PROACTIVE POLICING AND ROBBERY RATES ACROSS U.S. CITIES*

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Abstract

In recent years, criminologists, as well as journalists, have devoted considerable attention to the potential deterrent effect of what is sometimes referred to as "proactive" policing. This style of policing entails the vigorous enforcement of laws against relatively minor offenses in order to prevent more serious crime. The current study examines the effect of proactive policing on robbery rates for a sample of large U.S. cities using an innovative measure developed by Sampson and Cohen (1988). We replicate their cross-sectional analyses using data from 2000-2003, a period during which proactive policing is likely to have become more common than the period of the original study—the early 1980s. We also extend their analyses by estimating a more comprehensive regression model that incorporates additional theoretically-relevant predictors. Finally, we advance previous research in this area by using panel data. The cross-sectional analyses replicate prior findings of a negative relationship between proactive policing and robbery rates. In addition, our dynamic models suggest that proactive policing is endogenous to changes in robbery rates. When this feedback between robbery and proactive policing is eliminated, we find further evidence that proactive policing reduces robbery rates.

Key words: proactive policing, violent crime, deterrence, endogeneity

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INTRODUCTION

During the past decade, crime rates have fallen dramatically in the U.S., according to both official police statistics and victimization data. This pronounced decline was largely unexpected, but as Levitt (2004: 163) observes, “there has been no shortage of hypotheses to explain the drop in crime after the fact.” One explanation that has received considerable media attention is the possible effect of proactive, “quality of life,” or “broken windows” policing, a style that reflects strict enforcement of all laws, addressing minor infractions as well as serious crimes (Sampson and Cohen, 1988; Sherman, 1995). Indeed, a Lexis-Nexus search of articles in ten leading newspapers between 1991-2001 identifies “innovative policing strategies” as the most frequently referenced explanation for falling crime rates, followed by “increased reliance on prisons” (Levitt 2004: 164). Despite the widespread and often favorable public attention devoted to proactive policing, research on the efficacy of such strategies is surprisingly sparse.

One of the more sophisticated attempts to assess the deterrent effects of proactive policing is a study by Sampson and Cohen (1988) published more than 20 years ago. Two features of this study are particularly noteworthy: their innovative measure of proactive policing, which focuses on law enforcement responses to disorderly conduct and driving under the influence, and their creative theorizing about the direct effects of proactive policing on offending rates in addition to any indirect effects via increased probability of apprehension. Sampson and Cohen find support for the deterrence perspective in their analysis of 171 large American cities circa 1980. They report a negative association between proactive policing and robbery rates in multivariate models, net of indicators of racial inequality, marital disruption, socioeconomic status, and racial composition. A study by MacDonald (2002), approximately a decade and a half later, reaffirms Sampson and Cohen’s finding about the deterrent effect of proactive policing.

Our study builds on this literature in several ways: First, employing a similar cross-sectional design, we replicate Sampson and Cohen to determine whether proactive policing is associated with lower robbery rates using data for 2000-03, a period during which proactive policing is likely to have diffused more widely across police agencies.[1] Consistent with their study, we examine large U.S. cities with appreciable black populations. Second, we significantly expand their model specification. In the years since their study was published, a set of covariates has emerged as potent predictors of violent crime rates (Land et al., 1990). We thus determine whether any effect of proactive policing on robbery rates withstands controls for a composite measure of disadvantage.

Finally, we use panel data to further assess the effect of proactive policing on robbery rates (Finkel, 1995; Voas et al., 2002). While cross-sectional models identify contemporaneous associations, they cannot account for temporal ordering. In addition, unless properly instrumented, cross-sectional regression models are predicated on the assumption that the variables on the right-hand side of the equation are exogenous. This assumption is problematic when examining the impact of proactive policing because police practices are likely to be responsive to changes in levels of crime. To anticipate the results, using a cross-lagged effects model (Finkel, 1995), our analyses indicate that proactive policing is endogenous to robbery rates, suggesting that reciprocal causal effects must be purged to accurately quantify the effect of proactive policing on robbery rates. We do so through an application of the Arellano-Bond dynamic panel data estimator (Arellano and Bond, 1991; Arellano, 2003).

[1] In additional analyses, Sampson and Cohen examine age-race-disaggregated arrest rates and find the effect of aggressive policing on robbery is largest for adult and black offenders. For our purposes, analyses are restricted to assessing the overall deterrent effect of proactive policing, which is determined with data on "offenses known to the police" rather than arrest data.

POLICE STRENGTH, ARREST CERTAINTY, AND DETERRENCE

A common perception is that the police play a major role in preventing crime. Indeed, during times of rising crime rates, a frequent response is to call for more officers on the streets. Consistent with the deterrence perspective, the assumption is that greater police presence will reduce crime rates as would-be offenders adjust their perceptions to the increased probability of arrest. Although a reduction in crime seems intuitively plausible, research on the deterrent effects of arrest certainty and police size has produced conflicting results. While some studies report a deterrent effect (Levitt, 1997; Marvell and Moody, 1996; Sampson and Cohen, 1988; Tittle and Rowe, 1974; Wilson and Boland, 1978), others report no relationship (Decker and Kohfeld, 1985; Weiss and Freels, 1996), and one study even finds a positive relationship between arrest certainty and crime (Jacob and Rich, 1980-81). It is thus not clear that more police equals less crime.

Critics of deterrence explain the conflicting results by arguing that police have little, if any, impact on crime because most police work is not devoted to crime reduction, and the most frequently employed police strategies are poor crime-prevention strategies (Marvell and Moody, 1996: 610).[2] A related argument is that changes in police practices may affect crime through causal linkages that do not increase arrest certainty, given that the police have minimal control over important factors such as the willingness of citizens to report a crime, offer information to the police, or identify suspects (Wilson and Boland, 1978: 369).

Methodological issues may also explain discrepant findings on the effects of proactive policing in previous research. First, scholars have attempted to capture arrest certainty with measures that reflect police presence, including size of the police force (Wilson and Boland,

[2] In their research based on Granger causality modeling to correct for simultaneity bias, Marvell and Moody (1996) find that police size has a significant negative impact on crime rates, consistent with the deterrence perspective.

1978), the arrest/offense ratio or percentage of crimes cleared (Decker and Kohfeld, 1985; Tittle and Rowe, 1974), and the number of moving violations/citations issued (Jacob and Rich, 1980-81; Wilson and Boland, 1978). But these measures have potentially serious drawbacks. With respect to clearance rates, Wilson and Boland (1978: 368) contend that "a crime 'cleared by arrest' is whatever the police say it is" and argue that such rates may vary substantially among police departments for reasons that have little to do with the objective probability of getting caught. Likewise, Jacob and Rich (1980-81: 113) conclude "the number of moving violations is not a good indicator of the type of police aggressiveness that might be related to the apprehension of robbers and the deterrence of robbery." Finally, others contend that arrests are a measure of police responsiveness to crime rather than a primary source of deterrence (Decker and Kohfeld, 1985). From this perspective, arrests occur in response to crime rather than prevent it.

Decker and Kohfeld (1985: 439) raise an additional methodological issue, charging that the negative relationship between the arrest/offense ratio and crime rates reported in studies may be spurious for several reasons. One has to do with the use of "offenses known" in the measurement of both variables. In the conventional measure of certainty of punishment, the numerator of the crime rate (offenses known) is identical to the denominator of the measure of punishment certainty, inducing a nonlinear inverse relationship between crime and the certainty of punishment measure. Decker and Kohfeld claim the negative correlation between arrest/offense ratios and crime rates may be an artifact of the construction of the rate measures, which they confirm in their study. Others have noted this concern (Jacob and Rich, 1980-81: 114).

An additional methodological issue pertains to the nature of the causal relationship between police actions or police strength, and crime rates. With respect to arrest certainty, scholars have noted that an inverse association could occur either because high arrest/offense ratios lead to lower crime rates or because higher crime rates cause lower arrest/offense ratios. The latter situation might emerge if increases in reported crime swamp police resources so that the rate at which criminals are arrested declines (Wilson and Boland, 1978: 369). With respect to measures of police force size, the problem of reciprocal causation also arises but with different implications. It is plausible that increasing crime rates will instigate the hiring of more police officers. Unless this positive reciprocal effect is controlled, estimates of the expected negative effect of police size on crime rates will be suppressed.

Research has attempted to address the endogeneity issue with varying identification strategies. Marvell and Moody (1996) use the Granger causality test on pooled city and state data over 20 years to determine the causal direction, if any, between police force size and index crime rates. They find causation in both directions, although the impact of crime on the number of police is slight while the impact of police size on most crimes is substantial. In his panel study of 59 U.S. cities from 1970-1992, Levitt (1997) also finds support for deterrence. He uses the timing of mayoral and gubernatorial elections as an instrumental variable in 2SLS analyses, arguing that it affects police force size but does not directly influence crime rates. The results show that increases in police size substantially reduce violent crime but have a smaller impact on property crime. Although these results and others (e.g., Wilson and Boland, 1978) support a deterrence perspective, questions about methodological procedures remain (see McCrary, 2002).

In sum, recent research has attempted to assess the relationship between policing and crime, focusing primarily on measures of arrest certainty and the size of the police force. While

some results support the deterrence perspective, conclusions are difficult to draw due to methodological limitations. An alternative approach is to shift attention away from the arrest/offense ratio and measures of police size to consider more directly how the *style* of policing might affect crime, given that activities of the police are likely more important for the reduction of crime than is police strength per se (MacDonald, 2002; Sherman, 1995).

PROACTIVE POLICING AND CRIME

The nature or style of policing is the central consideration in Sampson and Cohen's (1988) study. They build on the work of Wilson and Boland (1978) and Wilson and Kelling (1982) to examine whether proactive policing reduces crime. This policing approach, also referred to as "quality of life" or "broken windows" policing, relies on the professional model of policing (Wilson, 1968). It does not necessarily involve direct collaboration with the community and is therefore different from "community policing."[3] Instead, crime control occurs through strict enforcement of all laws (Sampson and Cohen, 1988; Sherman, 1995). A proactive strategy does not mean that the officer is antagonistic or hostile but rather that s/he maximizes the number of interventions in, and observations of, the community. The most common form of proactive enforcement involves the use of field interrogations, whereby police engage in directed patrol efforts to question suspicious persons and enforce traffic violations (MacDonald, 2002: 595; Sherman, 1995).

