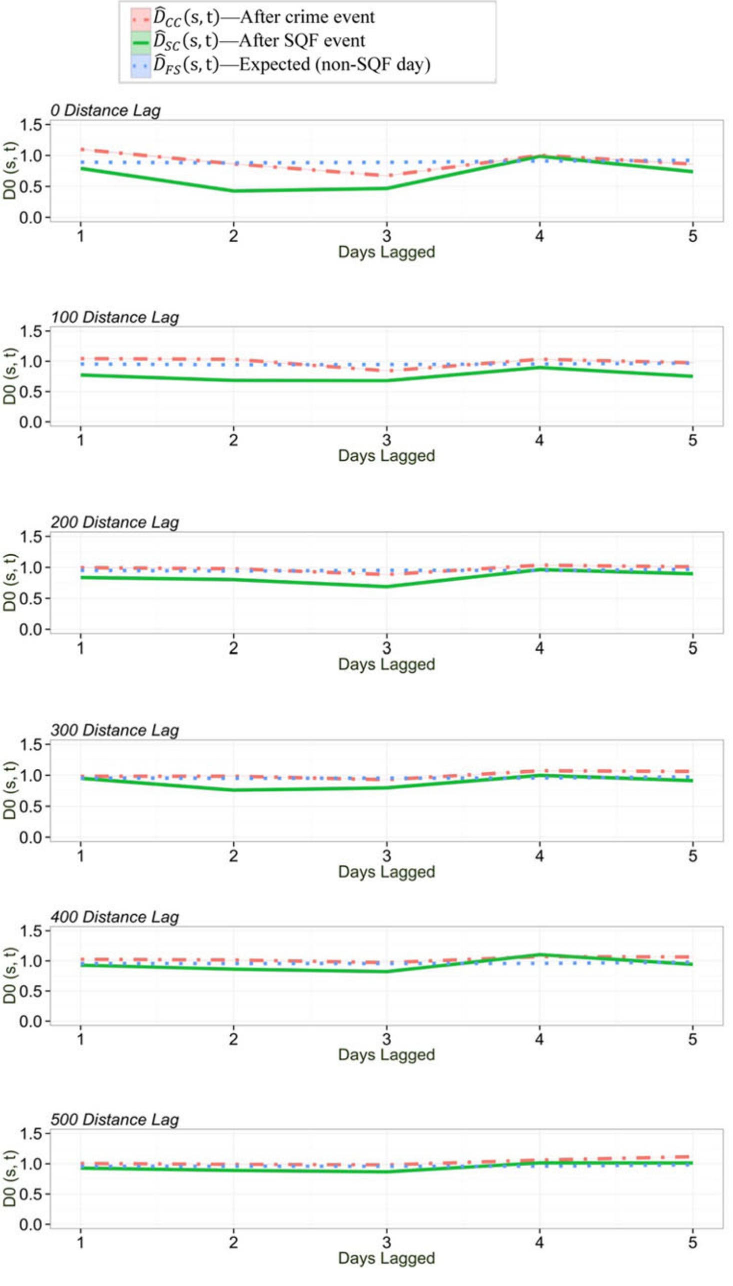
Space–Time Interaction by Distance Across Days (All SQFs)



Note. The confidence intervals for $\hat{D}_{FS}$(s, t) and $\hat{D}_{CC}$(s, t) would not be visible if included in Figure 2 but are provided in the text.

FIGURE 3

Space–Time Interaction by Distance Across Days (SQF Subsample)



Note. The confidence intervals for $\hat{D}_{FS}(s,\ t)$ and $\hat{D}_{CC}(s,\ t)$ would not be visible if included in Figure 3 but are provided in the text.

