SHORT-TERM CHANGES IN ADULT ARREST RATES INFLUENCE LATER SHORT-TERM CHANGES IN SERIOUS MALE DELINQUENCY PREVALENCE: A TIME-DEPENDENT RELATIONSHIP*

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The impacts of quarterly adult arrest rates on later male serious delinquency prevalence rates were investigated in Philadelphia police districts (N = 23) over several years using all male delinquents aged 10–15 years who were mandated to more than "straight" probation. An ecological deterrence model expects more arrests to lead to less delinquency later. A community justice or mass incarceration model, the ecological version of general strain theory, and an ecologized version of the procedural justice model, each anticipates more arrests lead to more delinquency later. Investigating quarterly lags from 3 to 24 months between adult arrests and later delinquency, the results showed a time-dependent relationship. Models with short lags showed the negative relationship expected by ecological deterrence theory. Models with lags of about a year and a half showed the positive relationship expected by the other three theories. Indicators needed so future works can gauge the relative merits of each theoretical perspective more accurately are described. The spatial distributions of current and 1920s delinquency rates were compared.

Delinquency is a core conceptual concern within criminology with extensive and varied empirical and theoretical traditions. Qualitative contributions date back at least to Henry Mayhew's (1851) scrupulously detailed Dickensian accounts of street life in the poorest sections of London in the first half of the nineteenth century. His theory about the causes of wayward preteens and teens included bonds (Hirschi, 1969) with "parents, masters, and mistresses," and "associations formed in tender years"; patterns of differential association (Sutherland, Cressey, and Luckenbill, 1992) with street sellers—costermongers— "who live by street traffic and . . . train . . . children to a street life"; strains (Agnew, 1995) originating from "friendlessness, and utter destitution"; and dispositional differences (Lynam et al., 2000), including "vagrant dispositions and tastes on the part of children, which cause them to be runaways" (Mayhew, 1851: 468). Quantitative ecological work that relies on official data sources for delinquency outcomes in the era before self-report studies (Hindelang, Hirschi, and Weis, 1981; Short and Nye, 1957) has been connecting delinquency rates with community characteristics like relative socioeconomic standing for close to a century (Burt, 1925; Shaw and McKay, 1942). Arguments about the appropriate theoretical interpretation or the value of these quantitative ecological findings stretch back almost as far (Alihan, 1938; Baldwin, 1979; Gordon, 1967; Kornhauser, 1978; Thorndike, 1939). In the past quarter century, studies adding survey indicators have helped clarify processes linking community features to delinquency (Simcha-Fagan and Schwartz, 1986), delinquency precursors (Elliott et al., 1996), or the differential risks created by community features for varying types of adolescents (Wikström and Loeber, 2000). Despite the magnitude of previous ecological delinquency research and the surrounding, sometimes bitter, controversies, the current work seeks to extend that research in two ways.

First, the current investigation will build on work performed primarily by Bursik and colleagues using court-based delinquency data (Bursik, 1984, 1986a, 1986b, 1989; Bursik and Grasmick, 1993; Bursik and Webb, 1982; Heitgard and Bursik, 1987). These works have demonstrated the following: changes in the relative sociodemographic positions of communities over a decade link with concurrent delinquency changes; delinquency changes might be reflective of changes in adjoining communities; the delinquency change–structural change link depends on the specific decade; and delinquency rates not only reflect but also sometimes drive ecological changes. Instead of examining decade-long shifts in official delinquency rates, however, the current work focuses on shorter term changes: those taking place over months not decades. It asks whether these shorter term shifts in delinquency prevalence reflect local shifts in justice agency dynamics.

Because the time frame for examining delinquency changes used here is much shorter, it raises questions about the relevance of the structural and mediating social, organizational, and subcultural factors that have appeared as antecedents or covariates of delinquency rates or changes in those rates (Bursik and Grasmick, 1993). Factors such as relative socioeconomic status, relative stability, or informal supervision may, for a whole community, change too slowly to link to shorter term delinquency changes.

But what can change relatively quickly in communities are the actions of the justice agents and agencies who operate there, and the results of those actions. Whether in response to shifting citizen criminal activity or concerns, political imperatives, agency initiatives, or the cycling of agency supervisory and detention processes, communities can experience quickly changing arrest rates, incarceration rates, or ex-felon return rates. This research will focus on short-term changes in adult arrest rates as a potential determinant of short-term shifts in delinquency prevalence rates.

With its focus on short-term changes in adult arrest and delinquency prevalence, this study combines ecological delinquency research with a sizeable burgeoning literature concerned with the effects of criminal justice coercion. That integration extends the ecological delinquency research tradition in a second way.

Criminal justice coercion can be reflected in many different indicators. Examples include arrest rates, arrestee rates, imprisonment rates, or supervision rates. In addition to the researchers concerned about and documenting the adverse political (Manza and Uggen, 2006), economic (Western, 2002), and family (Western, 2006) consequences of high arrest and incarceration rates at the national level, other researchers and theorists working at lower levels of aggregation have explained how higher arrest, imprisonment, or supervision rates can impair social capital and socialization processes in communities (Clear, 2007; Rose and Clear, 1998). Short-term changes in community delinquency prevalence rates constitute an offending outcome of interest to these scholars.

In addition, these rates are also of interest to three other theoretical perspectives. The four relevant frameworks are outlined below.

FOUR FRAMEWORKS

The community justice framework—also called the coercive mobility or mass incarceration model—expects local disruptive impacts if communities experience large fractions of adults removed through imprisonment (Clear, 2007; Rose and Clear, 1998; Western, 2006). The relevant dynamics are complex—see Clear (2007: 73–91)—but the key idea is straightforward.