Sampson and Cohen (1988) suggest two mechanisms through which proactive policing might reduce crime. The first pertains to the indirect effect of proactive policing on crime through arrest risk. As Wilson and Boland (1978: 373) argue: "By stopping, questioning, and

[3] See Greene (2000) for similarities and differences between community and proactive policing, and Muhlhausen (2002), Sabol (2005), and Zhao et al. (2002) for assessments of the impact of community policing programs.

otherwise closely observing citizens, especially suspicious ones, the police are more likely to find fugitives, detect contraband (such as stolen property or concealed weapons), and apprehend persons fleeing from the scene of a crime." Thus, proactive policing is hypothesized to affect crime rates by changing the actual probability that an arrest is made—in other words, by increasing the arrest/offense ratio.

The second means through which proactive policing may affect crime rates directly is by influencing prospective offenders' perceptions of the probabilities of apprehension for criminal behavior. Such an effect would result if would-be offenders *believe* their chances of being arrested for a crime have increased, even if they have not. Of course, this rests on the assumption that changes in police behavior are visible to prospective offenders. Because direct information is generally not available, prospective offenders have little idea about the objective probability of arrests (Sampson and Cohen 1988). Fisher and Nagin (1978: 388) note that "…unlike stock market prices, daily quotations of sanction levels are not available and the information that is available derives from very uncertain sources, including the criminal's own experience, the experience of his peers, [and] news reports…" Moreover, arrests for many serious index crimes (e.g., robbery) are relatively rare, so potential offenders often do not witness index crime arrests. On the other hand, vigorous intervention by police on driving violations, drunkenness, and public disorder is a visible indicator of police activity in an area, according to Sampson and Cohen (1988). They suggest, therefore, that control of minor offenses may have an influence on crime rates such as robbery that is not strictly tied to changes in the objective arrest risk for the latter.

Interest in proactive policing has grown markedly since the publication of Sampson and Cohen's study, in large part due to the highly favorable media coverage of the adoption of such a strategy in New York City under the leadership of police commissioner William Bratton. Violent

crime fell dramatically in NYC during (and following) Bratton's tenure (Kelling and Coles, 1996: 110,148-156). Despite favorable press coverage, however, the scholarly literature on the effectiveness of the NYC policing innovations has yielded mixed results (Bowling, 1999; Conklin, 2003; Harcourt, 2001; Kamen, 2000; Messner et al., 2007; Rosenfeld et al., 2007).

From this discussion, it is clear the role of policing in reducing violent crime remains an open question. Moreover, an obvious drawback of much of the research in this area is its limited geographic coverage—findings reflect the experience of a single city. A major contribution of Sampson and Cohen (1988) is their introduction of a proactive policing measure that is based on arrest data collected as part of the *Uniform Crime Reporting* program. They operationalize proactive policing in terms of the ratio of arrests for two "public order" crimes recorded in the *UCR* —disorderly conduct and driving under the influence (DUI)—to the number of police officers in the jurisdiction. These data are available for a large number of cities. Moreover, this policing measure does not include in either the numerator or denominator terms that appear in typical dependent variables (e.g., the number of violent offenses known).

More recently, MacDonald (2002) examines these issues in his study of large U.S. cities in the mid-1990s. Assessing the economic and political determinants of robbery and homicide rates in 164 cities, MacDonald (2002) finds that while community policing has little effect on the control and decline in violent crime during the 1990s, proactive policing strategies, as operationalized by Sampson and Cohen, have a significant inverse effect on violent crime rates and are also related to reductions in violent crime over time. Specifically, police departments with a more aggressive enforcement of disorderly conduct and driving while under the influence exhibit lower levels of robbery and homicide, consistent with the deterrence perspective.

SPECIFYING A MORE COMPLETE MODEL

While Sampson and Cohen (1988) and MacDonald (2002), among others, offer support for the idea that proactive policing is associated with lower violent crime rates, these analyses are limited in three important respects. First, research now considers the combined effects of social and economic predictors of crime. In many inner-city communities, as a result of macro-economic changes that have disproportionately affected the urban poor, scholars claim it is the combination of poverty, unemployment, and family disruption that defines the socio-economic context for residents (Sampson and Wilson, 1995; Wilson, 1987). They posit that "concentration effects" contribute to social disorganization, which, in turn, leads to more crime and violence. Consistent with these arguments, researchers typically measure the multiple disadvantages that characterize areas by incorporating several measures into an overarching index of concentrated disadvantage. As Land et al. (1990) note, incorporating these covariates into an index has the added benefit of minimizing multicollinearity. Given this measure is found to be positively associated with violent crime rates at "virtually all levels of analysis and time periods" (Land et al., 1990: 952), a rigorous assessment of the impact of proactive policing requires inclusion of a comprehensive measure of concentrated disadvantage.

Second, prior research on proactive policing and crime typically does not consider local politics, which is ironic given the intellectual history of this literature. Sampson and Cohen's (1988) arguments regarding aggressive policing are based on Wilson's (1968) seminal book *Varieties of Police Behavior*, which identifies three policing styles—the watchman, the legalistic, and the service.[4] For Wilson, the adoption of these organizational styles was driven by the local

[4] The watchman style emphasizes order maintenance, the legalistic style emphasizes vigorous, professional law enforcement, and the service style emphasizes serving the public and aiding citizens in need.

political culture. Some research has shown police strategies and politics to be related (Crank, 1990; Langworthy, 1985; Slovak, 1986; Wilson and Boland, 1978), yet Langworthy (1986) notes minimal differences in police styles across political cultures, concluding that police strategies are primarily internally determined. Hence, for many years, policing studies ignored political effects.

Recently, however, politics appears to have re-entered policing research. Stucky (2005) finds that cities with unreformed or "traditional city political systems" are likely to have more police per capita, net of other factors (see also Stucky, 2003). Similarly, Choi et al. (2002) find that cities with elected mayors were more likely to apply for Community Oriented Policing Services (COPS) grants from the federal government. They argue this was due to potential credit claiming opportunities for mayors who want to be seen as doing something about crime. Politics thus seem to matter for policing, although the direction of political effects may be opposite to that originally suggested by Wilson. Wilson argues that aggressive policing would be more likely in cities with reformed political systems because city managers would hire "professional" police chiefs. Yet, as just noted, research finds the opposite—traditional cities had more police per capita and were more likely to apply for COPS grants. We argue this is precisely because they are more susceptible to political pressure. If proactive policing results from community pressure to "do something about crime" beyond traditional strategies, it seems most likely to occur in places where community influence on government action is maximized (i.e., cities with elected mayors, partisan elections, and district based council representation). Given these arguments, it is essential to consider the effects of local politics on the style of policing and crime rates.

A third important limitation of the research that has followed in the tradition of Sampson and Cohen is the failure to directly confront the vexing problem of endogeneity, referenced earlier. Sampson and Cohen (1988: 171) correctly observe their innovative measure of proactive

policing “avoids spurious correlations” that could be produced by common terms in the deterrence measure and the crime rate (e.g., offenses known). However, they dismiss the possibility of simultaneity on purely a priori grounds arguing that “police intervention in moving violations and disorderly conduct is not causally determined by the crime rate but rather by the dominant political culture and the professionalism of the police department” (p. 171). This claim is by no means obvious. It is possible, for example, that increases in violent crimes such as robbery might stimulate a redeployment of police resources away from the less serious crimes of moving violations and disorderly conduct to combat more serious offenses. The observed negative association between proactive policing and robbery might thus reflect a causal process opposite to the deterrence hypothesis. Alternatively, if rising robbery rates stimulate pressures for the police to be “doing something” that is highly visible, the effect of increasing robbery rates on measures of proactive policing might be positive. Under this scenario any deterrent effect of proactive policing might be suppressed in models that fail to account for endogeneity.

In sum, although some previous research is suggestive of the deterrent effect of proactive policing, the substantive and methodological issues discussed above preclude firm conclusions. In this vein, Weisburd and Eck (2004: 60) note: “Many [police] tactics that are applied broadly throughout the United States have not been the subject of systematic police research nor have they been examined in the context of research designs that allow practitioners or policy makers to draw very strong conclusions. American police research must become more systematic…if it is to provide solid answers to important questions of practice and policy.” In line with this claim, the goal of the current study is to determine whether the previously observed deterrent effects of proactive policing are robust across more fully specified and methodologically rigorous models.

DATA AND METHODS

To facilitate comparisons with Sampson and Cohen (1988), we apply analogous selection criteria to generate the sample — U. S. cities with a population of 100,000 or more with at least 1,000 blacks in 2000.[5] Missing data reduces the sample somewhat, resulting in a sample size of 181 cities for our cross-sectional analyses. Our sample can be regarded as a reasonable reflection of contemporary large cities with appreciable black populations, representing seventy-eight percent of the universe of cities with the specified characteristics.[6] Our assessment of dynamic processes necessitates multiple years of observation. Given our original cross-sectional analyses employ data from the four year period, 2000-2003, we create a panel data structure for our investigation of endogeneity by extending backwards over the preceding 4-year period, 1996-1999. This eight year period gives us sufficient power to apply the Arellano-Bond dynamic panel data estimator that relies on identification by using lagged levels of the dependent, predetermined, and endogenous variables, and differences of the strictly exogenous variables.[7]

The data were collected from five sources: (1) counts of robberies known to the police and city population totals as compiled in the FBI's (UCR) program; (2) yearly arrest counts for DUI and disorderly conduct for agencies that submitted 12 complete months of data to the UCR; (3) police employee data as contained in the FBI's Police Employee Master File; (4) demographic data as reported in the 1990 and 2000 Census; and (5) two databases on political system characteristics of city governments. The UCR counts of robberies known to police for

[5] The minimum black population requirement is necessary to measure racial inequality (see Blau and Blau, 1982).
[6] A few cities require elaboration. First, Honolulu is not considered a place as defined by the Census so for all census variables we use Honolulu MSA data. Second, Las Vegas Metro PD does not have a corresponding census place so we use Las Vegas urbanized area census data. The Census recognizes Amherst Town, NY and Ramapo, NY as county subdivisions and not places. Therefore, we use country subdivision data.
[7] We used the 2000 sample as our selection criterion; therefore a few places have populations less than 100,000 and/or less than 1,000 blacks in years prior to 2000. Athens-Clarke County, GA was a new geographic type in the 2000 census. No comparable geography was available in the 1990 census. Thus, it is not included in panel analyses.

cities, along with corresponding population totals were taken from files distributed by ICPSR for all years except 2003.[8] The 2003 UCR data were taken from the FBI's website (table 8, http://www.fbi.gov/icr.htm#cisu). The FBI provided the arrest and police employee data in a personal communication.[9] The Census data on socio-demographic characteristics of cities were taken from the 1990 and 2000 Census tapes.[10] Information regarding city political system characteristics was obtained from the International City County Management Association (ICMA) and the National League of Cities' Municipal Officials Database.