(distance = 0 ft, lag = 1 day). Moreover, the likelihood of a crime event is lower when an SQF occurred in contrast to a day a crime event occurred in the absence of an SQF across all time periods and distances examined, with one exception (distance = 400 ft; lag = 4 days). We then compare $\hat{D}_{SC}(\mathrm{s},\ \mathrm{t})$ of the subsample with the second null spatial pattern $\hat{D}_{FS}(\mathrm{s},\ \mathrm{t})$ and find that the likelihood of a crime event after an SQF is 10% less when compared with crime levels on any given day at these locations (distance = 0 ft, lag = 1 day). The distribution of both spatial null patterns suggests that the diffusion of benefits (Clarke and Weisburd, 1994) observed at distances within 300 feet of the SQF occurrence seems to disappear by day 4. Both $\hat{D}_{SC}(\mathrm{s},\ \mathrm{t})$ and $\hat{D}_{CC}(\mathrm{s},\ \mathrm{t})$ experience a slight increase between days 3 and 4 (distance = 0 ft). Even though $\hat{D}_{SC}(\mathrm{s},\ \mathrm{t})$ begins to increase, it remains below $\hat{D}_{CC}(\mathrm{s},\ \mathrm{t})$ on day 4 and day 5 (distance = 0 ft). $\hat{D}_{SC}(\mathrm{s},\ \mathrm{t})$ is 0.988 and 0.736, whereas $\hat{D}_{CC}(\mathrm{s},\ \mathrm{t})$ is 1.00 (CI_{95} = 0.991 – 1.01) and 0.859 (CI_{95} = 0.849 – 0.879) on days 4 and 5, respectively. The likelihood of a crime after an SQF is most likely to approximate the likelihood of a crime on any given day 4 days after an SQF occurrence (distance = 0 ft). Although the apparent deterrent effect is attenuated at distances farther from the location of where the SQF occurred (distance > 0 ft), similar trends in crime reductions across all days are also evident.

The conclusions we reach from these findings, at least within the observed time period and sampling frame in the Bronx, are that SQFs in NYC have immediate crime-prevention benefits across short distances and within a limited time frame (less than 5 days), that there is little evidence of spatial displacement, and that there is some evidence (especially in the restricted sample analysis) of a diffusion of crime-control benefits.

Discussion

Our study is the first we know of to isolate the impacts of SQFs on crime at a microgeographic level by using short time periods. In our view, the focus on microgeographic impacts is consistent with the approach that the NYPD used in applying SQFs. Smith and Purtell (2008) and Weisburd et al. (2014) pointed to the application of SQFs as a hot spots strategy (Braga and Weisburd, 2012; Sherman and Weisburd, 1995; Weisburd, 2008). Accordingly, if the intervention is focused on microgeographic areas where crime is concentrated, then the evaluation strategy should be applied at a similar level of geography. We focused on time periods within a week because of our concern that two possible causal chains are operating, one that reflects the fact that crime leads to SQFs at microgeographic hot spots and the second that the application of SQFs at a place deters crime. The co-integration results support that concern because they suggest that the trends of both distributions are strongly related over time.

Our findings provide support for the deterrence argument. SQFs in the models we estimate have significant impacts on crime within small areas and across short time periods. The impacts are modest but suggest meaningful declines on average in crime within weekly time periods. We find little evidence of displacement in our space–time interaction approach.

Indeed, there is some evidence of a diffusion of crime-control benefits within the 500-ft buffers examined. Crime actually declines below what one would have expected.

We are not surprised by these findings. As we noted, strong scientific evidence shows the effectiveness of police approaches that focus on hot spots of crime (Braga, Papachristos, and Hureau, 2014). Studies reporting this evidence also have suggested that diffusion of crime-control benefits to areas nearby is more likely than displacement as a response to hot spots policing (Braga, 2007; Weisburd et al., 2006). Following Weisburd et al.'s (2014) observation that SQFs are concentrated at crime hot spots, our findings seem to follow a vast array of prior studies. Moreover, our findings of short-term deterrence follow that of the short-term benefits of police crackdowns (Sherman, 1990).

