JUST ENOUGH POLICE PRESENCE: REDUCING CRIME AND DISORDERLY BEHAVIOR BY OPTIMIZING PATROL TIME IN CRIME HOT SPOTS*

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Using observational data collected as part of a one-year preventive patrol study in Minneapolis, this investigation employs survival models to test hypotheses about the effects of specific instances of police patrol presence at high-crime locations on the time until the next occurrence of criminal or disorderly behavior at these locations. The results show that patrol stops must reach a threshold dosage of about 10 minutes in order to generate significantly longer survival times without disorder — i.e., greater residual deterrence — than that generated by driving through a hot spot. The optimal length for patrol stops appears to be 11 to 15 minutes. After that point, continued police presence brings diminishing returns. The theoretical and policy implications of these results are discussed.

Deterring criminal conduct through legal threats is a fundamental aspect of crime control efforts. Deterrence theory and research suggest that certainty of punishment has greater deterrent value than severity of punishment (e.g., Andenaes 1974; Blumstein, Cohen, and Nagin 1978). As an obvious, visible indicator of sanction threat, police patrol occupies a central place in crime control strategies. Patrol presence is a visible threat that increases the

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public's objective and subjective certainties of punishment. Zimring and Hawkins (1973:171), for example, note that the credibility of legal threats is a matter of cues, such as visible police presence, and of how those cues are interpreted. Likewise, Cook (1980:223-24) states that visible police presence increases certainty of detection and apprehension, and that frequent police presence in an area can raise potential offenders' perceptions of risk in that area.

Nevertheless, many researchers and practitioners now question the value of preventive patrol, the traditional means by which police have sought to be visible and to optimize deterrence (e.g., Klockars 1983; Skolnick and Bayley 1986). This change is due primarily to the influence of the Kansas City Preventive Patrol Experiment (Kelling et al. 1974), which indicated that variations in levels of motor patrolling among three groups of five patrol beats did not affect crime or citizens' perceptions. Though the study was criticized on methodological and statistical grounds (Larson 1976; Sherman 1986), the results gained widespread acceptance.

In contrast, results from less rigorous studies conducted in two precincts of New York City (Wilson 1983), the New York City subway system (Chaiken 1978), and four patrol zones in Nashville (Schnelle et al. 1977) suggest that large increases in patrol within relatively small areas decrease certain types of crimes. Yet increases like those employed in these studies are impractical to implement over large areas; thus the utility of the results is limited.

Sherman and Weisburd (1990, 1995) have reopened the issue with a study of preventive patrol in Minneapolis, which raised patrol levels at a random sample of high-crime addresses and intersections. Targeting specific high-crime locations rather than patrol beats provided larger sample sizes and units with higher baseline rates of crime. This approach made it possible to overcome statistical problems plaguing the Kansas City study (Sherman 1986). Police presence at the experimental locations was increased by about 13 percent during the one-year experiment (these hot spots received about three times as much patrol presence as the control hot spots), and produced a modest decrease in crime calls at these locations (Sherman and Weisburd 1990). This location-oriented approach appears to be the most effective and most practical means for enhancing the deterrent effects of preventive patrol. At the same time, it is not entirely clear to what extent this approach and other saturation approaches displace crime or how long their effects can be maintained.

The Disorder Problem

Though patrol studies tend to focus on serious crime, another important patrol issue is the police response to disorderly behavior and conditions. Evidence from studies conducted in several cities demonstrates that minor crime and otherwise disorderly behavior (such as vagrancy, panhandling, vandalism, public drunkenness, drug use, verbal harassment, and prostitution) and physical signs of decay and disorder (such as broken windows, graffiti, and abandoned houses) increase people's fear of crime (e.g., Lewis and Salem 1986; Skogan 1990; Wilson 1968). On the basis of survey results in Chicago, Lewis and Maxfield (1980) argue that disorder has a greater influence on citizens' perceptions than do crime rates because citizens witness and experience disorder more often that serious crime. Disorderly behaviors in particular seem to engender fear (Lewis and Maxfield 1980:182; Skogan 1990:47). Referring to disorderly behaviors as "soft crime," Reiss observes, "It is the visibility, frequency, chronicity, and cumulative nature of soft crime that is consequential" (1985:8).

In addition to creating fear, Wilson and Kelling (1982) argue that disorder erodes control over a neighborhood, making it more vulnerable to criminal invasion. Their thesis is that signs of social and physical disorder make residents and workers fearful. As a result, they isolate themselves and avoid contact with others. Previously stable residents may leave the area. This situation causes informal social control to break down and raises the level of anonymity in a neighborhood. If signs of disorder go unchecked, they become cues for potential offenders, indicating a lack of control over the area. Disorderly behaviors and minor violations then are likely to increase, eventually escalating into more serious criminal behavior. Disorder also can have a negative impact on the economic vitality of an area — for instance, by driving away residents with greater financial resources and making the area unattractive to workers and shoppers (Skogan 1990).

Though some observers have criticized the hypothesized link between disorder and crime (e.g., Greene and Taylor 1988), Skogan's (1990) analysis of survey data from neighborhoods in several major cities demonstrates that disorder is strongly and significantly correlated with the perceived crime problems in an area even after controlling for the population's poverty, stability, and racial composition. Moreover, using robbery victimization data collected in 30 neighborhoods, Skogan found that economic and social factors have only indirect links to crime; they are mediated through disorder (1990:75).

This finding has prompted many police departments to place greater priority on disorder problems (Reiss 1985; Skolnick and Bayley 1986). Evidence from community-oriented policing projects in places such as Houston (Pate et al. 1986), Newark (Pate et al. 1986; Police Foundation 1981), Oakland (Reiss 1985), Flint, Michigan (Trojanowicz n.d.), and the Georgetown section of Washington, DC (Sherman 1990) suggests that reducing disorder can have beneficial effects on crime and on fear (also see Skogan 1990). Likewise, Sampson and Cohen's (1988) analysis of robbery rates in 156 American cities indicates that robbery is reduced by aggressive policing of disorder, as measured by arrests for disorderly conduct and driving under the influence. Explaining this phenomenon, the authors state that "the mechanism hypothesized to account for the results is the impact of police activities in changing the perceptions of potential offenders by controlling incivilities and disorder" (1988:185).