DEPENDENT VARIABLE

To generate the sample, we initially collected data on robbery "offenses known" for all cities with populations 100,000 and over that were in the UCR for the four-year period: 2000-2003.[11] Robbery rates (per 100,000 population) were computed in the conventional manner for each year with available data using the robbery counts and totals from the UCR. To reduce the likelihood that reporting error might produce unstable estimates, we smoothed the data by aggregating the counts over the multi-year period. We imposed the selection criterion that a city must have at least 2 years of data to be included in the cross-sectional analyses. If a city reported for all years, the robbery rate is based on the 4-year total. If the city reported for three of the four

[8] The source is the National Archive of Criminal Justice Data located on the Inter-University Consortium for Political and Social Research (ICPSR) website (http://www.icpsr.umich.edu/NACJD/archive.html, as compiled in studies #9028 (1996-97), # 2904 (1998), #3158 (1999), #3447 (2000), #3723 (2001), and #4008 (2002).
[9] We are grateful to Debra K. Mack, Chief of Programs Support Section of the FBI's Criminal Justice Information Services Division, for providing the arrest and employee data and for clarification of these data.
[10] Sources for the Census data and documentation are: Census 2000 Summary File 1 [United States] / prepared by the US Census Bureau, 2001; Census 2000 Summary File 1 Technical Documentation / prepared by the U.S. Census Bureau, 2001; Census 2000 Summary File 3 [United States] / prepared by the U.S. Census Bureau, 2002; Census 2000 Summary File 3 Technical Documentation / prepared by the U.S. Census Bureau, 2002; Census of Population and Housing, 1990: Summary Tape File 1 on CD-ROM; (United States) [machine-readable data files] / prepared by the Bureau of the Census.--Washington: The Bureau [producer and distributor], 1991; Census of Population and Housing, 1990: Summary Tape File 3 on CD-ROM [machine-readable data files] / prepared by the Bureau of the Census. -Washington: The Bureau [producer and distributor], 1992.
[11] Cities in some states (e.g., Florida, Illinois) did not report information to the UCR and are excluded from analyses.

years, the robbery rate reflects those three years, and so on. The smoothed robbery rates were transformed to natural logarithm scale to reduce right skew in our cross-sectional analyses.

We used single year calculations of the robbery rates, rather than smoothed four year averages, in the panel analyses reported in table 2 and table 4. Robbery rates were computed in the conventional manner for the years 1996-2003. We also constructed the four-year average for 1996-1999 (reported in table 3) using the same rules described above for the 2000-2003 average for purposes of estimating cross-lagged regressions (explained below).

INDEPENDENT VARIABLES

The primary independent variable for the analysis is proactive policing. Following Sampson and Cohen (1988), this variable is a ratio measured as the sum of the number of arrests for driving under the influence and disorderly conduct, divided by the number of sworn police officers. For the cross-sectional analyses, we computed this ratio for each year during the 2000-2003 period and averaged the values for years with non-missing data, imposing the requirement of a minimum of two years of data for inclusion. Proactive policing ratios were transformed to natural log scale in our cross-sectional analyses. We used single year proactive policing ratios for the 1996-2003 panel data analyses reported in tables 2 and 4 and the 1996-1999 average for the analysis reported in table 3.[12]

In addition to examining the direct effect of proactive policing on robbery rates, Sampson and Cohen estimate an indirect effect via the robbery arrest/offense ratio. We computed an arrest/offense ratio by dividing the number of robbery arrests by the number of robberies known

[12] In 2002, proactive policing was problematic as there were only 20 valid cases. This was due, in most part, to missing data for the DUI and disorderly conduct arrests. As explained subsequently, the Arellano-Bond dynamic panel data estimator does not require complete data, so unbalanced data and missing observations pose no further threat to unbiased estimation. For all other years, missing data for proactive policing posed no serious problems.

to the police. These ratios were averaged over as many years within the period with non-missing data, with a minimum of two years for inclusion in the cross-sectional analyses. The arrest/offense ratio for our dynamic panel analyses were computed as above and remained single year measures for the analysis period of 1996-2003.[13]

In the interest of replication, we collected data on the same set of independent variables that appear in Sampson and Cohen's (1988) models: city population size (logged), median family income, percentage of the population age 15+ that is divorced, the proportion of the population that is non-Hispanic black, racial income inequality (ratio of the white per capita income to black per capita income), and a binary variable indicating cities located in the West as defined by the Census. To extend Sampson and Cohen's models to include additional covariates, we collected data from the 2000 Census on measures commonly used in the literature, specifically poverty (percent of the population below the poverty line), unemployment (percent of the population age 16+ who are unemployed), education (percent of the population age 18+ who have graduated from high school), female-headed households (percent households with children under age 18 that have a female householder and no husband present), residential instability (percent of the population age 5+ who have moved into a different house within the last 5 years), and percent young males (percent of the population that is male aged 15-24).

As noted earlier, a growing body of literature has identified these covariates as robust predictors of violent crime rates (Krivo and Peterson, 2000; Land et al., 1990; Messner and Golden, 1992; Parker and McCall, 1999; Wadsworth and Kubrin, 2004). Many are highly correlated, however, and produce collinearity when individually included in models. To reduce collinearity, we conducted principal components analysis on residential instability, racial

[13] Again, the year 2002 had a majority of missing data for the variable construction (robbery arrest information was missing); thus, only 51 cases for arrest/offense ratio in 2002 were non-missing.

inequality, percent young males, percent divorced, percent poverty, percent unemployed, percent high school graduates, percent female-headed households, median family income, and percent black. Consistent with prior research (Land et al., 1990), a "disadvantage" component emerged from the results. The following variables loaded strongly on this component (factor loadings in parenthesis): percent female headed households (.90), percent poverty (.92), percent unemployed (.90), median family income (-.89), and percent high school graduate (-.75).[14] With an eigenvalue of 3.8, this component accounted for 76 percent of the variation in the construct. We computed the index using the corresponding factor scores as multipliers.

We collected the same information from the 1990 Census and applied linear interpolation/extrapolation to construct annual values of the socio-demographic variables between the 1990 and 2000 Census for our panel data analyses. Our 1990 disadvantage component was consistent with the 2000 measure. The corresponding loadings for the 1990 factor (in parenthesis) were: percent female headed households (.90), percent poverty (.91), percent unemployed (.90), median family income (-.84), and percent high school graduate (-.84). The 1990 measure of disadvantage had an eigenvalue of 3.9 and accounted for approximately 77 percent of the variation of the construct.

Information on city political system characteristics was derived from two sources—the 2001 Form of Government (FOG) survey conducted by the International County/City Management Association and the National League of Cities (NLC) Municipal Officials Database, which was provided in personal communication by John Miller.[15] The index, ranging from 0-3, is composed of three elements relating to city political organization. The index

[14] We corrected for polarity to insure the index accurately reflected disadvantage for the 2000 and 1990 measures.
[15] This raises potential causal order issues. It is quite unlikely, however, crime in a particular year will lead to changes in the structure of city political systems. Moreover, city political system characteristics tend to be fairly stable over time.

increases by 1 for cities with mayor-council forms of government. It also increases by 1 if some or all city council members represent specific geographic areas. Finally, the index increases by 1 if city elections are partisan. Thus, the traditional government index is maximized at a value of 3 when there is a mayor-council form of government, city council members represent districts, and local elections are partisan. Unfortunately, information on local political arrangements is missing for some cities in the sample. To reduce the number of missing cases, Internet research determined the nature of local political arrangements from official city websites. Cities were assumed to be mayor-council unless a city manager was listed on the website. Similarly, if city council member lists referred to districts they were considered to be district cities, and so on.

ANALYTIC FRAMEWORK

We begin the analysis with a replication of Sampson and Cohen. This entails two cross-sectional OLS regressions with the arrest/offense ratio and the logged robbery rate serving as dependent variables, using Sampson and Cohen's set of covariates. Second, we expand their model for predicting robbery rates to include additional key covariates. Third, we estimate panel data models that allow us to take advantage of the temporal order of the relationship between proactive policing (X) and robbery rates (Y).

One of the most challenging tasks for statistical modeling is making causal inferences from non-experimental data. Panel data offer decided advantages over cross-sectional designs for the analysis of causal interrelationships among variables (Finkel, 1995). Cross-sectional data can provide evidence of covariation, but causal inference must be grounded in strong theory. In its absence, panel data enable specification of models that satisfy the time precedence criterion for a causal effect to exist and can be used to control for the effects of outside variables that otherwise may produce spurious association. As discussed above, prior theory offers few guidelines

concerning the precise nature of the temporal process that might link proactive policing with robbery rates. Accordingly, we explore five types of panel data models that capture the following hypothesized effects: 1) the contemporaneous effect of proactive policing on the level of robbery rates; 2) the effect of lagged proactive policing on the subsequent level of robbery rates; 3) a fixed-effects model of lagged proactive policing on the subsequent level of robbery rates; 4) the effect of changes in proactive policing on contemporaneous changes in robbery rates; and 5) the effect of lagged proactive policing on the subsequent level of robbery rates with lagged robbery rates included as a predictor. Taken together, these models allow us to examine the effect of proactive policing on robbery rates net of other variables and controlling for endogeneity, while allowing us to probe several important assumptions underlying models in previous research.