Up to a point, it helps a community to have criminals removed through arrest and later imprisonment. The most dangerous people are taken out of circulation for a time. If those rates go too high, however, then less-serious criminals are removed.

The latter removals disrupt families (Lopoo and Western, 2005) and reduce local social capital. Fewer adults are available to look after children, exercise local informal control, and represent the community externally. In short, making some simplifying assumptions about offending patterns; elevated crime rates increase removal rates; the latter, if they increase past a certain point, in turn will impair local social capital in households and communities. This weakening leads later to even higher crime rates (Clear, 2007: 85–7; Rose and Clear, 1998: figure 2).[1]

Interest in the community justice model has grown, driven in part by: historically high imprisonment and supervision rates for the entire country; recognition that larger numbers of lower income, predominantly minority urban communities in the United States have experienced extremely high rates of removal through arrest and incarceration in the last three to four decades; and an increasing awareness that most of those removed will return to their communities (Kubrin and Stewart, 2006; Lynch and Sabol, 2000; Visher and Travis, 2003).

1. Some of the work in this vein, given data limitations, uses local crime rates to stand in for local offending rates, despite the conceptual distinction (Baldwin and Bottoms, 1976).

Several empirical studies would seem to support the idea that high removal rates may drive crime rates even higher (Clear et al., 2003; Kane, 2006; Renauer et al., 2006). See Clear (2007: 158–66) for a discussion of relevant studies. These works, however, sometimes have failed to solve key analytic challenges. They have included nonrecursivity (Lynch and Sabol, 2000, 2004), endogeneity (Engle, Hendry, and Richard, 1983; Lynch and Sabol, 2004), and a failure to focus on subsequent unexpected changes in crime rates (Bohrnstedt, 1969). Furthermore, the underlying models lack conceptual clarity about the time lag between inappropriately high removal rates and the resulting elevations in crimes rates (Lynch and Sabol, 2000: 35–6). The lag question—how long does it take these processes to cycle?—is both theoretically and practically important.

In short, community-level dynamics relevant to the community justice model have been progressively elaborated by theorists, and empirical studies have provided some support. Nevertheless, some conceptual vagaries remain in these models, and few empirical studies have successfully addressed the most troublesome analytic challenges.

The conceptual "flip side" of the community justice model is probably more widely known among criminologists—ecological deterrence theory. This theory examines the "brighter" side of higher arrest rates and their ability to drive down subsequent offending or crime or arrest rates. The benefits accrue not just because offenders have been removed, but because, through various processes (Stafford and Warr, 1993), groups of potential offenders have been dissuaded for a time.

Deterrence theory is an enormously complex research area (Levitt, 2002), with significant conceptual and analytic challenges of its own (Cousineau, 1973; Gibbs, 1979). Temporarily putting aside these concerns, however, of key interest here is the timing of ecological deterrence impacts. How long does it take for higher arrest rates to produce an ecological deterrence effect? At the city level, a year may be too long a period (Chamlin et al., 1992; Greenberg, Kessler, and Logan, 1979), whereas lags of a day (D'Alessio and Stolzenberg, 1998), month, or quarter year (Chamlin et al., 1992) may be sufficient.

Extrapolating from this finding of time-dependent ecological deterrent impacts suggests two additional points. If adult arrests have deterrent impacts on later delinquency, then *those too* might be time dependent. Furthermore, the *adverse* impacts of arrests discussed by the community justice framework might *also* prove time dependent. The relevant community justice dynamics involving shifts in supervisory practices and/or affiliation patterns (Clear, 2007; Rose and Clear, 1998) may take a few months or more than a year before they demonstrably influence delinquency rates.

Beyond the two theoretical perspectives outlined so far, community justice and ecological deterrence, are two additional potentially relevant frameworks linking coercion and juvenile offending. One of these is the ecological version of general strain theory (GST) (Agnew, 1999). In this model, adult arrest rates are not proxies for adult removals as they are in the community justice model. Rather, the ecological GST simply postulates that having more adults arrested in the community may create more offending.

According to the ecological GST, for those in positive relationships with arrestees, higher arrest rates in a community represent more extensive "loss[es] of positive stimuli" (Agnew, 1999: 127). Those linked to arrestees may experience "economic deprivation" or "family disruption" even if arrestees are held in custody only briefly. Furthermore, arrests themselves, regardless of the outcomes, are likely experienced as "presentations of negative stimuli" (Agnew, 1999: 127) by those around the arrestee who were not threatened by him/her. Agnew (1999: 127) suggested that "exposure to negative stimuli [will] increase the likelihood that community residents will experience a range of negative emotions . . . [which] should have a direct effect on crime." As more and more preteens and teens have their fathers or mothers or older brothers or older sisters arrested and locked up, even if only briefly, that may affect the emotions of those teens and preteens and, perhaps, their chances of engaging in serious delinquent behavior. In line with Agnew's GST model, recent studies (Hoffmann, 2003; Hoffmann and Ireland, 2004) link strain at the individual, family, and community levels with individual-level delinquent outcomes, although the interplay between community and individual factors can be complex (Hoffmann, 2006).

The current study does not purport to include indicators of subjective strain, emotional reactions to strain, or a comprehensive set of objective strain indicators. It simply suggests that an elevating impact of adult arrest rates on later delinquency rates aligns with an ecological GST seeking to explain community differences in offending.