The Geographic Distribution of Crime and Disorder: Hot Spots

A third line of research with implications for patrol effectiveness and reduction of disorder concerns the geographic distribution of crime. According to routine activities theory (Cohen and Felson 1979) crime does not occur randomly in time and space, but is produced by the convergence in time and space of motivated offenders, suitable targets, and the absence of capable guardians.[1] This convergence in turn is affected by factors such as daily activity and traffic patterns, community organization, and the development of various forms of technology.

Using routine activities theory to develop a "criminology of places," Sherman, Gartin, and Buerger (1989) examined calls for service in Minneapolis for one year and discovered that 3.3 percent of the city's address and intersections generated 50.4 percent of all calls for which police cars were dispatched (1989:37). Sherman et al. refer to these locations as "hot spots." The concentration of calls in hot spots was significantly greater than would be expected by chance. Hot spots of predatory crime, (robbery, criminal sexual assault, and auto theft) produced large numbers of both predatory crime calls and total calls. In addition, hot spots were often clustered within one-half block to two blocks of one another. These findings provided the basis for the Minneapolis Preventive Patrol Study discussed above. The concentration of calls for service in a very small percentage of a city's addresses and intersections has

[1] Routine activities theory was originally applied only to exploitative offenses such as robbery and auto theft, but Felson (1987) has since expanded the theory to cover mutualistic offenses (such as prostitution), competitive offenses (such as fights), and individualistic offenses (such as individual drug use).

also been found in Kansas City (Sherman 1992) and Boston (Pierce, Spaar, and Briggs 1988). Weisburd and Green (1994) report similar clustering of city drug markets.

If disorder contributes to crime, curbing disorder at these crime-prone locations may be a means of reducing the amount of crime they produce. Currently no longitudinal data are available to conduct a proper test of whether Wilson and Kelling's (1982) thesis applies to the degeneration of specific clusters of addresses and intersections. In a subsequent analysis of the data on Minneapolis hot spots, Weisburd, Maher, and Sherman (1992) reported that cross-sectional correlations between different types of crimes at hot spots tend to be small. This finding raises questions as to whether general causes such as disorder inevitably lead to more serious crimes at hot spots. For instance, "morals" calls (primarily prostitution) were related only weakly or insignificantly to most of the other calls these authors examined (1992:55). On the other hand, calls about drunks, a more common form of disorder around Minneapolis hot spots (see Koper 1992a:92; Weisburd et al. 1992:58), were related significantly to most of the other calls including serious offenses such as robberies, assaults, and burglaries of businesses. Moreover, Weisburd et al. found little evidence of crime or disorder specialization at hot spots (1992:58-59). Overall the Minneapolis hot spots produced equal proportions (about 27%) of the city's calls for hard crime and soft crime (1992:52). Hot spots seem to be locations that foster both disorderly behavior and more serious crimes, though the array of such behaviors occurring regularly in particular hot spots may depend on the characteristics of those individual hot spots.

The leading hot spot for predatory crime in Minneapolis was an intersection containing bars, a liquor store, and a park (Sherman et al. 1989:45). The second-ranking predatory hot spot was a bus depot. Such locations are nodes of activity that attract many people for various business, leisure, and travel activities, and they present many opportunities for criminal, rowdy, and otherwise troublesome behavior. Often they may be host to persons congregating and drinking, particularly at certain times of the day. Criminal offenders may be attracted to these spots by the numbers of potential victims passing through the area and by the anonymity of such places. Homeless persons may gravitate to these areas because of the opportunities for panhandling; in the process, they become both sources of disorder and potential victims. Indeed, surveys show that people are troubled by disorder in public areas such as parks and shopping centers; yet one can argue that informal methods of

social control are not as effective as formal ones in such places (Skogan 1990).

Police tactics have the potential for reducing disorder at hot spots. For example, increasing the frequency and duration of patrol presence at hot spots can raise the level of guardianship at such places, thereby increasing the probability that disorderly behavior will be prevented or controlled. Because public-area hot spots often possess features that facilitate disorder, reducing disorder at these locations may not cause substantial displacement of disorder problems to other places or times of day (Cornish and Clarke 1987). Even if disorder is displaced, it may be displaced to areas less conducive to more serious crime (Sherman and Weisburd 1988) or to times of day when there are fewer potential victims or persons to be troubled.

DATA

Before we discuss the specific hypotheses explored in this study, it will be helpful to describe the data used for the analysis. The data come from the Minneapolis Preventive Patrol Experiment (Sherman and Weisburd 1988, 1990, 1995), which examined the effects of preventive patrol at 100 hot spots from December 1, 1988 to November 30, 1989. For operational purposes, a hot spot was defined as a cluster of addresses which together produced 20 or more hard crime calls (e.g. robbery, rape, burglary) and 20 or more soft crime calls (e.g., disturbances, prostitution) over a one-year period (the selection year dated from June 1987 to June 1988) and showed a stable number of calls over a two-year period. The hot spots had to be locations where crime occurred in public (i.e., public places or places with spillover of activity into streets or parking lots) so that police presence could reasonably be expected to have a deterrent effect. The boundaries of the hot spots were defined so that each hot spot was no longer than one standard linear street block, no hot spot was within one block of another hot spot, and each hot spot could be viewed entirely from an epicenter. (See Sherman and Weisburd 1988, 1995 for more detail on the research design and on characteristics of the hot spots.)

The hot spots were assigned randomly to the treatment or control group. Officers patrolling the experimental locations spent their uncommitted patrol time at these locations, thereby providing "intensified but intermittent patrol" (Sherman and Weisburd 1988:25). Activities conducted at the hot spots, if any, were left to the discretion of the officers and their supervisors.