Another difficult problem in non-experimental research is how to statistically control for unobserved differences that may produce spurious correlations. We may cast this as a question of making it possible to control for variables that have not or cannot be measured; or we may interpret this as a question of whether "to pool or not to pool" the data when we have repeated observations for the same units. While it is true that all units differ, an assumption that the regression function is constant over time and space yields efficiency and parsimony. Yet, a pooled model can be greatly misspecified if this assumption is incorrect. From either perspective, a solution can be found by using only within-unit, over-time differences, essentially discarding any information about differences between units. This class of regression models, known as fixed-effects models, allows each unit to serve as its own control, such that the change in each unit's outcome is solely a function of change in its independent variables. The essence of a fixed-effects model is to examine how within-unit, over-time variation in explanatory variables is related to over-time variation in the dependent variable. By discarding between-unit variation,

fixed effects models get rid of the source of variation that is likely to be confounded with unobserved characteristics of the units (Allison, 2005). In so doing, any correlations between the unobserved variables with the observed variables are inconsequential.

The simplest way to purge all between-unit variation, and retain only within-unit, over-time variation, is to allow each unit its own intercept by entering *N*-1 dummy variables into the regression equation. This approach, known as the least-squares dummy variables (LSDV) estimation, is computationally burdensome for large *N*. Fortunately, mean differencing within units, say, $y^*_{i,t} = y_{i,t} - \bar{y}_i$; $x^*_{i,t} = x_{i,t} - \bar{x}_i$, produces the same results (Greene 2002).[16]

It is also easy to show that first differencing, rather than mean differencing, can also eliminate unobserved variables. In the simple case of *T*=2, we could write separate time 1 and time 2 equations:

$$y_{i1} = \alpha_1 + x_{i1}\beta + z_i\gamma_1 + \varepsilon_{i1}, \qquad i=1,2,\ldots,N \qquad \text{eq. 1}$$
$$y_{i2} = \alpha_2 + x_{i2}\beta + z_i\gamma_2 + \varepsilon_{i2}.$$

If we consider the ***z*** to be unobserved variables (referred to as the unit effect, $u_i = z_i\gamma$) and subtract the time 1 equation from the time 2 equation, the difference model is:

$$y_{i2} - y_{i1} = \alpha^* + \beta(x_{i2} - x_{i1}) + (\gamma_2 - \gamma_1)z_i + \varepsilon_i^* \qquad \text{eq. 2}$$

where $\alpha^* = \alpha_2 - \alpha_1$, $\varepsilon^* = \varepsilon_2 - \varepsilon_1$, and, if we assume that $\gamma_1 = \gamma_2$, then $z_i\gamma_2 - z_i\gamma_1 = 0$. Unobserved (and time-invariant) variables are differenced out of the equation.

[16]In an excellent text, *Fixed Effects Regression Methods for Longitudinal Data Using SAS*, Allison (2005) demonstrates the equivalence between LSDV–estimated fixed effects and "conditioning out" all stable characteristics of cross-sectional units via OLS estimation on within-unit mean deviation scores. Allison also presents a comprehensive comparison of the strengths and weaknesses of fixed effects vs. random effects methods, explicating a powerful "hybrid method" that addresses each approach's limitations by blending the strengths of both.

Yet another statistical issue in panel data analysis concerns the merits versus deficits of dynamic specifications (Allison, 1990; Achen, 2000; Beck and Katz, 2004; Keele and Kelly, 2006). This debate addresses the specification of a lagged dependent variable when modeling change. Although the protagonists in this debate are far from in agreement, we accept the position that the issue of whether to include a lagged dependent variable comes down to a theoretical question: "does the past matter for the current values of the process being studied?" If one suspects that it does, then inclusion of a lagged dependent variable is appropriate. Because prior crime levels, for a variety of reasons, are likely to influence future crime levels, we believe this is the appropriate way to approach panel data models of crime.

If we combine the features of differencing to remove spuriousness (from unobserved variables) with the theoretical rigor of allowing past values to determine current ones, we have a model that is well-positioned to estimate unbiased effects of explanatory variables.[17] Consider a model containing a lagged dependent variable and an explanatory variable *x*:

$$y_{it} = \alpha + \rho y_{i,t-1} + \beta_1 x_{it} + u_i + \varepsilon_{it} \qquad \text{eq. 3}$$

where $u_i = z_i \gamma$ is referred to as the unit effect and $(u_i + \varepsilon_{it})$ is known as the composite-error term. The first difference transformation removes both the constant term and the unit effect (i.e., impact of unobserved variables, where $u_i = z_i \gamma$):

$$y_{it} - y_{i,t-1} = \rho(y_{i,t-1} - y_{i,t-2}) + \beta_1(x_{it} - x_{i,t-1}) + (\varepsilon_{it} - \varepsilon_{i,t-1}) \qquad \text{eq. 4}$$

or more simply:

$$\Delta y_{it} = \rho \Delta y_{i,t-1} + \beta_1 \Delta x_{it} + \Delta \varepsilon_{it} \qquad \text{eq. 5}$$

[17] Differencing also presents a solution to the difficulty arising when one tries to place the fixed-effects model in the context of a dynamic panel model due to the correlation between the lagged dependent variable and the error term (Nickell, 1981).

The differenced lagged dependent variable will be correlated with the error term (the former contains $y_{i,t-1}$ while the latter contains $\varepsilon_{i,t-1}$); but with the unit effect, u_i swept out we may construct instruments for the lagged dependent variable from the second and third lags of *y*, in the form of differences or lagged levels. Depending on the strength of ρ and if ε is *i.i.d.* (independent and identically distributed), lags of *y* will be highly correlated with the lagged dependent variable (and its difference) but uncorrelated with the composite-error term (Anderson and Hsiao, 1982; Arellano and Bover, 1995; Baum, 2006: 233). This form of instrumental variable estimation in the context of the dynamic panel data model is the essence of the Arellano-Bond estimator (Arellano and Bond, 1991).

Although the conventional panel design allows for modeling a temporal sequence, the successful estimation of causal effects in the presence of endogeneity is rarely unambiguous, even with panel data. The assumption that proactive policing (as well as all other independent variables) are strictly exogenous means that their observed values are uncorrelated with the regression error in a model predicting robbery rates. If proactive policing is not strictly exogenous—if robbery rates influence policing—then the assumption for OLS to estimate the parameters of the structural model and their accompanying test statistics consistently is undermined, and the parameters are not identified.

It is relatively easy to show this. Consider our point of departure, eq. 2, in which we estimate the impact of change (first difference) in proactive policing on the change (first difference) robbery rate, with the simple linear model using OLS:

$$\Delta RobRat_i = \beta \Delta PP_i + \Delta u_i \qquad \text{eq. 6}$$

where $\Delta Robrat_i$ is the first difference in robbery rate in locality *i* between time *t* and *t*-1, ΔPP_i is first difference in the measure of proactive policing, and Δu_i is the error term. Assume, for the

moment, that eq. 6 is well-specified but also that it captures only part of the picture – causality also runs from the change in level of robbery rate to proactive policing. Here we can specify:

$$\Delta PP_i = \gamma \Delta Robrat_i + \Delta u_{ppi} \qquad \text{eq. 7}$$

showing that the change in level of proactive policing is influenced by robbery rates, wherein we expect $\gamma > 0$ because police agencies adopt the proactive policing innovation in response to increasing crime rates. If eq. 6 is estimated by OLS but the true set of relationships is captured by eqs. 6 and 7 together, then the estimated effect of proactive policing on robbery rates, $\hat{\beta}$, will not be "consistent" – it will suffer from "endogeneity" or "simultaneity" bias – because the regressor ΔPP is itself endogenous in a system of simultaneous equations, making it correlated with the error term Δu_i in eq. 6; and $\hat{\beta}$ will be biased upwards by the positive quantity $\hat{\gamma}$. Indeed, if the reverse causality is strong enough, i.e., if $\hat{\gamma}$ is large relative to $\hat{\beta}$, we would infer that $\beta > 0$ even if the true impact of proactive policing on crime is negligible or negative.

In cross-sectional data, estimation of reciprocal causal effects data relies on incorporating instrumental variables that satisfy several restrictive assumptions about the relationship between these variables with *x* and *y*, and the error terms in their respective equations. The temporal component of panel designs, on the other hand, gives us leverage to observe how prior values of *x* influence future values of *y*, and vice versa. Accordingly, we proceed to estimate cross-lagged regression models (Finkel, 1995) to elicit evidence, or the lack thereof, of endogeneity between proactive policing and robbery rates.