A final potentially relevant theoretical perspective is procedural justice (Tyler, 1990, 1998, 2003, 2004). If young males in a locale observe adults being arrested by local police in ways interpreted as unfair and/or unwarranted [for examples, see Simon and Burns (1997) and Venkatesh (2008)], then it may reduce their respect for the law and/or their willingness to obey the law. Community norms about the law, like the level of cynicism (Sampson and Bartusch, 1998), vary ecologically. Police actions witnessed by community residents contribute to those norms (Anderson, 1999). If preteens and teens in a community witness an increasing volume of unjustified arrests or unfair police practices (e.g. Brunson, 2007), then it may deepen cynicism about the law and strengthen norms favoring offending.

The procedural justice argument invokes a different social dynamic than does the community justice thesis. The former envisions key local attitudes and norms shifting among youth because they witnessed or heard about procedurally unjust interactions with police, perhaps during or after arrest. Community justice, in contrast, focuses on abraded social dynamics in local primary and secondary groupings as a result of removals through arrest and/or incarceration. Although the procedural justice model is silent on how long it takes for views about police fairness to shift, some work has demonstrated that regional views about a large police department can change dramatically in a short time in response to high-profile events (Weitzer, 2002). Thus, within-community shifts in views toward police taking place in a few months, based on events observed or heard about, seem plausible.

In sum, four theoretical perspectives expect short-term impacts of changes in criminal justice coercion operationalized here as adult arrest rates on later changes in offending, operationalized here as male serious delinquency prevalence rates. Three expect a positive relationship: community justice because local social controls in the families and neighborhoods become abraded; ecological GST because these events are collectively experienced as negative events and set in motion an array of stress-related dynamics across segments of the population in the locale; and an ecological version of procedural justice because respect in a locale for law and law enforcement may decline across segments of the population if many of arrests are viewed as unwarranted and/or carried out in a disrespectful manner. One perspective, deterrence, anticipates a negative relationship. Through direct witness or other social learning processes (Stafford and Warr, 1993), more arrests should result in more collective caution about engaging in behaviors likely to lead to being apprehended as a serious delinquent.

The current work does not seek to test the relative merits of each relevant perspective. Rather, it seeks simply to examine this relationship and observe its directionality and timing. The researchers are not aware of other current research that has tested this relationship.

DATA SOURCES AND METHODS

UNIT OF ANALYSIS

Philadelphia police districts ($N = 23$) excluding Fairmount Park and the Airport served as the ecological units of analysis. The arrest data for the period were available only at this level. These districts have substantial historical tradition. They were originally aligned with city election wards (Schmidt, 1987). Individual wards and groupings of smaller wards were previously used in Philadelphia delinquency research in the 1920s (Shaw and McKay, 1942). In 2000, the average population of these 23 districts was about 65,000; this figure is very close to the average population of

Chicago's 75 "natural areas" in 1940, which was around 66,000. The latter natural areas have been widely used in delinquency research (Bursik and Grasmick, 1993). Beyond aligning closely with comparably sized units used in at least one other city for delinquency research, these units provide a second advantage for current purposes. Because the districts are sizable, they permit constructing quarterly serious male delinquency prevalence counts such that the distribution of counts does not accumulate an extremely large number of zeros.

None of the four relevant theoretical perspectives described specify the size of spatial arenas within which the described dynamics operate. Although it is likely that different-sized relationships might emerge in different-sized spatial units, which is part of the modifiable areal unit problem (Chainey and Ratcliffe, 2005) and the related aggregation problem (Hannan, 1971), and is indeed a worthwhile external validity topic to investigate, police districts as the unit of analysis are not precluded by any of these theoretical orientations.

INSTITUTIONAL SETTING AND OUTCOME VARIABLE

Because every state has a different juvenile justice system (King, 2006), the context for processing juveniles in Pennsylvania and in Philadelphia is described here (Pennsylvania Juvenile Court Judges' Commission, 2006).

The juvenile nonarrest rate, historically, has been much lower in Philadelphia than in other large U.S. cities (Monahan, 1960). "Hence, in Philadelphia, as compared to other areas of the country, a much higher proportion of allegedly delinquent children receive treatment by the court, and the information on these cases approaches a completeness and representativeness as regards all children apprehended in the commission of delinquent acts" (Monahan, 1961: 257–8).

Juveniles are charged by attorneys in the Philadelphia District Attorney's charging unit, which handles both adults and juveniles. Juveniles may be physically transferred to the secure Youth Studies Center if the offense is serious and they are 12 years or older, placed under supervision, placed in a program, or sent home. Cases move forward through a Family Division called "Family Court" that is part of the general jurisdiction Court of Common Pleas in Pennsylvania (National Center for Juvenile Justice, 2006). Within the division are branches for juveniles and domestic matters (First Judicial District of Pennsylvania, 2006). In addition to processing delinquencies, the Juvenile Branch also processes cases of abuse, neglect, dependency, and truancy.

Juvenile probation officers working at an intake unit at the Youth Study Center, which is a secure county detention facility operated by the Philadelphia Department of Human Services (DHS) (Philadelphia Department of Human Services, 2008), are the initial key point of contact for new delinquent petitions (Philadelphia Courts First Judicial District of Pennsylvania, 2008). These officers at the unit perform many functions, including liaising with police, prosecutors, and DHS; appointing defense counsel; and providing support for the delinquency hearing (Philadelphia Courts First Judicial District of Pennsylvania, 2008).

DHS contracts with private service providers and, in collaboration with Family Court and the Office of the Public Defender in Philadelphia, monitors conditions in facilities and programs. In Philadelphia, as elsewhere in the state, detention facilities at all security levels and programs run by counties or by the state, as well as privately run programs, are available for delinquents who require more than juvenile probation (National Center for Juvenile Justice, 2006).