Throughout the course of the project, trained observers were sent to the 100 most active hot spots by random assignment between 6:30 p.m. and 2:30 a.m. on Tuesday through Saturday nights (to coincide largely with the periods generating the most calls for service), where they conducted 70-minute observations of police presence and street disorder.[2] The observations were performed during 13 periods, each lasting 28 days. In this way, observations at each location were spread evenly across the year.

Twenty-nine observers participated in the data collection.[3] Project staff members recruited these people through newspaper ads and gave them three days of training. To enhance the validity and reliability of the observations, training exercises required participants to code material from videotapes and written scenarios. Further, supervisors made unannounced field visits to verify that observers were conducting observations when assigned to do so and to double-code police presences and disorders.[4]

This investigation uses data from 6,273 observations conducted between December 1, 1988 and November 30, 1989 (see Appendix A).[5] The sample includes about 63 observations per hot spot for an average of 74 observation hours at each hot spot for the whole year. The observers recorded the beginning and ending times of police presences and of disorderly *behaviors* that they witnessed at the hot spots.[6] Observers recorded both events within the hot spots and events outside the boundaries of the hot spots but within visual distance.

When recording police presences, the observers counted *presences*, not numbers, of officers. For example, one police squad car represented one police presence, regardless of how many officers were in the car. In addition, the observers identified presences as stops or as drive-bys. Off-duty police working as security guards

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[2] This was done to verify officers' reports of the time they spent at the hot spots and to examine differences in patrol dosage and street disorder at experimental and control hot spots.

[3] Observers worked both full-time and part-time. There was very little turnover, but the staff was expanded during the course of the project.

[4] Systematic tests (i.e., reliability scales) were not conducted, however, on the reliability of the observation instruments.

[5] The observers made a total of 6,465 observations, but 192 (3%) of these observations are excluded from the study because of missing or conflicting data on the beginning and ending times of police presences, disorders, and/or observations. These observations contained 5 percent of the total observed disorders and 4 percent of the observed police presences. Examination of these observations across months revealed that the proportions of excluded observations in December 1988 and January 1989 (the first two months of the project) were slightly higher than in the other months. Examination of these cases by time of day and experimental/control group status revealed no marked differences.

[6] Copies of the observer codebook, developed by Anne Beatty, Joanne Oreskovich, and Michael Buerger, can be obtained from the Crime Control Institute or the author.

were recorded as police presences if they were working in uniform. The data document 24,813 police presences, the great majority of which were drive-bys (21,733). The average length of the stops was 14 minutes.

The observers also recorded several different criminal and otherwise disorderly behaviors: solicitation, drug transactions, physical assaults, auto or building break-ins, vandalism, verbal disorders (e.g., loud shouting or verbal harassment of passersby), loud disputes, drunk or drugged behavior, loud noise or music, the presence of bag persons, and persons down (as if drunk, ill, or injured). An "other" category was created to record other types of disorderly conduct (such as urinating in public) or more serious crimes (such as robbery or rape) which were not included on the observation sheets. The data capture 4,014 observed disorders. These included 418 criminal events (solicitation, drug transactions, physical assaults, auto or building break-ins, and vandalism) and 3,220 noncriminal events. The remaining 376 disorders were either unidentified or classified as "other." Most of these disorders were relatively brief, lasting an average of four minutes.

Defining disorder involves a certain degree of subjectivity on the part of researchers. Coding particular instances of disorder compounds this problem by introducing an unknown amount of observer bias. In addition, no information is available to indicate how users of the hot spots perceived these behaviors. Nonetheless, the behaviors recorded by the observers are consistent with those cited as problematic in the disorder literature. Skogan (1990), for example, discusses the concern expressed by survey respondents in 40 city neighborhoods about problems such as public drinking, verbal and physical street harassment, drug use and sales, vandalism, and noisy neighbors. Moreover, Skogan's discovery that urban residents with different background characteristics tend to agree on definitions of disorder (1990:54-57) adds credibility to efforts to define, observe, and record such behavior. Finally, the training and reliability checks discussed above were conducted to increase consistency and minimize bias in identifying disorderly behaviors.

The data have a number of strengths for deterrence research. First, they capture many disorderly events not included in official data and do not suffer from reporting effects and potential tampering problems, as do official data. Furthermore, the data present observed levels of crime and disorder rather than perceptions of crime and disorder levels. Police *visibility* also is measured directly rather than being inferred from other information. More important, the data allow for the study of threat communication and the reactions of criminal and disorderly persons to environmental cues

(i.e., visible police presence), a line of deterrence research advocated by Cook (1980:260). In a sense, they also provide a target-specific study of criminal opportunity (Cook 1980:243). Police presence serves as an attribute characterizing opportunities for crime and disorder at the hot spots.

POLICE PRESENCE, DISORDER, AND DETERRENCE: HYPOTHESES AND CONCEPTUAL FRAMEWORK

This study evaluates the effects of patrol presence, not any particular style of policing or set of police activities. As Reiss argues,

> Perhaps one of the ways that the police can more effectively control soft-crime is by increasing their presence in situations where they wish to control the incidence of soft-crime or its consequences. By being present, they either increase the risks of potential offenders to the point that offending is thwarted or their presence increases the probability that those being observed can be arrested for some infraction of the law (1985:29-30).

This should be especially true at hot spots. Whether in a squad car or on foot, officers are more visible in the small geographic area of a hot spot.

The presence of an officer, however, may not deter social disorder if it does not signal a change in the objective probability of apprehension (Zimring and Hawkins 1973). In other words, disorderly persons may not feel threatened if police tend to ignore many disorderly behaviors. This issue is especially salient to non-criminal disorderly behaviors and behaviors prohibited only by seldom-enforced municipal ordinances. The deterrability of disorderly behaviors is also uncertain; some of these acts involve conflicting standards of behavior and/or occur when people are highly emotional or under the influence of drugs (Andenaes 1974; Zimring and Hawkins 1973).