Given that our baseline replication of the Sampson and Cohen cross-sectional analyses uses averaged data from the four year period, 2000-2003, and that we extend the panel data structure for our investigation of endogeneity backwards over the preceding 4-year period, averaging scores from 1996-1999, we specify a two-wave design (wave 1 for 1996-1999 and

wave 2 for the original 2000-2003 period). The cross-lagged effects model can then be shown as a two dependent variable extension of a two-period conditional change/static-score model (Finkel, 1995). The two structural equations can be written as:

$$\begin{aligned} \Delta y_{i2} &= \beta_1 \Delta x_{i1} + \beta_2 \Delta y_{i1} + \Delta u_{i1} \\ \Delta x_{i2} &= \beta_3 \Delta y_{i1} + \beta_4 \Delta x_{i1} + \Delta u_{i2} \end{aligned} \qquad \text{eq. 8}$$

Visually the two-wave cross-lagged effects model has the following form:

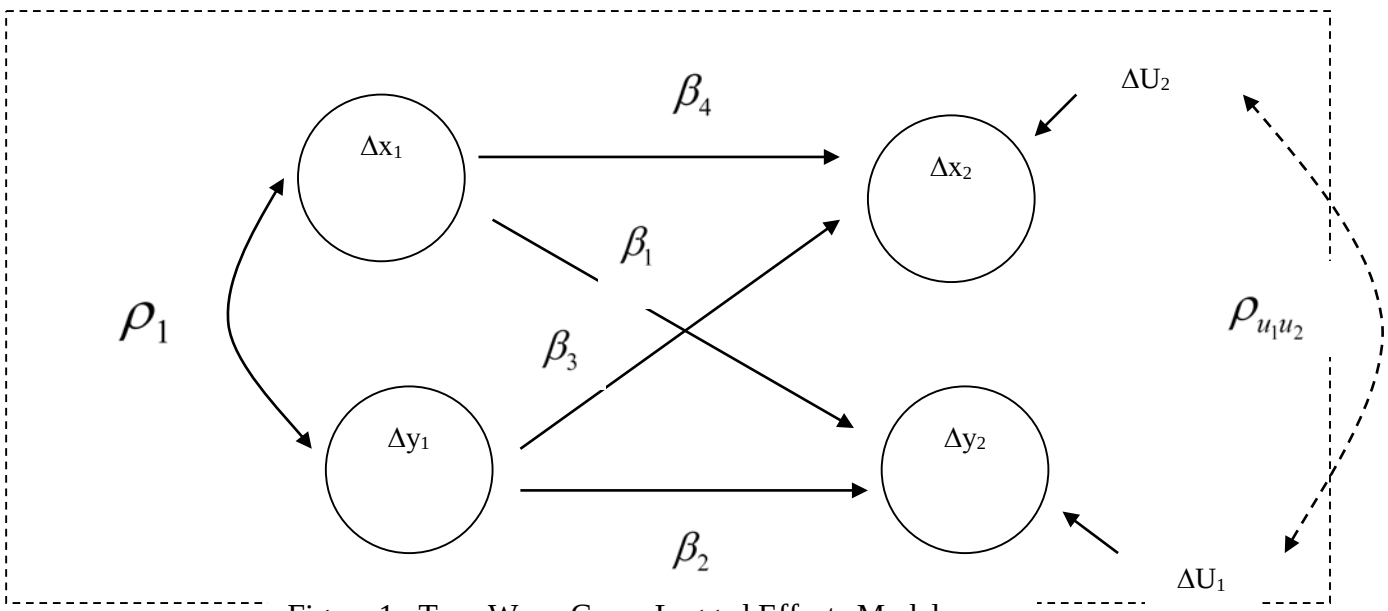

Figure 1. Two-Wave Cross-Lagged Effects Model

where Δx_1 and Δx_2 represent the average annual change in proactive policing at waves 1 and 2, respectively, Δy_1 and Δy_2 are the corresponding average annual change in robbery rates, the correlation between the wave 1 variables is given by ρ_1 and the correlation between the structural disturbances of wave 2 is captured in $\rho_{u_1 u_2}$ (see table 3). Under assumptions that disturbance terms Δu_1 and Δu_2 have zero means, constant variance, and are uncorrelated with the lagged endogenous variables Δx_1 and Δy_1, the cross-lagged model parameters are consistently estimated via OLS.

The two-wave cross-lagged effects model has wide applicability and represents the most basic model for estimating possible reciprocal effects. The coefficients of interest are the cross-

lagged effects from the wave 1 variables to the wave 2 outcomes (i.e., β_1 and β_3) because these represent the estimate causal effect from each variable to the other. The cross-lagged model corresponds to the Granger test for causality in time series analysis, which posits that a variable "Granger-causes" the other if any value of the first variable measured at time *t*-1, *t*-2,…*t*-m, has a significant effect on the second variable at time *t*, controlling for the second variable's prior values (Finkel, 1995: 24-28). In the language of the fixed-effects model, the effect of Δy_1 on Δx_2, controlling for Δx_1, represents the effect of the average change (between 1996-1999) in robbery rates in the ith city on the average change in proactive policing in the subsequent period (2000-2003) in that same i^{th} city after accounting for the impact of change in proactive policing during the prior (1996-1999) period; and vice versa for the effect of Δx_1 on Δy_2, controlling for Δy_1. To anticipate the results, cross-lagged regression models suggest that proactive policing responds to prior change in robbery in exactly the manner described in eq. 7 with $\gamma > 0$.

With evidence that proactive policing is endogenous to robbery rates, our final model, and our concluding inference, is derived from the Arellano-Bond dynamic panel data estimator (Arellano and Bond, 1991; Arellano, 2003; Baum, 2006). The Arellano-Bond estimator was designed for situations that embody every aspect of our research problem (Roodman, forthcoming). First, our data are observed in a relatively small number of time periods and dominated by a much larger number of cross-sectional units, "small *T*, large *N*", that is, a panel data structure rather than a time-series cross-section structure (Beck and Katz, 2004). Second, we assume the data generating process is dynamic, with current realizations of the dependent variable influenced by past ones. Third, we allow that our panel data have arbitrarily distributed fixed individual effects, which admit that each unit has its own data generating process rather than one shared process. This is equivalent to the assumption that unobserved (and possibly

unobservable) variables may produce spurious correlations. Fourth, we allow that proactive policing is not strictly exogenous to robbery rates, meaning that it is correlated with past and possibly current realizations of the error term. Other explanatory variables are assumed to be exogenous. Fifth, the only available instruments are "internal" – based on lags of the instrumented variables. Sixth, we assume that the error variance (but not the first difference transform) may be heteroskedastic and autocorrelated within units but not across them. Finally, the data generating process is suited to a linear functional relationship.

Appendix A reports relevant model diagnostics and technical details of our use of the Arellano-Bond dynamic panel data estimator. In non-technical and highly simplified terms, the procedure involves purging the unobservable individual effects by differencing, fitting a dynamic panel-data model by drawing instruments from within the data set with lags, and assessing the essential assumptions of no serial correlation and identification.

RESULTS

Sampson and Cohen hypothesize that proactive policing is likely to be related to robbery rates through two processes: an indirect effect via an increased risk of apprehension and a direct effect reflecting deterrence accompanying greater police visibility. The first equation, Model I, in table 1 is relevant to the hypothesis of an indirect effect of proactive policing on robbery rates. In this equation, we regress the arrest/offense ratio on proactive policing and control for the same variables specified in Sampson and Cohen's analysis. Contrary to their findings, proactive policing has no effect on the arrest/offense ratio. This suggests that proactive policing does not reduce robbery rates by changing the actual probability that arrests are made. Considering Sampson and Cohen's control variables, the results also indicate that certainty of arrest for

robbery is lower in large cities and cities with appreciable black populations, and higher in more affluent cities as reflected in median income.

[table 1 about here]

Turning to the prediction of robbery rates, Model II is again based on Sampson and Cohen's specification. The evidence is consistent with the hypothesis of a direct effect of proactive policing on robbery rates. Proactive policing exhibits a significant negative association with robbery rates: cities with greater numbers of arrests for DUI and disorderly conduct relative to the number of sworn police officers tend to have lower robbery rates, controlling for the structural characteristics in the model. With respect to the control variables, the percent of Non-Hispanic Blacks in a city and racial inequality are positively associated with robbery rates. In addition, median income is significantly associated with robbery rates in an inverse direction, net of other factors. The results in Model II also show that population size is positively associated with robbery rates, as is location in the Western part of the U.S. Contrary to Sampson and Cohen, percent divorced in a city does not exert a significant effect on robbery in this model.

Model III in table 1 allows for more rigorous testing of the cross-sectional relationship between proactive policing and robbery rates. In this model, the measure of median income is incorporated in the disadvantage index, and measures of governmental structure, age/sex structure, and residential instability are included. Results reveal that the effect of proactive policing withstands the introduction of the additional controls. Population size and racial inequality continue to exhibit significantly positive effects. The dummy variable for "West" is no longer significant. The government index and the measures of age/sex structure and residential mobility also are not significant. Alternatively, as expected, the disadvantage index yields a

significantly positive coefficient. Indeed, it is by far the most powerful predictor of robbery rates, as indicated by the comparatively large standardized coefficient (.536).[18]

Turning to the panel analyses, we begin by estimating conventional panel models that assume all predictor variables are exogenous to robbery rates (see table 2). We consider five specifications. In the first, the robbery level (in log form) is regressed on contemporaneous values of predictor variables (Model IV). In the second, causal inference is strengthened a bit by capitalizing on the temporal ordering of cause and effect; accordingly, Model V regresses (log) robbery rates on prior values (one year lag) of the independent variables. Our next model (Model VI) is a fixed-effects model.[19] Differences in inference between Models IV and VI reveal any pernicious effects of unit heterogeneity. Model VII expresses the outcome as a change score and estimates the effect of change in proactive policing on change in robbery rates, with other time-varying explanatory variables lagged. The final specification is a dynamic panel model where robbery rates at time *t* are regressed on robbery rates at time *t*-1, as well as all other right-hand-side variables measured at time *t*-1. Model VIII is the "conditional change/static-score" or "regressor variable" model (Allison, 1990; Finkel, 1995).[20] It is important to note that Model

[18] Similar to Sampson and Cohen, our results indicate the explanatory power for the models of robbery rates greatly exceeds that for arrest certainty. This is not surprising given the selection of control variables for the baseline model is informed by criminological theories of, and macro-level research on, violent crime. There is little theoretical guidance in the literature concerning the social structural determinants of arrest certainty.

[19] We also fit random effects panel models to account for possible unit heterogeneity (available upon request). While fixed-effects models necessarily exclude variables with no within-unit variability, random-effects panel models permit the inclusion of indicator variables. However, in doing so, random effects estimates use information both within and between units. The unobserved variables must therefore be assumed to be uncorrelated with all observed variables. Unless one makes this assumption, random effects do not really control for the effects of unobserved variables and fixed effects are preferable.