Pennsylvania operates under a Balanced and Restorative Justice model, and this is reflected in the disposition by the Philadelphia Juvenile Court of male juvenile cases in 2000 (N = 7,432). The most frequent to least frequent outcomes were as follows: withdrawn or dismissed (40.4 percent), probation (23.2 percent), placement (15.4 percent), other (10 percent), consent decree (8.1 percent), referral (1.5 percent), informal (1.1 percent), and transfer to criminal court (.3 percent). These proportions of nondismissed cases that receive program placement (26 percent) or probation (39 percent) seem close to those reported for male felony-charged juveniles in other systems (Holsinger and Latessa, 1999).[2]

In Philadelphia, the 2000 figures seem to be pretty typical for the study period 1996–2002. For those years, the number of cases disposed averaged 6,630 [standard deviation (SD) = 1,051], the number of cases withdrawn or dismissed averaged 2,368 (SD = 632), the number of cases resulting in placements averaged 1,058 (SD = 155), and the percentage of nonwithdrawn/nondismissed cases resulting in a placement averaged 25 percent (SD = 2.8 percent).[3]

The current analyses will focus only on serious male delinquents, who experienced their first arrest and were aged 10–15 years at the time of arrest. The reasoning for each of these restrictions is explained below.

2. Holsinger and Latessa (1999) using data from an un-named "medium sized county in a midwestern state" examined dispositions for a 2-year random sample from 1995 to 1996 of juveniles adjudicated for felonies. Their data for males showed (authors' calculations from their table 1) that the percent of males who received "straight" probation was 51 percent, and the percent receiving a program assignment—in their table either "Program" or "Department of Youth Services"—was 32 percent.

3. These values represent the authors' calculations from NCJJ Philadelphia County data.

"Serious" means those adjudicated delinquent and placed in a program. "Program" here refers to secure correctional facilities that are run either by the City of Philadelphia or by the state, as well as residential and community programs; i.e., every nondismissed, nontransferred case that received a disposition of more than "straight" probation (supervision only).[4] Available data covered only program-mandated delinquents. For this group, however, data were complete for 1996–2002.[5]

The restriction to "serious" delinquents fits with the four theoretical perspectives described above. Each of the four perspectives described above anticipates juveniles being more or less likely to be involved in *serious* criminal activity as a result of antecedent conditions. The seriousness of the charge was the main factor that shaped whether first-time juveniles adjudicated delinquent received probation (supervision only) or were mandated to treatment (Fader et al., 2001).[6]

Because serious delinquents—first time and repeaters both—were about 90 percent male, these analyses used only males. Past research in Philadelphia (Monahan, 1957: 257) and elsewhere has commented on the different offenses committed by male and female delinquents (Moffitt et al., 2001; Schneider, 1992) as well as the rate differences by gender (Hagan, Simpson, and Gillis, 1987). The four perspectives described may apply to both boys and girls, but this initial test is limited only to boys

4. Transfers to adult criminal court occur only in small numbers in Philadelphia. For example, in 2000, there were only 20 transfers (.2 percent of all cases disposed, .4 percent of all nonwithdrawn/nondismissed cases) (National Center for Juvenile Justice, 2008).

5. Delinquency data were first collected in 1994, and the collection continued through 2004. The figures were complete, however, only for the years 1996 through 2002. There were over 17,000 first-time delinquents over the entire 11 (1994-2004) years, and 97.3 percent of those were successfully geocoded.

6. For first-time delinquents, it is not denied that extralegal factors such as race might play a role in the decision of whether the juvenile received probation. There has been a lengthy and interesting discussion about the roles of those factors (Fader et al., 2001). Nevertheless, the roles of those extralegal factors in the assign probation decision are not problematic for the current analyses. Because all delinquents came from one juvenile justice system, and because the analyses control for differences across units of analysis (see below), the only way that the impacts of extralegal factors on the assigned probation decision could create a problem would be if one wished to make the case that 1) these impacts operated differentially across the units of analysis—police districts, 2) the impacts of those biasing factors were changing over time, *and* 3) how those impacts were changing was markedly different across districts. One must postulate a district × time interaction term for the biasing impact of extralegal factors on the probation decision. Although such a postulate is possible, the authors are unaware of any previous research on extralegal factors and first-time delinquent decision making around probation that would make such a postulate plausible.

given the complex relationship among gender and delinquency pathways, and gender and delinquency rates.

The sample is limited only to first-time offenders for two reasons. Most importantly, it removes the impacts of prior criminal justice processing on later delinquency. How juveniles behave (Hindelang, Hirschi, and Weis, 1981) and how they are later treated by the juvenile justice system (Holsinger and Latessa, 1999) are both influenced by the number of, and outcomes originating from, previous arrests. Prior arrests and prior arrest outcomes become additional complicating factors. Second, and perhaps more importantly, it seems that all the theoretical perspectives described above speak specifically to processes leading to initial serious delinquency rather than to delinquency reengagement. This observation is clearest in the community justice framework, which speaks to the failure of socialization processes and informal local controls.

One should not presume that first-time delinquents mandated to programs were not involved in serious offenses. They were. Among all first-arrest Philadelphia delinquents for 1998–1999 placed in programs, 77 percent of these delinquents scored in the three highest categories of a five-category offense-severity scale (Bishop and Frazier, 1988; Fader et al., 2001).[7]

Finally, why the restriction to the ages of 10–15? Most importantly, this restriction addresses the nonrecursivity problem (McClendon, 1994: 314–8) originating from the bidirectional relationship between crime rates and removal rates. Because the models examine temporal lags only up to 2 years, delinquents counted earlier cannot contribute to later adult arrest rates. With lags of up to 2 years, it is only by restricting the analysis to juveniles aged 10–15 years at the time of their first arrest that we can be confident we have addressed nonrecursivity in the main analysis and the model checks.