Nonetheless, when such conduct occurs in the presence of officers, it can provoke unwanted encounters with police. Officers may choose to enforce applicable ordinances or simply question disorderly persons. Such contacts may be perceived as punitive (Gibbs 1975) or, for those involved in deviant lifestyles, risky. Indeed, there is evidence that field interrogations decrease certain types of crimes (Boydstun 1975). In the context of this research, visual proximity to police may increase subjective assessments of the probability of enforcement or field interrogations, thus communicating some level of threat and deterring disorderly conduct. Further, Cook's (1980) notion of limited rationality is that people adopt rule-of-thumb principles which guide their decision making even when they are very emotional or inebriated. If this is true, such

principles should moderate the behavior of some (but certainly not all) people, especially when police are present and there is more opportunity to incur legal trouble. As Zimring and Hawkins (1973:140) point out, legal threats deter many people most of the time, even under conditions unfavorable to deterrence.

Earlier analyses conducted with the data used here also support the claim that patrol can influence disorderly behavior. Increases in patrol at the experimental locations resulted in 50 percent less observed disorder at these hot spots than at the control hot spots (Sherman and Weisburd 1990), and disorder was less likely to commence in the presence of officers (Koper 1992a).

This study further explores the process by which patrol affects behavior, examining the preventive effects of recent visual cues of enforcement (police presences) on disorder at hot spots. We investigate this issue using Sherman's (1990) concept of residual deterrence associated with police crackdowns. Sherman (1990:7) identifies a crackdown as an increase in the certainty or severity of official police reactions to specific types of offenses or to all offenses in a specific area. This is achieved by increasing police presence (in certain places or situations), sanctions, and/or threats in the media. Residual deterrence refers to effects that continue after a crackdown has ended. According to Sherman, crackdowns create residual deterrence by increasing uncertainty about risk. This causes potential offenders to overestimate risk levels. Even after a crackdown has ended, perceptions of heightened risk may take time to decay. Consequently the withdrawal of a crackdown should not cause an immediate return to pre-crackdown offending levels; this notion is supported by Sherman's review of police crackdowns.

The increase of intermittent, unpredictable police presence in the Minneapolis experimental hot spots also approximated a crackdown-backoff pattern (Sherman 1990). Previous analyses by the author (Koper 1992b) illustrate residual deterrence from the experiment in a very direct manner: they use a minute-based data file containing a record for each minute of observation at every hot spot. In minutes when police were not visibly present at the hot spots (N=379,622), the proportion of minutes during which disorder was occurring was 25 percent less in the experimental areas than in the control areas. For criminal disorders, this difference was 65 percent. Both differences were statistically significant, demonstrating that higher patrol levels in an area enhance deterrence not only during the extra time when police are visible but also when police are not present. This concept has not been tested explicitly by other patrol studies.

Extrapolating from Sherman's work, this study treats specific instances of observed police presence at hot spots as analogous to area crackdowns in order to test whether stronger dosages of police presence (as measured by the length of the presences) create residual deterrent effects, and, if they do so, whether there is an optimal length for police presences at hot spots. Testing these issues entails analysis of the time between police departure from a hot spot and the next disorder. If specific instances of police presence create residual effects, one would expect longer presences to produce longer follow-up periods without disorder. Longer presences may heighten uncertainty about police intentions and actions, and may increase perceptions of risk among people at the hot spot. This alone may drive potential troublemakers from the area, creating a more lasting residual effect. Of course, police also may move troublemakers out of the area by direct contact. Associates of these persons also may learn by word of mouth about police presence or actions at the hot spots (Zimring and Hawkins 1973). If potentially troublesome persons are not driven away, longer presences still may make them more cautious and less disorderly for some time. A parallel exists in psychological research, showing that longer exposure to films of fatal car accidents strengthens favorable attitudes toward safe driving practices, though this effect disappears over time (Leventhal and Niles 1965).

On the other hand, longer police presences may not drive criminal/disorderly persons from the area or make them more cautious. Disorder simply may begin or resume as soon as police leave. Further, the occurrence of disorder could be a function of the flow of people through the area. Soon after police leave a hot spot, the next potential troublemaker(s) may enter the area without any knowledge of recent police presence.

Yet if we assume that longer police presences lengthen residual deterrence, is there a point at which longer presences cease to provide additional benefits? In other words, is there an optimal length of time for police to remain at hot spots? Sherman (1990) theorizes a maximum utility point for crackdown length, beyond which no additional gains in residual deterrence are made. Such a finding here would imply an optimum length of presence for increasing perceptions of risk and driving troublemakers away from a hot spot. Once a presence has passed that length, a longer stay brings little additional benefit. To date, however, we have no empirical basis for estimating where that threshold may lie, if it exists.

Finally, this discussion does not explicitly consider the benefits of actions that police might take during longer stops, such as talking with citizens about problems, conducting foot patrols, or confronting suspicious and troublesome persons. Because the hot spots experiment did not implement specific styles of police activity, this should not pose a major problem. The longer the time police spend at a location, however, the more likely it seems that they will engage in these types of activities, whether the stop is proactive or reactive. In addition to any direct results, the perceptual effects of such actions may heighten uncertainty and enhance deterrence. On the other hand, troublesome persons may not feel threatened if officers appear to be preoccupied with other matters, as when they are responding to a call. The data do not permit clear inferences in this regard. This study treats police activities and their potential benefits as properties inherent in longer police stops.

ANALYSIS

I employ continuous-time, parametric event history models (Allison 1984; Kalbfleisch and Prentice 1980; Lawless 1982) to determine whether longer patrol presences create residual deterrence in the form of longer survival times without disorder. The investigation uses a police instance-based file (N=19,498) whose cases correspond to blocks of time during which at least one police officer was present (see Appendix A). To provide a basic contrast between the conditions of police presence and no police presence, police stops and drive-bys that overlapped and/or resulted in continuous police presence at a hot spot were combined into a single observation of police presence.[7] This file pools observations from all hot spots.