[20] Models IV and V are fit using Prais-Winsten (iterative) estimation of regression coefficients with panel-corrected standard errors under the assumption that disturbances are heteroskedastic and share a common first-order autocorrelation (shown as rho) within panels. Models VII and VIII apply OLS estimation of regression coefficients with panel-corrected standard errors under the assumption that disturbances are heteroskedastic but without correlation within panels (Beck and Katz, 1995).

VIII is still a model of change in robbery rates, but it differs from the specification in Model VII in that it assumes current robbery rates are, in part, influenced by past ones.

[table 2 about here]

Table 2 evidences the impact of unit heterogeneity. Interestingly, proactive policing and population size are the only explanatory variables that retain their signs and significance when we compare Model IV with the fixed effects model (Model VI). Using only within-unit (over time) covariation, divorce rate, percent non-Hispanic black, and disadvantage are no longer statistically significant. Percent young males and percent that moved shift from non-significance to being significant and negative, while the sign for racial inequality is reversed. Clearly, unobserved characteristics of cities affect model estimates.

Our model estimation also gives us perspective on autoregressive error. Estimates of rho in Models IV-VI are strong and positive, ranging from 0.7 to 0.97. This source of correlated error is virtually eliminated in our change models (VII and VIII); indeed model specifications that set rho to zero (table 2) are easily preferred over those which estimate this additional parameter.[21]

At first glance, the panel analyses results appear to yield inconsistent evidence about the deterrent effect of proactive policing on robbery rates. The coefficients for proactive policing in the equations predicting robbery are significant and negative, as in the cross-sectional analyses. A high level of proactive policing in a given year is associated with lower robbery rates, both contemporaneously and in the following year. However, the coefficients for proactive policing on change in robbery rates (including Model VIII) are all non-significant. There is thus no consistent support in the conventional panel models of table 2 for the claim that levels of proactive policing or changes in levels of proactive policing reduce crime.

[21] Supplemental analyses show that ρ =-0.09 in Model VII specification and ρ =0.018 in Model VIII specification.

However, before we dismiss the hypothesis that proactive policing deters robberies, we need to bring evidence to bear on the assumption that proactive policing is exogenous to robbery rates. If a feedback loop exists in a structural model, each of the variables in the loop will be correlated with the error term in the equation in which they are one of the independent variables. This means that the OLS coefficients will be biased estimates of the effects of the variables in the loop. Again, panel data offer a compelling format for assessing reciprocal causation. Table 3 reports the estimated coefficients and test statistics of the cross-lagged model.

[table 3 about here]

The regression estimates shown in table 3 support our suspicion that proactive policing is endogenous to robbery rates. Of the four path coefficients estimated in our cross-lagged regression, only the effect of lagged change in robbery rates on subsequent change in proactive policing attains statistical significance. The sign is positive and the standardized effect size is the largest among the four coefficients, suggesting that police agencies likely adopt more proactive policing strategies in response to increasing crime rates.

Given our results thus far, we close our inquiry by estimating a dynamic panel data model of the effect of proactive policing, treating this as an endogenous regressor, and the other exogenous predictors, including the arrest/offense ratio, on robbery rates using the Arellano-Bond estimator. In table 4 we report two sets of coefficients.

[table 4 about here]

Dynamic models (regression on a lagged dependent variable) portray the time path of the dependent variable in relation to its past values. A justification for the inclusion of the lagged dependent variable is often tied to the concept of the stability of social systems (Coleman, 1968). A causal system is stable if it will approach at some future time period a fixed equilibrium point

where the values of y for each case will be constant (Finkel, 1995: 9). Given that most systems analyzed in empirical research have not yet reached equilibrium, but are moving toward this state, the effect of the lagged dependent variable on Δy is often interpreted as a proxy for causal paths linking prior values of y to its future realizations through variables that are omitted from the model. The regression effects for the explanatory variables are therefore generally interpreted in two ways. In their raw form, they represent the short-term effect on y, or Δy , across the panel waves. If we remove the adjustment of y to its future equilibrium state, we have an alternative interpretation as the "long-run" effects of x on the equilibrium value of y. The long-run effect of a covariate is usually defined to be the sum of the current and lagged coefficients divided by 1 minus the sum of the lagged coefficients on the dependent variable. This latter quantity is referred to as the coefficient of adjustment (Johnston, 1972: 300-303). We report both interpretations in table 4. In the column labeled "**b"** we give the short-term effects; the long-run effects are reported in the subsequent column.[22]

The evidence clearly points to the deterrent effect of proactive policing on robbery. Specifically, in the short-run, proactive policing reduces robbery rates by an average of almost 22 points (robberies per 100,000 population) for every additional increase in arrests for DUI and disorderly conduct per police officer, net of other effects and after accounting for its endogeneity to robbery rates. This deterrent effect is estimated to bring a 25 point reduction when the system reaches its equilibrium state.

None of the coefficients (short- or long-run) for our exogenous variables attains statistical significance.[23] Aside from certainty of arrest, this result is to be expected because the panel

[22] This column is labeled "b" to indicate that the short-run effects given in Table 4 are commensurate with the metric scaled (unstandardized) effects reported in the prior tables, also under the columns labeled "b."
[23] The region dummy and traditional government index are time-invariant and thus "partialed out" of the regression.

scores are the result of linear interpolation/extrapolation from observed scores in the 1990 and 2000 Censuses. This near stability eliminates almost all information because the first difference (change) is constant over time. As such, there is little methodological justification for making inference or interpreting their effects on the change in robbery rates. Rather, these variables are treated as controls to further condition our investigation of the impact of proactive policing.

SUMMARY AND DISCUSSION

The purpose of this research has been to revisit and extend an innovative study that reports an intriguing finding of considerable theoretical and practical significance. In their pioneering study in the 1980s, Sampson and Cohen report a significant, negative relationship between proactive policing and robbery rates. This finding has been interpreted as supporting a principal claim of deterrence theory, namely, that vigorous enforcement of laws prohibiting these forms of disorder increases police visibility and thereby acts as a deterrent against serious violent crimes. In terms of causal modeling, however, the evidence that we bring to bear in support of the desired impact of proactive policing is far more compelling than that from previous efforts.

We have reassessed the impact of proactive policing on robbery rates using more recent data for a comparable sample of cities. Our results replicate the earlier findings of a negative association between proactive policing and robbery rates when an analogous model is estimated. We also have assessed the robustness of this relationship by expanding the set of structural control variables included in the equation. The results reveal that the negative relationship initially observed by Sampson and Cohen (1988), and subsequently reaffirmed by MacDonald (2002), is indeed robust, at least for the sample under investigation. Proactive policing retains its statistically significant negative association with robbery rates in the more fully specified model.

In addition, we have explored potential implications of endogeneity by using a dynamic model from panel data whose practical performance has been thoroughly studied in the econometric literature. The result of our dynamic panel data analyses are supportive of those obtained in the OLS regressions, though model diagnostics unambiguously locate proactive policing as endogenous with robbery rates. Interestingly, our results do not provide additional evidence of Sampson and Cohen's finding of an indirect effect of proactive policing through the arrest/offense ratio. Thus, the effect of proactive policing on robbery rates appears to be through generalized perceptions of greater law enforcement activity, regardless of the actual probability that arrests are made.

While these results are highly suggestive, we acknowledge limitations associated with our analyses. Although our model specification is extensive and includes most of the "usual suspects" in macro-level studies of violent crime, causal inference with non-experimental data is inherently precarious. The adequacy of our solution for simultaneity bias is also open to question. The use of the intertemporal covariance matrix of the errors to the validity of the instrumental variables requires strong assumptions that ultimately depend on a sound theoretical understanding of the causal processes involved. Unfortunately, existing theory offers little guidance as to whether the implied lag structure of this approach is appropriate. We thus do not claim to have resolved the long recognized, but still vexing, problem of disentangling causal order in the study of policing and crime. Our analyses do indicate, however, that further efforts along these lines are warranted, including studies that systematically examine various lag structures using longitudinal data encompassing longer time spans.

A final challenge for future research is to "deconstruct" the notion of proactive policing. We have commended Sampson and Cohen for developing an innovative measure that has a

certain degree of face validity and has the considerable benefit of being based on data collected as part of the UCR, thereby permitting analyses for relatively large samples of cities. However, there is some ambiguity associated with the measure. It is intended to reflect more than what it does on the surface – the enforcement of laws against disorderly conduct and DUI. It presumably captures a more general style of policing which results in a highly visible police presence.

But what *specific* styles of policing are, in fact, captured by the proactive policing indicator? The terms "proactive," "quality of life" or "zero tolerance" policing have been used rather loosely to encompass a wide array of enforcement approaches (Taylor, 2001), which may well have different consequences for crime. Moreover, the proactive policing measure could be serving as a proxy for other policing variables that are associated with the prevention of robbery such as increased training or the use of technologies such as Geographic Information Systems. Unfortunately, the kinds of data necessary to "unpack" proactive policing into the elements of policing most relevant to robbery are not as widely available as the data used to construct the current measure. It is thus not possible here to identify the precise mechanisms that may be at work, but the results of our analyses, in conjunction with earlier studies, provide ample grounds for further exploration into the connection between distinctive policing styles and violent crime rates. With greater knowledge of such mechanisms, it may be possible to design randomized experiments or quasi-experiments that would overcome the limitations inherent in any effort to make causal inferences on the basis of statistical modeling of correlational data (see Berk, 2005).

We close with a comment on policy implications. Policy makers sometimes express frustration with sociological analyses of crime on the grounds that the primary variables under consideration are rarely amenable to direct manipulation (for a classic statement, see Wilson, 1975). Policing, in contrast, is a factor that should be responsive, at least to some degree, to

deliberate decisions about social policy. The identification of deterrent effects of particular styles of policing thus holds considerable promise for crime control. We caution, however, against a narrow approach to the general question of what kinds of policing strategies should be promoted. Policing may very well be important for crime control, but in a democratic society, it is ultimately part of a larger institutional framework dedicated to criminal justice. As Manning (2005) has observed, democratic policing aims to be fair, just, and humane, in addition to providing for the security of the population. Thus, whatever the deterrent effects of proactive or any other policing style might prove to be, policy decisions need to be informed not only by considerations of crime control but by the fundamental values of a democratic society.