Second, this age restriction would seem more congruent with at least some of the theoretical perspectives described. Although peers contribute significantly to delinquent engagement (Chung and Steinberg, 2006; Rutter and Giller, 1984), parental monitoring knowledge of adolescents in their early teen years (12–15) links strongly to current and future delinquency (Lahey et al., 2008). The preteen and early teen years are pivotal for potential parental or adult influences (Anderson, 1999; Seidman et al., 1998). Because the community justice framework describes how removal of parental and supervisory figures may impair youth socialization, and

7. The three most serious categories in Bishop and Frazier's (1988) scale were felony person, felony property, and felony public order. The DHS did not authorize the release of this individual-level information to us for the purposes of this research.

because research has clearly demonstrated the strong impacts of parental supervision when teens are in the 12–15 age range, the age restriction aligns with this theoretical perspective.

With this age restriction, were we missing the most delinquent fractions of the delinquent population? No, because we were also concentrating on first-arrest delinquents and on prevalence rates. It is true of course, in Philadelphia and elsewhere, that incidence rates are higher for older as compared with younger delinquents. Incidence rates, however, were not the focus here; prevalence rates were. Furthermore, in the last quarter century, nationally, younger delinquents have increased their rate of serious delinquent involvement. “Violent and drug arrest rates rose for young juveniles [10–13] from 1980-2003” (Snyder and Sickmund, 2006: 130). In keeping with this national picture, analyses of a risk classification score developed using only system information available at the time of disposition (Jones et al., 2000) showed no significant differences between all Philadelphia first-arrest male juveniles in the database aged 10–15 years (inclusive) (mean risk classification score = 79.98, n = 6,524) and those arrested for the first time at the ages of 16 or 17 years (mean = 78.90, n = 6,765) ($p > .09$). Among male *first-time* arrestees, using an instrument taking into account seriousness of arrest charge, those individuals aged 10–15 years were equivalent to those aged 16–17 years. Finally, restricting the ages to 10–15 years permitted comparing rates developed with those from the 1920s.

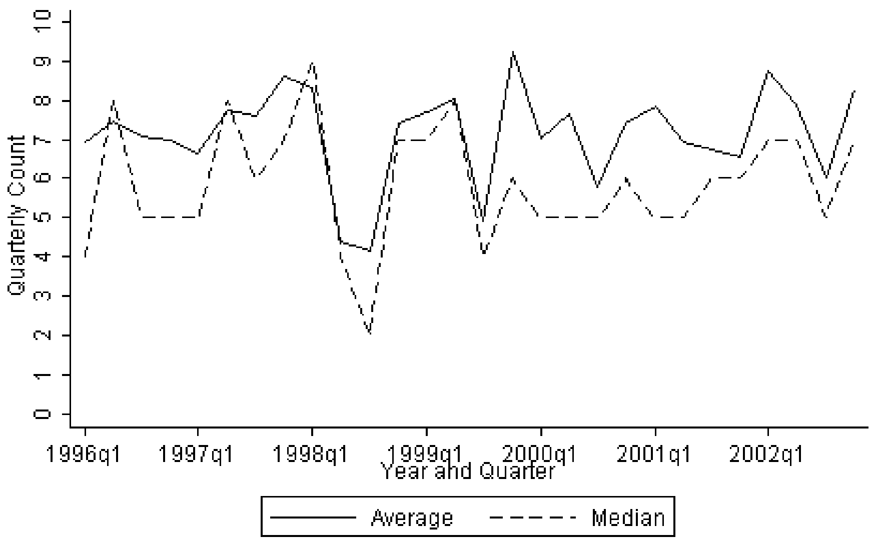
The number of zero counts in a quarter did not seem inflated. Across 644 observations (23 districts × 7 years × 4 quarters), there were only 44 zero-count observations (6.9 percent) if male delinquents only aged 10–15 years were counted and only 10 (1.6 percent) if males of all ages were counted. About 65 percent of the variation in these delinquency counts was between districts, and the rest was within districts, over time.

Figure 1 shows, by quarter and across police districts, the median and average male first-arrest delinquency counts for 10–15-year-olds from 1996 to 2002. Each district’s count was weighted by the square root of its estimated population of 10–15-year-old males for that quarter.[8] Both the average and median counts trended upward from early 1996 through late 1997, dipped sharply in early 1998, and then rose again in late 1998 before dipping again. From the middle of 1999 through the end of the period, the median remained relatively stable until late 2001, whereas the average, after rising in late 1999, trended downward through 2000 and the first half

8. Using 1990 and 2000 census figures for population, by each individual year of age and by sex, the number of total males 10–15 in each district was estimated for each year via linear interpolation and extended forward through 2002. Within a year within a district, the number was presumed equal for each quarter.

of 2001 before rising (late 2001), falling (early 2002), and then rising again at the end of the series.

Figure 1. Philadelphia Delinquency Prevalence Counts 1996–2002: By Quarter Year, across Police Districts



NOTES: Male serious delinquency prevalence counts, shown by quarter, ages 10-15. Weighted data. Police district = unit of spatial aggregation.

ARREST INDICATORS AND LAGS

Monthly total adult arrest counts (male and female) by police district were obtained for calendar years 1996 through 1999. These counts were postcharge; initially unsubstantiated arrests were not included. These were converted to adult rates per 100,000 population aged 18 years and older and calculated for each quarter year.[9] Thus, there were 16 quarters with both arrest and delinquency data. As the lag increased, the arrest quarters were matched with later delinquency quarters.