The event history models use a subsample of the total police blocks, hereafter called presences. (The selection process is discussed in greater detail in Koper 1992a.) Presences were excluded if they were not observed in their entirety, or if at least one minute of follow-up time did not elapse after they ended; that is, the presences had to begin after the observers arrived and had to end before the observers left. In addition, the analysis was restricted to presences 20 minutes or less in duration so that potential follow-up periods would be comparable among drive-bys and stops of different

[7] For instance, if a squad car was present at a hot spot from 8:00 to 8:06 and if a second squad car stopped at the same hot spot from 8:04 to 8:10, the data would show a police stop from 8:00 to 8:10.

lengths.[8] To provide a clearer test of the preventive effects of patrol, I also excluded presences if they were concurrent with observed disorder. I did this to weed out cases in which police took no action to restore an orderly environment, as well as instances in which officers were responding to problems that either prevented them from restoring order or somehow contributed to the continuation of disorder after they departed. In such cases, we would not expect to find any residual deterrence.[9]

This procedure left a final sample of 16,997 presences (87% of the original police-instance sample), 16,050 of which are drive-bys. The remaining 947 are stops lasting 20 minutes or less. The average length of these stops is about seven minutes (see Appendix A). The follow-up period for each presence begins the minute after the presence ends. If a disorder was observed before the end of the observation and before the next observed police presence, the case has a disorder event (i.e., a failure) at the time of the disorder. If no disorder failure occurred, the observation is censored at the time of the next police presence or at the end of the observation, whichever comes first. When an observation is censored, the event history models take into account the fact that the case survived at least to the time of censoring.

Different event history models make different assumptions about the unobserved rate at which events occur (Allison 1984), also called the hazard rate. Allison (1984:23) defines the hazard rate as the probability of failure in the interval from time T to time $T + S$, given that a case has survived to time T. (In this case, it is the probability of failure in the interval from minute T to minute $T + 1$,

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[8] As a presence grows longer, its potential follow-up time before the end of the observation period decreases. Preliminary analysis revealed that presences longer than 20 minutes had noticeably shorter follow-up periods (whether or not a disorder occurred) before the end of the observation period or before a new police presence. For example, the percentages of cases with 30 minutes of potential follow-up was 11 percent to 14 percent for drive-bys and stops up to 20 minutes, but only 6.8 percent for 21- to 30-minute stops and 0 percent for stops longer than 30 minutes. This systematic discrepancy in the data may have biased the survival estimates for stops longer than 20 minutes.

An informal analysis of 20- to 30-minute stops examined the percentage of cases registering disorder failures within a designated time, x, given that they had x minutes of potential follow-up time (Koper 1992a). These presences did well within a 10-minute follow-up period, but the analysis had very small Ns, and the overall results were inconclusive.

[9] I did this by removing cases in which disorder occurred during any minute of the presence. Preliminary analysis revealed that these cases had a high failure rate; most failed immediately. Unfortunately, when these cases were removed, the study lost a substantial percentage of failures. Therefore I also estimated the models with these cases, using a dummy variable to indicate whether disorder occurred during the presence. This variable had a very strong and highly significant negative effect on survival time in both models. Otherwise the results followed the same pattern as those presented in the text. If anything, the results of these runs suggested that the findings presented here may be conservative estimates of the effects of some police stops, relative to effects of drive-bys.

given survival to minute T.) Preliminary analysis with life tables (Lee 1980) suggested that a log-normal model is appropriate for the data. The log-normal model assumes that the baseline hazard rate rises and then falls over time. Life table estimates showed that the overall sample has a hazard rate which rises, peaks at about five minutes after a presence, and follows a downward trend thereafter.

On the basis of these preliminary analyses, I also decided to limit the follow-up period for each police presence. Relatively few cases have follow-up observation time in the 30-to 70-minute range without censoring or disorder; this is especially true for stops longer than 10 minutes. Consequently the hazard becomes erratic in this range, fluctuating substantially in response to single events. Because of this problem with sample size and an absence of theory regarding the dissipation, or decay, of residual deterrence after a police presence, the models presented below use a maximum 30-minute follow-up period for each presence.[10] Cases not experiencing an event or censoring before 30 minutes are thus censored at 30 minutes. Overall 1,210 cases, or 7 percent of the sample, show disorder failures within 30 minutes (see Appendix B).

The log-normal model is an accelerated failure-time event-history model that is expressed in terms of the natural logarithms of the survival times:

$$logT = a + b_1x_1 + b_2x_2 + \ldots + b_nx_n + cu, \qquad (1)$$

where T is the survival time, x_1 through x_n are covariates, and cu is a normally distributed error term (Allison 1984; Kalbfleisch and Prentice 1980). When censored data are used, the model estimates the parameters using maximum-likelihood procedures rather than ordinary least squares.

Model 1 shows the effect of drive-by presences relative to all stops and displays the effect of increasing duration (in minutes) for police presences; duration is set to 0 for drive-by presences. (See Table 1.) In relation to all stops, drive-by presences have significantly longer log survival times. This is probably the case because, even after removal of presences overlapping with disorder, many of the stops are responses to problems and occur during times that have higher risk for disorder. The duration variable, however, shows that each extra minute of police presence has a significant and positive effect as well.

The coefficients of Model 1 show the additive effects of the covariates on the log of survival time. When the coefficients are exponentiated, the impact of the covariates on survival time itself is

[10] This resulted in the loss of very few failures from the final sample and did not change the inferences drawn from the analysis.