Appendix A. Technical Details on the Arellano-Bond Dynamic Panel Estimator

Instrumental variables (IV) estimation provides one solution to the problem of an endogenous explanatory variable. This requires at least one variable that is correlated with proactive policing ("instrument relevance") and that is also uncorrelated with u_i, the error term in the robbery rates prediction equation ("instrument validity"). Because IV estimation relies on satisfying restrictive assumptions, it can fall short. Statistical guidelines are complicated, potentially misleading, and interlaced with decision-making (with all the pitfalls of model selection criteria). For example, instrument relevance implies strong instruments. The consequences of weak instruments are particularly damaging to the IV approach, but finding strong instruments is not an easy undertaking and adding instruments comes with a cost—the finite sample bias of the IV estimator increases with the number of instruments. From the other perspective, testing the instrument validity assumption relies on the validity of orthogonality conditions corresponding to the instruments not being tested (Baum, 2006: 200-202).

IV estimation in panel data was originally proposed by Anderson and Hsiao (1981; 1982) as an alternative to the fixed effects model for sweeping out individual (unit) effects in a model with a lagged dependent variable. The Anderson-Hsiao estimator begins by handling the unit effects by first differencing, then using an instrumental variable that is correlated with the lagged first differenced dependent variable but not the differenced error term. In addressing the criticism that the instrument proposed by Anderson and Hsiao is weak, Arellano and Bond (1991) suggested a much more efficient estimator that uses all available lags at each observation as instruments.[24] It is essential to their use of the inter-temporal covariance matrix of the errors to

[24] Presentation of the material in this section follows closely from Baum (2006) and from documentation of the XTABOND procedure in the *Stata Cross-Sectional Time-Series Reference Manual* (StataCorp, 2003a: 15-33). We use the XTABOND procedure to estimate the models presented herein (StataCorp, 2003b).

the validity of the instrumental variables that the errors are in fact serially uncorrelated. If this condition is not met, the estimators lose consistency.

Because of the critical essence of the lack of serial correlation, Arellano and Bond paid close attention to testing the specification, offering both a direct test for *m*-order serial correlation based on the differenced residuals and Sargan tests (closely related to a Hausman test) of over-identifying restrictions (Arellano and Bond, 1991: 281-283). As general specification tests, the Sargan tests have a dual utility: when used in conjunction with the direct test for serial correlation, the Sargan tests can help shed light on the causal designation of right-hand side (RHS) variables because the Sargan tests can be used to exploit differences in the over-identifying restrictions based on treatment of RHS variables (Arellano and Bond, 1991: 291).

The Arellano-Bond estimator fits a dynamic panel-data model of the form:

$$y_{it} = \sum_{j=1}^{p} \alpha_j y_{i,t-j} + x_{it}\beta_1 + w_{it}\beta_2 + \nu_i + \varepsilon_{it} \qquad i=1,\ldots,N \quad t=1,\ldots,T_i \qquad \text{eq. 1A}$$

where the α_j are p parameters to be estimated, x_{it} is a $1 \times k_1$ vector of strictly exogenous covariates, β_1 is a $k_1 \times 1$ vector of parameters to be estimated, w_{it} is a $1 \times k_2$ vector of predetermined or endogenous covariates, β_2 is a $k_2 \times 1$ vector of parameters to be estimated, ν_i are the random effects that are independent and identically distributed (i.i.d.) over the panels (cities) with constant variance σ_ν^2, and ε_{it} are i.i.d. over the whole sample with constant variance σ_ε^2. Arellano and Bond derived a GMM estimator, known as the Arellano-Bond dynamic panel-data estimator, for α_j, $j \in \{1,\ldots,p\}$, β_1, and β_2 using lagged levels of the dependent variable and the predetermined and endogenous variables, and differences of the strictly exogenous variables. First differencing eq. 1A removes ν_i and produces an equation that is estimable by instrumental variables. Note also that x_{it} and w_{it} may contain lagged independent variables and time dummies,

but time-invariant covariates, like WEST and TGI in our analyses, are partialed out of the estimation by differencing.

Our OLS regressions assumed that all variables on the RHS, including proactive policing, are strictly exogenous. In the language of simultaneous-equation models, variables are termed endogenous if their values are determined within the model and predetermined if their values are determined outside the model, with endogenous variables regarded as stochastic and predetermined variables treated as nonstochastic. Predetermined variables are further divided into two categories: exogenous and lagged endogenous. This latter classification may appear counterintuitive given the preceding definitions, but since the value of a lagged endogenous variable is known at the current time, it is regarded as nonstochastic, hence, predetermined (cf. Johnston 1972). To elaborate, a variable x_{it} is said to be strictly exogenous if $E[x_{it}\varepsilon_{is}] = 0$ for all t and s, for panel i in time t. In many cases, this assumption is not tenable. Intuitively, if the error term at time t has some feedback on the subsequent realizations of x_{it}, then x_{it} is a predetermined variable. Endogenous variables differ from predetermined variables in that the former allows for correlation between the x_{it} and the ν_i at time t, while the latter does not. Formally, an independent variable is predetermined if $E[x_{it}\varepsilon_{is}] \neq 0$ for $s < t$, but $E[x_{it}\varepsilon_{is}] = 0$ for $s \geq t$, and endogenous if $E[x_{it}\varepsilon_{is}] \neq 0$ for $s \leq t$, but $E[x_{it}\varepsilon_{is}] = 0$ for $s > t$.

The Arellano-Bond estimator uses differences as instruments for strictly exogenous variables and levels lagged one or more periods as instruments for predetermined variables. Endogenous variables are treated similarly to the lagged dependent variable, with levels of endogenous variables lagged two or more periods serving as instruments. In addition, the Arellano-Bond approach easily handles missing observations in the interior of the panels. Begin

by considering a simple balanced panel with no missing values and no predetermined or endogenous variables (aside from the lagged dependent variable):

$$y_{it} = y_{i,t-1}\alpha_1 + y_{i,t-2}\alpha_2 + x_{it}\beta + \nu_i + \varepsilon_{it} \qquad \text{eq. 2A}$$

after first differencing eq. 2A we have:

$$\Delta y_{it} = \Delta y_{i,t-1}\alpha_1 + \Delta y_{i,t-2}\alpha_2 + \Delta x_{it}\beta + \nu_i + \Delta\varepsilon_{it}\,. \qquad \text{eq. 3A}$$

The first three observations are lost to lags and differencing. Since x_{it} contains only strictly exogenous covariates, Δx_{it} will serve as its own instrument in estimating the first-differenced eq. 3A, assuming that ε_{it} are not autocorrelated, for each i at t=4, y_{i1} and y_{i2} are valid for the lagged variables. Similarly, at t=5, y_{i1} and y_{i2}, and y_{i3} are valid instruments. Continuing in this fashion we obtain an instrument matrix with one row for each time period that we are instrumenting:

$$Z_i = \begin{pmatrix} y_{i2} & y_{i3} & 0 & 0 & 0 & \dots & 0 & 0 & 0 & \Delta x_{i5} \\ 0 & 0 & y_{i1} & y_{i2} & y_{i3} & \dots & 0 & 0 & 0 & \Delta x_{i6} \\ \vdots & \vdots & \vdots & \vdots & \ddots & \vdots & \vdots & \vdots & \vdots & \vdots \\ 0 & 0 & 0 & 0 & \dots & 0 & y_{i2} & \dots & y_{i,T-2} & \Delta x_{iT} \end{pmatrix}$$

Since the number of lags of the dependent variable, p, is 2, Z_i has $T - p - 1$ rows and $\sum_{m=p}^{T-2} m + k_1$ columns, where k_1 is the number of variables in x. The extension to other lag structures is readily apparent, and unbalanced data and missing observations are handled by dropping rows for which there are no data and filling in zeros in columns where missing data occur. For predetermined variables, levels lagged one or more periods serve as valid instruments; endogenous variables are treated similarly to lagged dependent variables; lagged levels of two or more periods serve as valid instruments.

Although Arellano and Bond derived one-step and two-step GMM estimators, they recommend using the one-step estimator for inference on the coefficients. The two-step Sargan

test, a test of over-identifying restrictions, however, may be better for inference on model specification because the one-step Sargan test over-rejects in the presence of heteroskedasticity. When the number of orthogonality conditions, *r*, exceeds the number of parameters, *k*, there are testable restrictions implied in the econometric model. Estimation of θ in the linear regression, $y = x'\theta + u$, sets to zero *k* linear combinations of the *r* sample orthogonality conditions $b_N(c)$. When the model is right, there are *r*-*k* linearly independent combinations of $b_N(\hat{\theta})$ that should be close to zero.

The main result here is that a minimized optimal GMM criterion scaled by *N* has an asymptotic chi-square distribution with *r*-*k* degrees of freedom (Arellano, 2003: 192-193). A statistic of this form:

$$N_s(\hat{\theta}) = N b_N(\hat{\theta})' \hat{V}^{-1} b_N(\hat{\theta}) \xrightarrow{d} \chi^2_{r-k}$$

is called a Sargan test statistic. The Sargan test statistic for the two-step Arellano-Bond estimator is:

$$S_2 = \left(\sum_{i=1}^{N} \hat{\hat{\varepsilon}}_i^{*\prime} Z_i \right) A_2 \left(\sum_{i=1}^{N} Z_i' \hat{\hat{\varepsilon}}_i^{*} \right)$$

where $\hat{\hat{\varepsilon}}_i^* = y_i^* - X_i^* \hat{\delta}_2$ are the two-step residuals for the differenced y_i and X_i, and $\hat{\delta}_2$ is the two-step estimator of the $K \times 1$ vector of coefficients, Z_i is as defined above, and $A_2 = \sum_{i=1}^{N} Z_i' G_i Z_i$, with $G_i = \hat{\varepsilon}_i^* \hat{\varepsilon}_i^{*\prime}$ estimated by the one-step residuals.