As a form of sensitivity analysis, four alternative operationalizations of arrest rates were generated. Before using them in the analyses, these rates

9. The adult population for each district for each year was calculated using linear interpolation from 1990 and 2000 census figures for the population aged 18 years and older, aggregated to police districts.

were centered so scores represented deviations around the respective district averages (see appendix A for rationale). An "average" indicator took the monthly average arrest rate, over each quarter. To obtain a yearly equivalent, the rate was then multiplied by 12 (variable = arrtavyr; dv_avyr after centering). A smoothed version of the average quarterly arrest rate expressed in yearly terms also was constructed and tested (variable = aravyr3 and dv_avyr3 after centering). A "maximum" indicator looked at each quarter, retained the maximum monthly arrest rate over the 3 months in each quarter, and then multiplied it by 12 to obtain a yearly equivalent (variable = arrtmxyr and dv_mxyr after centering) rate for the quarter. A smoothed version of the latter, using a three-quarter moving window, also was constructed (variable = armxyr3 and dv_mxyr3 after centering).[10] The indicators using the smoothed and unsmoothed maxima capture the "deepest" cut into the local social networks (community justice), the greatest strain (GST), the maximal possibilities for unjust incidents taking place (procedural justice), or the most deterrence in a quarter. The results will be shown for each centered operationalization. The descriptive statistics for the four operationalizations before and after centering appear in table 1.

Lagged arrest rates were created for lags of up to eight quarters (2 years), pairing earlier arrest rates with later delinquency counts. As described, only the deterrence perspective has investigated different lag periods. One implication of that work is that lags from one quarter to less than a year may be appropriate intervals in which deterrent effects might emerge. If socialization processes like those envisioned by the community justice thesis are operating, then one might expect increased delinquency anywhere from a few months to several months later. Too little is known about either the relevant ecological strain or the procedural justice models to estimate when their impacts should emerge. Exploring lags that range from one to eight quarters may help to clarify the time dependency of arrest-delinquency links.

Even with a lag of eight quarters, if there was no smoothing, 16 quarters of delinquency and arrest data were available. With smoothing, 14 quarters were still available.

The arrest rates refer to different underlying constructs depending on the theoretical framework used. For the community justice thesis, they are only a proxy for removal rates. What do we know about how many arrestees were removed and for how long?

10. The smoothing "loses" one quarter at the beginning and end of the series. Thus, some differences are expected between analyses using smoothed and unsmoothed arrest figures.

Only a fraction of felony arrestees were removed for a substantial period because of subsequent jail or prison convictions. For 2002 (Cohen and Reaves, 2006), roughly 39 percent of Philadelphia arrestees who became felony defendants were convicted (Cohen and Reaves, 2006, appendix table G), and of those convicted, 67 percent were incarcerated in jail or prison (Cohen and Reaves, 2006, appendix table H). So at least about one in four (26.1 percent) of adult arrestees who became felony defendants experienced subsequent removal for at least a year. Even before case resolution, arrestees may be removed for a substantial period; 21 percent of felony defendants were held in custody pending disposition, and 23 percent of felony defendants' cases were not resolved within a year.

For the ecological GST, however, arrests are not proxies for removal but instead are themselves immediately problematic. Even if the arrest is for a minor charge, the arrestee experiences the stigma and associated strain of the arrest itself, as do those around him or her. The impacts of the strain on the anger and frustration of those linked to the arrestee will depend on a host of factors. Postarrest obligations to justice agencies, including potential pretrial restrictions, can have adverse economic, social, or family impacts for those around the arrestee.

For an ecologized procedural justice model, arrests can be criminogenic if those witnessing or hearing about the arrests, or subsequent treatment of arrestees, view arrests or later treatment as either procedurally unfair or disrespectful. All else equal, in this framework, higher arrest rates make it more likely that others linked to arrestees will view the criminal justice system, and laws, more negatively.

Finally, for the deterrence perspective, arrests represent just one type of criminal justice coercion experienced by the community. All else equal, arrests might increase general deterrence in the area through a range of localized processes.

Although the arrest figures were based on the location where the arrest took place rather than the residence of the arrestee, we maintain that this is not problematic for two reasons. First, as is well known, most offenders for most crimes travel only short distances (Rengert, Piquero, and Jones, 1999; Rossmo, 2000). So, if offenders start the journey to crime from their residences, which they often do (Rengert, 1980; Rengert and Wasilchick, 1985), the offense site will be close by. "An empirical regularity that has emerged from journey-to-crime research is that offenders select targets (or victims) that are located near their residence" (Snook et al., 2006: 219). This observation holds across many crime types. For example, a recent analysis of a sample of urban robbers found that the median distance between robbers and their homes was .43 miles (min = .07, max = 1.55) and that three quarters of the robbers lived within .62 miles of their crime scenes (Snook et al., 2006: 224). In Philadelphia, police districts average

5.4 square miles. Some are more than (the 7th and 8th), or close to (the 14th and 5th) 10 square miles. Even some of the smallest districts, like the 22nd and 23rd, are 2 miles in length (East to West) and are close to a mile in height (North to South). Given the consistent finding of strong distance decay effects, over many crimes with offenders in many types of places (Rengert, Piquero, and Jones, 1999), combined with the substantial area of Philadelphia's police districts, we maintain that many of those arrested in a police district reside in that same district, although we cannot specify that proportion.[11]

EXPOSURE VARIABLE, TRENDS, SEASONALITY, AND INCIDENTAL PARAMETERS

The yearly estimated number of 10–15-year-old males from census linear interpolation (see footnote 8) became the exposure variable used in the analyses. Leaving the winter quarter (January through March) as the reference string, three dummy variables captured seasonal variation (April–June, July–September, and October–December) and were included in all analyses.