Table 1. Log-Normal Survival Models (N=16,997)

Models	B	Std Err	Chi-Square	Grp N
Model 1				
Ln L = –5244.5				
Drive-By	.811	.342	5.622*	
Duration	.204	.102	4.005*	
Duration^2	–.007	.006	1.714	
Constant	5.021			
Scale	2.11			
Model 2				
Ln L = –5240.7				
Stop 1-5 min	–.343	.173	3.947*	481
Stop 6-10 min	.286	.268	1.135	279
Stop 11-15 min	1.584	.619	6.548**	121
Stop 16-20 min	.058	.486	.014	66
Constant	5.490			
Scale	2.109			

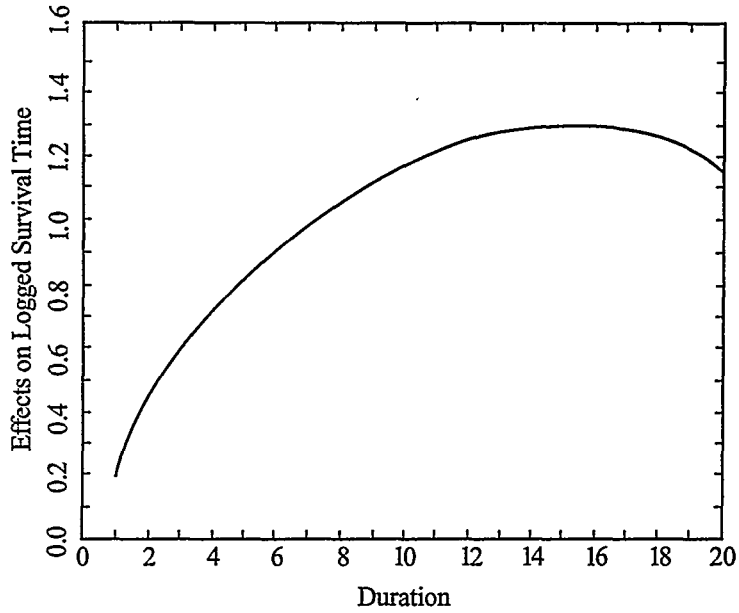
NOTE: The constant (a in Eq. (1)) represents the –log of the hazard rate. The scale term (c in Eq. (1)) represents p^{-1} where p is a parameter affecting the scaling of the distribution of $logT$ and hence the shape of the hazard rate. See Kalbfleisch and Prentice (1980).

* $p < .05$; ** $p < .01$

revealed. These effects are multiplicative rather than additive. Each extra minute of duration multiplies the mean survival time by $e^{.204} = 1.23$. In other words, each extra minute of police presence increases survival time by 23 percent. (This percentage change is calculated by subtracting 1 from the multiplicative effect and multiplying the difference by 100.)

The duration-squared variable tests for nonlinear effects (i.e., a plateau effect). This variable is not statistically significant, but its negative sign suggests that within a 20-minute patrol dosage range, the benefits of duration peak before patrol stops reach 20 minutes in length. An estimate of the plateau point is provided by taking the coefficient for duration and dividing it by –2 times the coefficient for duration squared. This process places the plateau point somewhere between 14 and 15 minutes. (See Figure 1.) Duration increases the log of survival time until duration reaches about 14 minutes. (The total effects on the log of survival time presented on the y-axis of Figure 1 are calculated by multiplying each duration value by .204 and adding this product to the product of duration squared and –.007.) Stops appear to be most effective when they are 13 to 15 minutes long. After 15 minutes, the benefits of increasing duration decline, though we must be cautious about interpreting this effect because it is not statistically significant.

Figure 1. Duration Response Curve



The positive effect of the drive-by category and the possible existence of a maximum utility point for presence length raise two additional questions. First, if drive-by presences are treated as a minimum, baseline dosage of police patrol, is there a threshold point that stops must pass before they create greater residual deterrence than drive-by presences? In other words, at what point (if any) does it become worthwhile for officers to stop at hot spots, as opposed to merely driving through? Second, if there is a point at which stops become superior to drive-bys, is there also a point at which stops cease to be more effective than drive-bys (i.e., a point of diminishing returns)? Drive-bys create a .811 increase in the log of survival time; Figure 1 shows that stops produce a .8 increase in the log of survival time once they reach about five minutes in duration. After stops pass that point, their effects remain greater than .8, but this does not tell us whether any of these stops generate significantly more residual deterrence than do drive-bys.

Model 2 (see Table 1) attempts to address these questions more explicitly. In Model 2, presences are broken down into five categories: drive-bys, 1- to 5-minute stops, 6- to 10-minute stops, 11- to 15-minute stops, and 16- to 20-minute stops. The model omits drive-bys, thereby using them as the reference category. In relation to drive-by presences, 1- to 5-minute stops have significantly worse survival times, but 11- to 15-minute presences have significantly better survival times. The latter stops increase survival time by

388 percent in relation to drive-bys. The other categories have positive but insignificant effects. Stops begin to show residual effects superior to those of drive-bys after stops reach about five minutes in length. If a stop is longer than 10 minutes, it creates a residual effect, which is a statistically significant improvement over the residual deterrence generated by driving through a hot spot. Model 2 identifies a maximum utility point for duration somewhere in the 11- to 15-minute range. For stops longer than 15 minutes, residual effects decrease and are not significantly better than those generated by drive-bys. However, the small sample size in the 16- to 20-minute category should make us cautious about drawing strong inferences for that group (see last column).[11]

To illustrate more clearly what some of these coefficients mean in terms of real survival times, I computed log-normal survival estimates for follow-up periods of up to 30 minutes for drive-bys and 11- to 15-minute stops. These estimates form survival curves, which are presented in Figure 2. These curves show the probability of surviving without disorder for various follow-up times up to 30 minutes. Table 2 also shows the probability of survival to selected time points for drive-bys and 11- to 15-minute stops. For instance, the respective probabilities of 30 minutes of order following a drive-by and an 11- to 15-minute stop are .84 and .96. Conversely, the probability of disorder occurring within 30 minutes after a drive-by is .16. For 11- to 15-minute stops, this figure is .04, a 75 percent reduction in the probability of disorder in relation to the probability for drive-bys. These figures, however, may underestimate the true difference in residual deterrence generated by the two categories. Both Table 2 and Figure 2 show that the difference in survival times between the groups increases over time. If the follow-up periods were longer, the residual benefits of 11- 15-minute stops would probably appear even greater.