But does evidence of the effectiveness of hot spots policing mean that the strategy is warranted or even desirable? One key question is whether the cost of SQFs justifies the crime prevention achieved. By using the Bartik's instrument analyses, we estimated that in the peak years of SQFs in NYC, almost 700,000 SQFs would lead to only a 2% decline in crime. Even if we assumed that the approach as practiced in NYC was constitutional, this seems like a very large police investment for a relatively small crime-prevention gain. At the same time, if we rely on estimates of the costs of crime and assume that most of the crimes prevented here were not very serious (equivalent to burglaries), then the savings to society following RAND's (The RAND Corporation, 2015) crime calculator would be more than $150,000,000. If we assumed that half of the crimes averted were serious crimes like robbery, then the benefit would be more than $400,000,000. Caution should be used in drawing such exact estimates. Crime calculators use many assumptions, which can be challenged. Moreover, our estimates give us an average predicted value. Within degrees of sampling error, the effects could be meaningfully smaller or larger. But it does suggest that even with a relatively small change in the crime rate in a city as large as NYC, meaningful societal benefits can be achieved.

Effectiveness and costs are not the only criteria for the application of police interventions in the public sphere. Many scholars have noted that democratic policing demands that the police act with restraint and with constitutional fairness in their interactions with the public (Frydl and Skogan, 2004; Skolnick, 2011). Cutting off the hand of someone who steals may have strong deterrent value and is still practiced in societies, such as Saudi Arabia. But whatever the crime-prevention benefits of such an approach, it does not fit the norms of what has become democratic criminal justice in Western societies. The Court's decision in *Floyd v. City of New York* (2013) has defined the use of an unrestricted program of SQFs, such as that in NYC, to be illegal. Currently, it seems that using SQFs as a general crime-prevention approach is not consistent with legal norms in the United States.

Irrespective of legal and constitutional issues, the police must balance effectiveness with approaches that meet public standards of legitimacy (Lum and Nagin, 2015). Recent studies of police legitimacy have suggested that although effectiveness is a key factor influencing public perceptions of the police, the way the police treat the public (or are perceived to treat the public) is as important and most times more important in understanding public trust and confidence in the police (Jonathan-Zamir and Weisburd, 2013; Kochel, Parks, and Mastrofski, 2013; Meares, 2015; Tyler, 2004). SQFs, as noted previously, often lead to charges of police bias and heavy handedness (Gelman et al., 2007; Hanink, 2013; Harris, 1993). It is clear today that many sectors of the public are distrustful of heavy-handed policing tactics. Riots in many cities in the country have followed incidents of perceived or actual police misbehavior. In NYC, the growing dissatisfaction with the police in minority communities and especially among minority youth was a key factor in fueling the legal challenges to the NYPD's SQF policies.

Some scholars have argued that a focus on procedurally just police interventions will increase the effectiveness of policing more generally (Meares, 2015; Tyler and Fagan, 2008). We agree with Lum and Nagin (2015) that irrespective of whether procedurally just policing improves crime prevention, it needs to be a key focus of policing. In this context, it is reasonable to ask whether using SQFs as a general policy to reduce crime in urban contexts makes sense. In 2011, almost 700,000 citizens were stopped in NYC, and most of these were minority and young individuals. It is impossible not to be concerned with how such a widespread application of an intrusive policing tactic will affect a generation of young minority citizens in the future.

Moreover, evidence suggests that crime prevention can be achieved without resorting to an unrestricted SQF policy. A generation of studies has now shown that when police focus efforts on high-crime places or people, they can achieve crime-prevention gains (Braga and Weisburd, 2015; Kennedy, 1996; Sherman, 2007). In particular, consistent experimental evidence shows that police focusing on hot spots of crime will yield crime reductions (Braga et al., 2014; Sherman and Weisburd, 1995). Braga and Weisburd (2010) suggested that hot spots policing should be carried out with attention to procedural justice and concerns with police legitimacy. This, in our view, would be a more productive way to implement hot spots approaches than a simple use of SQFs applied broadly to hot spot areas.

At the same time, as Bratton (2014) has remarked, SQFs are an important tool for police. In specific circumstances, SQFs are not only warranted and legal, but also they represent effective police practices. Lum and Nagin (2015) have remarked:

> In making these points about the ambiguities in the evidence of the effectiveness of broken windows policing and the stop, question, and frisk tactic, we are not suggesting that aggressive policing tactics involving stopping and questioning citizens and, when appropriate, arresting them have no place in policing. To the contrary, our point is that aggressive policing of this type should target serious crime problems and high-risk repeat offenders rather than employed carte blanche. A case in point is police tactics to reduce firearms violence (for a review, see Koper and Mayo-Wilson, 2006, 2012). In these gun studies, various

> aggressive enforcement approaches were used, from traffic and pedestrian stops to car checks. Unlike zero tolerance approaches that use arrest for minor offenses indiscriminately, these tactics were specifically tailored to mitigate opportunities for firearms carrying in crime hotspots and were found to have positive effects (see e.g., McGarrell, Chermak, Weiss, and Wilson, 2001; Sherman, Shaw, and Rogan, 1995). Most recently, Rosenfeld, Deckard and Blackburn's (2014) study of police efforts to reduce gun crime in St. Louis finds similar effects.