To gauge temporal trends in preliminary generalized multilevel models, three parameters were tested while controlling for exposure and seasonality: a linear trend, an orthogonal quadratic trend, and an orthogonal cubic trend. The data pattern and statistical tests confirmed that none of these trend variables were significant, either alone, together, or in any possible two-way combination. Each trend variable was allowed to have varying impacts across districts. There was no significant variation in any of the slopes, which suggests no significant departures from the average slope for any of these temporal trend indicators. Despite their nonsignificance, to isolate naturally occurring temporal trends, two temporal parameters, linear and quadratic, were retained in final models.

Twenty-two dummy variable incidental parameters, corresponding to each district save the 39th, also were included in each model. Although the correlation between each of these incidental parameters and the deviation scored arrest parameters is 0, including them allowed for more precise estimation of the seasonal and temporal parameters; see appendix A.

11. Unless one is willing to maintain that for the arrest rate variable: 1) the percent of arrestees residing in the district where the arrest took place changed markedly over time, *and* 2) in different ways for different districts during the course of the study, the arrest variable is not biased. We know of no reason to argue for these two points simultaneously. Indeed, a case can be made that the impact observed here for arrest rates represents a *conservative* lower bound of what would be observed if arrestees rather than arrest rates were used.

ANALYSIS PLAN

Data were analyzed using cross-sectional (panel data) time series for negative binomial models with fixed effects for district-specific heterogeneity on the outcome (Allison and Waterman, 2002; Hausman, Hall, and Griliches, 1984). These models present some complexities, which are described in appendix A.

Incidence rate ratios (IRRs) appear in the results. These values indicate the factor by which an expected mean count changes given a unit change in the predictor. For example, with an arrest rate per 100,000 adults 18 years and older, an IRR of 1.000020 would suggest an increase in the mean count of delinquents by a factor of .2 (20 percent) for an increase of 10,000 in the arrest rate per 100,000 (or, alternatively, an increase of 10 per 100). Table 1 shows descriptive statistics for the variables used in the models and the uncentered arrest variables (weighted data).

RESULTS

CHANGES IN DELINQUENT COUNTS

Negative Binomial Models

Table 2 shows the results for four different operationalizations of arrest rates. The four different operationalizations presented consistent patterns across the eight different lags ranging from arrest preceding delinquency by one quarter of a year up to eight quarters. For *all* versions of the arrest rates, the impacts on delinquency 1) were initially in the negative direction expected by deterrence; 2) increased monotonically between lags of two and five or six quarters, which suggests a clear time dependency of the relationship; and 3) ended up in the positive direction expected by the other three theoretical perspectives.

With a lag of just one quarter, higher arrest rates earlier were associated with *fewer* first-time delinquents later. These impacts, in line with the deterrence perspective, proved significant ($p < .05$) for both the maximum (dv_mxyr) and the average (dv_avyr) unsmoothed arrest models. How sizable were these deterrent impacts? Using the average arrest rate (dv_avyr), a two SD increase in the rate ($2 \times 1{,}615$) would be associated with a 14 percent decrease in the expected average quarterly delinquency count $[(1 - .999956) \times 3{,}230 = .142]$.

As noted, with widening temporal gaps between earlier arrest and later delinquency, negative arrest impacts faded and positive impacts emerged; all arrest IRRs increased monotonically as the lag lengthened from two to five or six quarters. Consequently, when delinquency counts reflected a period at least 18 months after the arrest variable (lag ≥ six quarters), at least one significant *positive* impact of earlier arrest on later delinquency

Table 1. Descriptive Statistics

	Variable	Variable Label	N	Mean	SD	Minimum	Maximum
Outcome	Number of male delinquents aged 10–15 years	nmdel	644	19.37	14.72	.00	78.00
Exposure	Number males aged 10–15 years	nml1015	644	3,512.85	1,848.78	294.20	7,917.40
Predictors	Seasonal dummy: April–June	q2dum	644	.25	.43	.00	1.00
	Seasonal dummy: July–September	q3dum	644	.25	.43	.00	1.00
	Seasonal dummy: October–December	q4dum	644	.25	.43	.00	1.00
	Time: Linear trend	seq96lin	644	14.50	8.08	1.00	28.00
	Time: Quadratic trend centered	seq96quc	644	65.50	58.81	.00	196.00
	Adult arrest rate, per year, based on quarterly maximum	arrtmxyr	368	7,357.31	4,519.33	1,088.20	24,750.90
	Adult arrest rate, per year, based on quarterly maximum, smoothed	armxyr3	322	7,386.32	4,400.82	1,240.32	24,100.93
	Adult arrest rate, per year, based on quarterly average	arrtavyr	368	6,642.15	4,092.29	965.34	24,074.93
	Adult arrest rate, per year, based on quarterly average, smoothed	aravyr3	322	6,659.05	3,979.76	1,064.80	23,040.75
	Adult arrest rate, per year, based on quarterly average, centered by district average	dv_avyr	368	.00	1,615.06	–5,771.67	9,159.01
	Adult arrest rate, per year, based on quarterly maximum, centered by district average	dv_mxyr	368	.00	1,826.17	–6,166.89	8,467.87
	Adult arrest rate, per year, based on quarterly average, smoothed, centered by district average	dv_avyr3	322	.00	1,330.49	–4,724.77	8,428.02
	Adult arrest rate, per year, based on quarterly maximum, smoothed, centered by district average	dv_mxyr3	322	.00	1,513.63	–4,837.37	8,086.12

NOTES: Data are organized by police district (N = 23) and by quarter year. The data are weighted by the square root of the number of males aged 10–15 years for each quarter in each district. Period = 1996–2002 for delinquency data and 1996–1999 for arrest data. Uncentered arrest variables not used in analyses are shown here for descriptive purposes only.