[11] These results persisted in slightly more complicated versions of the models, which included variables for warm/cold months (with May through September coded as warm months), hour of day, and hour of day squared. The warm months had highly significant negative effects on survival time in each model; the hour-of-day variables were marginally significant in each run. Nevertheless, introduction of these variables produced almost no changes in the effects of the police variables. The slight changes that did occur tended to favor stops: drive-bys became only marginally significant (p=.06) in Model 1, and the negative effect of 1- to 5-minute stops in Model 3 became only marginally significant (p=.07).

Figure 2. Log-Normal Survival Curves for Drive-Bys and 11- to 15-Minute Stops

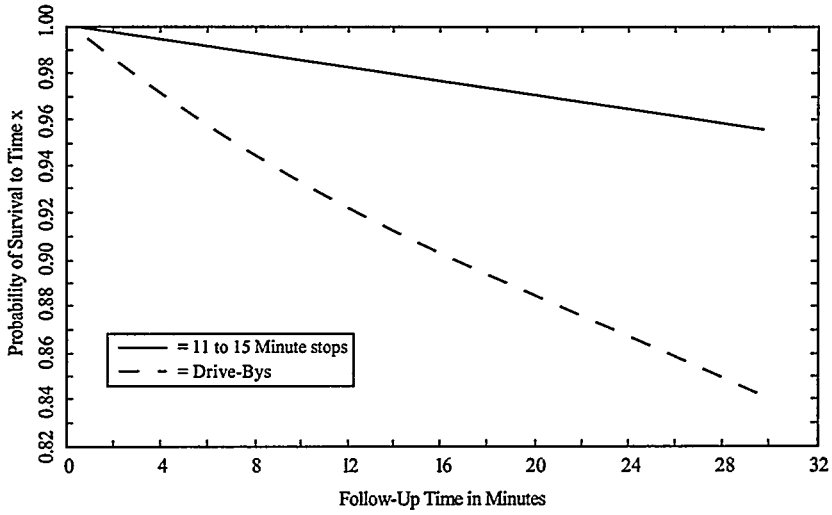


Table 2. Log-Normal Survival Estimates for Drive-Bys and 11- to 15-Minute Stops

Follow-Up Time In Minutes	Probability of Survival to Time T (Drive-Bys)	Probability of Survival to Time T (11-15-Min. Stops)
5	.967	.995
10	.935	.988
15	.906	.981
20	.881	.973
25	.859	.966
30	.839	.959

NOTE: These estimates equal 1 minus the value of the cumulative normal distribution evaluated at $(logT - z)/c$ where T is the chosen follow-up time, z represents the difference between drive-bys and 11- to 15-minute stops, and c is the scale term shown in Model 2 of Table 1 (Lawless 1982:24). In Model 2, z is 5.490 (the constant term) for drive-bys and (5.490 + 1.584) for 11- to 15-minute stops.

A limitation of these models is that they take an ahistorical view of each presence and its aftermath. That is, previous events (presences and disorders) both within each 70-minute observation window and over the course of the entire year could affect each presence and its associated survival time. Yet the randomly selected starting times and the relative shortness of the observations make it difficult to reconstruct even the most immediate history of the location. Moreover, there is no theoretical basis for deciding how far back in time such measures should extend. In view of the flow of activity and people into and out of hot spots (Sherman et al. 1989), very recent history (i.e., the last hour) may matter very little.

Besides, the last few minutes before a presence are likely to furnish an incomplete picture of prior history. The most important effects of different levels of police presence may result from patterns developing over several hours, days, months, or years. Capturing these effects would require longitudinal study of the "criminal careers" of the hot spots. Such efforts to model historical factors are beyond the scope of this investigation.[12]

A related caution is that data corresponding to the individual characteristics of the hot spots were not available for this study. Certainly some locations were "hotter" than others. This problem should be minimized, however, by the hot spot selection process, which ensured substantial homogeneity among locations.

To test the robustness of the results and the appropriateness of the log-normal distribution, I estimated the same models with exponential and Weibull parametric survival models and a Cox (1972) proportional hazards model. These models affirmed the earlier results and showed the log-normal distribution to be the best-fitting parametric distribution.[13]

Finally, I conducted survival analyses only for overall disorders. Though it would have been desirable to run the models for criminal events only, the extremely small number of cases with criminal failures (around 1 percent of the cases) would have made it very difficult to find any significant effects (Allison 1984:50).

DISCUSSION AND CONCLUSIONS

To enhance our knowledge of effective patrol tactics at high-crime locations, this study has investigated the residual deterrent effects associated with specific instances of police presence at such locations and has shown that stronger dosages, as measured by

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12 However, to test for any cumulative effects from the higher patrol levels in the experimental locations, I entered an experimental/control area dummy variable into the models. The variable was insignificant, and its inclusion had no substantial impact on the effects of the police variables.

13 The exponential model assumes a constant hazard rate, while the Weibull model assumes that the hazard rate either increases or decreases at a constant rate and does not change direction. The fits of the Weibull and the exponential distributions can be tested formally against one another by using a log-likelihood ratio test. This test showed that a Weibull distribution is inappropriate for the data. Though the exponential and log-normal models cannot be tested formally against one another, inspection of the log-likelihoods produced by each model is an informal means of assessing which model fits the data better (Schmidt and Witte 1988). The log-normal model produced log-likelihoods that were 50 to 55 points better (closer to 0) in each instance. At any rate, the exponential results were essentially the same as those from the log-normal models in terms of directions, statistical inferences, and magnitudes of effects.