Additionally, Sargan tests are sensitive to the instrument set specified in the Z_i matrix (Arellano and Bond, 1991: 291; table 4). Contradictory assessment to the direct test for serial correlation may reflect failure of the exogeneity assumption, suggesting that alternative forms of the instrument matrix, Z_i, should be considered. Our post-estimation assessment of model

specification, two-step Sargan test of over-identifying restrictions ($\chi^2_{(33)}$ = 44.78; Pr > χ^2 = 0.08), supports the adequacy of our instrumentation and the appropriateness of our assignment of proactive policing as endogenous and all other explanatory variables, including certainty of arrest, as strictly exogenous.

Arellano and Bond also derived a direct test for *m*-order serial correlation based on the differenced residuals[25]. Our assumption of no autocorrelation is evidenced in both the one-step GMM test (z = -0.60; Pr > z = 0.55) and by the two-step GMM test that the average autocovariance in the residuals of order 2 is 0 (z = -0.39; Pr > z = 0.69). In short, model diagnostics overwhelmingly favor the regression estimates reported in table 4.

As noted in the body of the paper, our research design entails a relatively small number of time periods with observations for a much larger number of cross-sectional units, "small *T*, large *N*", that is, a panel data structure rather than a time-series cross-section structure (Beck and Katz, 2004). While various analysts often make much or little of the distinction between temporally and serially dominated data sets (with T > N and N > T), for our purposes the critical issue is whether T is large enough to do the requisite averaging over time, and also whether it is large enough to address some econometric issues (Beck and Katz, 2004). The size of T tells us a lot about which potential econometric problems might be serious ones for the data analyzed. While there is no universally accepted cutoff level, "panel" studies almost invariably have single digit T's (3 being a common value) while "large T, small N" data sets (often referred to as time-series--cross-section (TSCS) data) commonly have T's of twenty or more (Beck and Katz, 2004).

[25] See Arellano and Bond (1991) for derivation of the second-order form and StataCorp (2003a; 2003b) for the *m*-order form.

Although TSCS and panel data may share a common notation, they differ, and the properties of alternative estimators depend not so much on the relative size of T and N, but rather on the absolute sizes of each (cf. Bruno, 2005). For example, since the seminal paper by Nickell (1981), in which it is shown that the least-squares dummy variable (LSDV) estimator is not consistent for finite T in models with a lagged dependent variable, a number of consistent instrumental variable and generalized method of moments estimators (GMM/IV) have been proposed in the econometric literature as an alternative to LSDV (see Anderson and Hsiao, 1982; Arellano and Bond, 1991). GMM/IV estimators can handle different numbers of instruments for each observation by using all available lags at each observation as internal instruments with different lag structures for strictly exogenous, predetermined, and endogenous variables.

More recently Bruno (2005) has argued that a weakness of GMM/IV estimators is that their properties hold when N is large, so they can be severely biased and imprecise in panel data with a small number of cross-sectional units. But GMM/IV estimators can generate moment conditions prolifically. This can cause several problems in finite samples (Roodman, forthcoming). First, since the number of elements in the estimated variance matrix of the moments is quadratic in the instrument count, it is *quartic* in T. In addition, a large instrument collection can overfit endogenous variables. In the extreme, consider that in 2SLS, if the number of instruments equals the number of observations, the R^2's of the first-stage regressions are unity, and the second-stage results match those of (biased) OLS. This bias is present in all instrumental variables regression and becomes more pronounced as the instrument count rises. Although there is little guidance from the literature on how many instruments are too many, it is clear from simulation studies that the number of instruments should not outnumber the cross-sectional units (N) and that small T is preferable to large T (Ruud, 2000; Roodman, forthcoming).

Cleary there is no panacea in analyzing N × T structured data. The Arellano-Bond dynamic panel data estimator has all of the notable limitations that apply to fixed effects models. A fixed effects analysis cannot conclude anything about the inter-unit effects of the independent variables, since such effects have been removed. Thus, the ability of this approach to remove the bias of relevant omitted variables relies on the assumption that those variables are fixed over time. And even when omitted variables are stable, discarding between-unit variation can yield standard errors that are considerably higher than those produced by methods that utilize both within- and between-unit variation (Allison, 2005). The methods we apply in this paper embrace the perspective that data analysis must depend both on the relevant theory and a variety of informed choices that seem most defensible given practical and logistical considerations.

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Table 1. Cross-Sectional Regressions of Certainty of Arrest and Robbery Rates

	Certainty of Arrest Model I		(log) Robbery Rate Model II		(log) Robbery Rate Model III	
	b	β	b	β	b	β
(log) Proactive Policing	<.001	.002	-.145*	-.141	-.124*	-.121
(log) Population	-.023*	-.153	.170*	.169	.159*	.158
Percent Divorced	-.004	-.065	-.018	-.047	.024	.063
Western Location	-.009	-.040	.230*	.150	.140	.091
Racial Inequality	-.016	-.058	.289*	.159	.317*	.174
Percent Non-Hispanic Black	-.001*	-.224	.016*	.372	.010*	.237
Median Income (in $1000s)[a]	.002*	.217	-.032*	-.505	-	-
Disadvantage Index	-	-	-	-	.406*	.536
Traditional Government Index	-	-	-	-	.061	.076
Percent Young Males	-	-	-	-	.280	.006
Percent Moved	-	-	-	-	.323	.025
Intercept	.563*	-	4.254*	-	2.199*	-
R^2	.217		.764		.780	
	N = 181		N = 181		N = 181	

*Statistically Significant for a Two-Tailed Test at the .05 Level

[a] Incorporated in the "Disadvantage Index" for Model III of Robbery Rates

Table 2. Panel data models estimated under the assumption that all predictor variables are exogenous to robbery rate at time t[z]

	Model IV[a] (log) Robbery Rate b	Model V[a, b] (log) Robbery Rate b	Model VI[c] (log) Robbery Rate b	Model VII[d, e] Δ Robbery Rate b	Model VIII[b, d, f] Robbery Rate b
Robbery Rate (lagged)	-	-	-	-	0.988*
(log) Proactive Policing	-0.088*	-0.078*	-0.039*	-0.617	-0.018
(log) Population	0.188*	0.174*	0.899*	-0.288	1.115
Divorce Rate	0.019*	0.024*	0.023	-0.844*	-1.781*
Western Location	0.300*	0.263*	-	11.148*	12.654*
Percent Racial Inequality	0.282*	0.293*	-0.494*	-8.884	-7.513
Percent Non-Hispanic Black	0.012*	0.013*	-0.003	0.653*	0.764*
Disadvantage	0.419*	0.423*	0.026	0.921	4.283
Traditional Government Index	0.067*	0.079*	-	3.840*	5.705*
Percent Young Males	-0.0077	0.0008	-0.237*	-0.954	-1.801
Percent Moved	0.000	0.004	-0.020*	0.069	0.397
Intercept	2.154*	1.933*	-2.169	18.945	1.791
rho	0.704	0.743	0.972	-	-
R^2	0.96	0.95	0.05	0.05	0.95
	N = 1180	N = 1035	N = 1180	N = 834	N = 1029

[z] Based on 180 panels (cities). Total number of observations in each model varies by lag structure and method of model estimation

[a] Prais-Winsten (iterative) estimation of regression coefficients with panel-corrected standard errors under the assumption that disturbances are heteroskedastic and common first-order autocorrelation (shown as rho) within panels

[b] all predictor variables are in one-year lags

[c] unit heterogeneity removed via fixed effects ($y^*_{i,t} = y_{i,t} - \bar{y}_i$; $x^*_{i,t} = x_{i,t} - \bar{x}_i$); effects of Western Location and Traditional Government Index are excluded due to no inter-unit variability

[d] OLS estimation of regression coefficients with panel-corrected standard errors under the assumption that disturbances are heteroskedastic (no autocorrelation)

[e] change-score (Δ) model wherein $y^* = y_t - y_{t-1}$ and proactive policing is first differenced ($pp^* = pp_t - pp_{t-1}$); all other predictor variables are in one-year lags

[f] static-score/regressor variable model

* indicates that the z-ratio is significant at p<0.05

Table 3. Cross-Lagged Model to Assess Reciprocity

	ΔRobbery Rate ΔY_2		ΔProactive Policing ΔX_2	
	b	β	b	β
Lagged Δ Proactive Policing ΔX_1	1.230	0.017	0.039	0.036
Lagged Δ Robbery Rate ΔY_1	0.159	0.122	0.003*	0.148
R^2	0.015		0.022	
ρ_1	-0.071			
$\rho_{u_1 u_2}$	-0.037			
	N = 180			

* indicates that the z-ratio is significant at $p<0.05$

ρ_1 is the estimated correlation between Δproactive policing and Δrobbery rates at wave 1

$\rho_{u_1 u_2}$ is the estimated correlation between the structural disturbances of the wave 2 equations

Table 4. Arellano-Bond dynamic panel-data estimation of effects of exogenous and endogenous predictors of change in robbery rates between time t and time t-1[a]

	Model XI[b]	
	b	long-run effect
Robbery Rate	0.138	0.862[c]
Proactive Policing	-21.851*	-25.339*
Population (000's)	-0.147	-0.170
Divorce Rate	-17.614	-20.426
Percent Racial Inequality	99.942	115.899
Percent Non-Hispanic Black	-2.691	-3.121
Disadvantage	-12.557	-14.562
Percent Young Males	-3.307	-3.835
Percent Moved	5.307	6.154
Certainty of Arrest	2.965	3.439
Intercept[d]	7.072*	---

[a] based on 172 panels (cities) contributing between 1 and 6 years (avg. observations/panel = 3.59) of non-missing observations. Total number of observations is 617.
[b] standard errors adjusted for within-unit heteroskedasticity
[c] coefficient of adjustment
[d] the estimated intercept is an estimate of the coefficient on a time trend; it is the average linear change in robbery rate and has no separate long-run function.