We agree with Lum and Nagin (2015) and suggest that specific types of crime hot spots make appropriate sites for the application of SQF policies. Although it may be inappropriate to stop citizens on most streets in the city, it seems reasonable to stop every citizen to check for gun possessions if there have been repeated shootings on a street segment. Indeed, stops are effective at reducing such problems at crime hot spots (Braga, Pierce, McDevitt, Bond, and Cronin, 2008; Sherman, Shaw, and Rogan, 1995). Moreover, such stops can be made in ways that explain to the public why the police are making stops and in ways that do not stigmatize specific classes of people. Just as we allow special rules regarding policing methods at airports or near schools, methods such as police stops can be justified legally in particular places where security or safety demands it (Kahan and Meares, 1998; Weisburd, 2008). Of course, the task is not to allow police discretion to define the number of such places to a degree that intrusive police activities, such as SQFs, become common.

Although we think that our work provides an important advancement over prior studies in this area, we think it is important to note that it does not solve the problem of causality that we noted previously with certainty. Absent a randomized experimental evaluation of SQFs, there is simply no way to assure that the deterrence observed is not a result of confounding in our models. This is true of all efforts so far to assess the deterrent value of SQFs. We think that our approach of examining microgeographic units within short time periods provides in this context a believable effort to identify the effect of SQFs on crime. Nonetheless, here (as well as elsewhere), caution is warranted.

In turn, our analyses do not adjust for the potential of other policing strategies to impact crime. Weisburd et al. (2014) noted that SQFs increased while police numbers remained stable or declined in NYC. This would imply that the increase in SQFs came at the expense of other types of policing strategies during the period we study. For example, perhaps ordinary preventive patrol of hot spots has declined during this period, and accordingly, overall hot spots policing strategies have declined. Conversely, perhaps special unit activities have increased in crime hot spots despite the decline in number of police. We have no data on these deployment decisions in NYC. Nonetheless, we think it is reasonable that our approach measures the specific marginal benefit of SQF strategies. Of course, to the degree that SQFs are coupled with other policing strategies, we are gaining an incomplete picture of the contribution of SQFs to crime reduction. It is time for cities like NYC to collect such data routinely so that we can estimate such impacts.

Furthermore, although the NYPD is required to record SQFs (Jones-Brown et al., 2010), there have been several concerns and substantiated claims that they are underreported in NYC (New York Bar Association, 2007; Rudovsky and Rosenthal, 2013; Schneiderman, 2013). The completeness of the current SQF data is unknown (Rosenfeld and Fornango, 2014). Our data may strongly underestimate the occurrence of SQFs during the period we study. However, that is hard to imagine given the extraordinarily high levels of SQFs in this period. Nonetheless, it may be the case and this may be affecting our findings in some way. Absent knowledge about this bias, we cannot predict how it would have impacted our results.

Conclusions

Given the fact that SQFs were applied as a hot spots policing strategy during the period of time that we study, as well as the strong theoretical grounding and empirical evidence for hot spots policing approaches, our findings simply follow what we already know about focused policing. We think it is time for scholars to recognize that SQFs focused on microgeographic hot spots are likely to reduce crime. The question is whether this approach is the best one for crime prevention at hot spots and whether its benefits are greater than its potential negative impacts on citizen evaluations of police legitimacy. Of course, this does not have to be an either–or approach. SQFs, as we have described, have a place in crime prevention that maximizes their deterrent value, while minimizing infringement on the rights of citizens. Finding that correct balance should be a normative argument and one that is informed by empirical evidence.

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