Cox's (1972) proportional hazards model makes no assumptions about the distribution of the baseline hazard rate and essentially estimates the impact of the covariates on the hazard rate itself without presenting an estimate of the baseline hazard rate. The inferences from this model agreed with those from the log-normal and exponential models.

durations of the police presences, improve residual deterrence of criminal and disorderly behaviors. Yet the results reveal that stops must reach a threshold dosage of about 10 minutes in order to generate significantly more residual deterrence than is generated by simply driving through a hot spot. The survival models also show that within a 20-minute dosage range, the optimal length for police presences is about 14 to 15 minutes. After that point, the returns from continued presence diminish. (Limitations of the data, however, and the insignificance of the nonlinear effect in Model 1 should temper conclusions regarding this leveling-off point).

The findings suggest that longer presences, at least up to a point, increase uncertainty and raise perceptions of risk at hot spots. This outcome lengthens survival times, probably through a combination of driving away some troublesome persons and making others more cautious for some time afterward. The policy implication of this study is that police can maximize crime and disorder reduction at hot spots by making proactive, medium-length stops at these locations on a random, intermittent basis in a manner similar to Sherman's (1990) crackdown-backoff rotation strategy. In this way, police can maximize deterrence and perhaps minimize the amount of unnecessary time they spend at hot spots. In fact, the results imply that longer patrol stops at the experimental hot spots in the Minneapolis Preventive Patrol Experiment (Sherman and Weisburd 1995) may have been a primary mechanism by which crime and other disorderly behaviors were reduced in those places. This study reinforces Sherman and Weisburd's (1990, 1995) contention that preventive patrol, if focused properly, has a deterrent (or at least a displacement) effect on crime. It also provides further justification for proactively concentrating more resources on specific troublesome locations rather than on neighborhoods (see Pierce et al. 1988; Sherman et al. 1989; Taylor and Gottfredson 1986).

An important finding implicit in these results is that preventive patrol decreases noncriminal disorderly behaviors. Despite the difficulties inherent in deterring these behaviors (i.e., conflicts of values, emotional contexts, and lack of criminal penalties), longer presences exert a restraining effect. Although this study cannot demonstrate conclusively that reduction of noncriminal disorder reduces crime, it provides clear indications that these phenomena are intertwined. At least we can say that they respond to the same preventive measures (i.e., police presence in cars and/or the uniform). In view of the link between disorder and fear, intermittent patrol stops at hot spots have the potential to improve citizens' perceptions of these locations. Though disorderly behaviors were not frequent at the hot spots in this study, they were not uncommon.

Disorder may be much more common at hot spots in cities with greater crime problems and warmer climates than in Minneapolis. The findings suggest that longer police stops, if made regularly, could substantially reduce the amount of disorder witnessed by regular and occasional users of these areas. We can only speculate, however, on the possible beneficial effects of this practice on fear, informal social control, and the vitality of these locations.

Still, a number of questions remain concerning hot spot patrol interventions. This study has further solidified the basis for proactive stops at such locations, but it has not addressed what officers do while at hot spots. A logical next step (beside further investigating the maximum utility point for duration of stops) would be to experiment with different styles of policing. Departments might assign officers to conduct short walking tours of hot spots or to engage in problem-oriented policing at those places (Goldstein 1990; Sherman 1986). Also, although this study has shown that preventive patrol can prevent disorder, it does not address how officers should best handle disorder (particularly noncriminal disorder) that occurs when they are present.[14]

Another question concerns displacement. The data do not permit assessment of displacement effects. Nevertheless, hot spots seem to have social and physical characteristics that facilitate crime and disorder. As stated earlier, any displacement that occurs may displace disorder to areas or times less conducive to such conduct. Besides, departments could continuously monitor this displacement, if any. A department could shift targets if and when it discovered indications of crime and disorder displacement.

Further research is needed to determine whether these findings can be generalized to cities with warmer climates and greater problems of crime and disorder. In addition, future research may be able to show how often officers should stop at hot spots for maximum effect. The major implication of this study is that optimization of patrol time at a city's most troublesome locations can help to reduce crime and otherwise disturbing behavior at these places, thereby improving the effectiveness of preventive patrol.

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[14] Policing disorder raises a number of dilemmas such as defining disorder, dealing with decriminalized or noncriminal behavior, and controlling discriminatory treatment, unequal enforcement, and extralegal conduct by officers. I do not raise these issues here, however, because this research does not evaluate order-maintenance policing as such.

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Appendix A. Descriptive Statistics

Variable	Mean	Std Dev	Min	Max
Observations per Hot Spot	62.73	6.98	46	83
Observed Minutes per Hot Spot	4452.65	495.07	3,266	5,893
Disorders per Hot Spot	40.14	23.80	5	114
Police Presences per Hot Spot	194.98	99.01	39	528
Minutes per Observation	70.98	1.25	20	102
Disorders per Observation	0.64	1.60	0	35
Police Presences per Observation	3.11	2.45	0	15
Duration of Police Stops Used in Survival Analysis	6.55	4.69	1	20
Time to Disorder or Censoring for Police Presences Used in Survival Analysis	11.41	9.07	1	30
Potential Follow-Up Observation Time for Presences Used in Survival Analysis	12.13	9.32	1	30

Unless otherwise noted, statistics on police presences refer to the original file based on police instances (N=19,498).

Appendix B. Survival Data Summary

Time in Minutes	Number At Risk	Disorder	Censored
1	16,997	133	232
2	16,632	123	1,654
3	14,855	122	1,314
4	13,419	99	1,094
5	12,226	130	1,245
6	10,851	67	846
7	9,938	53	792
8	9,093	58	675
9	8,360	42	578
10	7,740	45	711
11	6,984	32	504
12	6,448	34	462
13	5,952	45	397
14	5,510	27	339
15	5,144	31	464
16	4,649	26	317
17	4,306	13	292
18	4,001	14	264
19	3,723	11	241
20	3,471	24	274
21	3,173	17	178
22	2,978	6	183
23	2,789	7	205
24	2,577	7	166
25	2,404	10	195
26	2,199	10	138
27	2,051	7	113
28	1,931	8	133
29	1,790	4	109
30	1,677	5	1,672