Table 2. Impacts of Adult Arrest Rates on Later Initial Male Delinquency Counts

		IRR (b(e))	SE	z	$p <$	95% Confidence Interval Lower	Upper
Adult Arrest Rate Variable	**Temporal Lag (in Quarters)**						
dv_mxyr	**Adult arrest rate, per year, based on quarterly maximum**						
	L1.	.9999613	.000017	−2.32	.05	.9999286	0.999994
	L2.	.9999901	.000017	−0.60	ns	.9999578	1.000022
	L3.	.9999969	.000017	−0.18	ns	.999964	1.00003
	L4.	.9999997	.000017	−0.02	ns	.9999669	1.000033
	L5.	1.000012	.000015	0.77	ns	.9999821	1.000041
	L6.	1.00003	.000016	1.85	.10	.9999983	1.000063
	L7.	1.000028	.000014	1.99	.05	1	1.000055
	L8.	1.000015	.000014	1.11	ns	.9999881	1.000043
dv_mxyr3	**Adult arrest rate, per year, based on quarterly maximum, smoothed**						
	L1.	.9999796	.000022	−0.95	ns	.9999375	1.000022
	L2.	.9999677	.000023	−1.38	ns	.9999218	1.000014
	L3.	.9999863	.000022	−0.61	ns	.9999426	1.00003
	L4.	.9999989	.000022	−0.05	ns	.9999555	1.000042
	L5.	1.000034	.000023	1.52	ns	.99999	1.000079
	L6.	1.000037	.000018	2.00	.05	1.000001	1.000072
	L7.	1.000033	.000019	1.74	.10	.9999958	1.00007
	L8.	1.000016	.000020	0.83	ns	.9999777	1.000055
dv_avyr	**Adult arrest rate, per year, based on quarterly average**						
	L1.	.999956	.000019	−2.32	.05	.9999186	.9999932
	L2.	.999990	.000018	−0.56	ns	.9999538	1.000026
	L3.	.999993	.000020	−0.36	ns	.9999536	1.000032
	L4.	1	.000020	0.00	ns	.9999613	1.000039
	L5.	1	.000018	0.00	ns	.9999654	1.000035
	L6.	1.000027	.000019	1.44	ns	.9999903	1.000063
	L7.	1.000033	.000016	2.06	.05	1.000002	1.000064
	L8.	1.000026	.000016	1.58	ns	.9999939	1.000058

Table 2 (continued).

dv_avyr3	**Adult arrest rate, per year, based on quarterly average, smoothed**						
	L1.	.9999736	.000025	–1.07	ns	.9999254	1.000022
	L2.	.9999597	.000027	–1.5	ns	.999907	1.000012
	L3.	.9999918	.000026	–0.31	ns	.9999406	1.000043
	L4.	.9999915	.000026	–0.33	ns	.9999406	1.000043
	L5.	1.000028	.000026	1.05	ns	.9999762	1.000079
	L6.	1.000034	.000021	1.62	ns	.9999928	1.000076
	L7.	1.000043	.000023	1.89	.10	.9999984	1.000089
	L8.	1.000038	.000024	1.58	ns	.9999909	1.000085

NOTES: Cross-sectional time-series negative binomial models with fixed effects. Outcome = number of first-time delinquent boys, aged 10–15 years. The data are organized by police district (N = 23) and by quarter year. Lag indicates by how many quarters the adult arrest rates precede the delinquency counts. Other predictors included in the model but not shown are as follows: three seasonal dummies, linear trend for time, quadratic trend for time, and exposure variable (number of males aged 10–15 years). All models also include 22 unit-specific dummy variables (incidental parameters), except for dy_avyr3 (lag = 7 quarters). That model with incidental parameters did not converge, which is not an unusual occurrence (Allison and Waterman, 2002), so they were removed. All adult arrest rates centered around respective district average. n ranges from 322 to 368 district quarters depending on the arrest variable.
ABBREVIATIONS: ns = not significant; SE = standard error.

emerged for three of the four arrest operationalizations. For the fourth, the smoothed average arrest indicator (dv_avyr3), only marginal significance ($p < .06$) was reached for the positive IRRs.

An additional consistency across operationalizations was when the strongest delinquency elevating impacts of adult arrests surfaced: It was always after arrests were lagged by six or seven quarters.

In sum, across three versions of the arrest variable, at roughly a year and a half after arrests were captured, first-time delinquency rates were significantly elevated by earlier adult arrests; and for a fourth version, the impact was almost significant.

How sizable were the strongest impacts of adult arrest rates on later delinquency? It seemed that a two SD increase in the relevant centered arrest rate could generate about an 11 to 12 percent increase a year and a half later in the average expected quarterly delinquency counts.

Therefore, all four operationalizations of the arrest rate demonstrated several consistencies in these models. First, with a short lag of just one quarter of a year, earlier arrest rates linked negatively with later delinquency counts as predicted by the deterrence perspective. With the two nonsmoothed arrest variables, these impacts were significant. Second, for all four operationalizations of the arrest rate, as the lag between earlier adult arrest rates and later delinquency counts increased, the relationship between the two shifted, with impact parameters increasing monotonically between lags of two and five or six quarters. Third, consequently, with all four operationalizations, a significant (3) or near-significant (1) positive impact of earlier arrest on later delinquency emerged after at least a year and a half.

These results were obtained under relatively stringent analytic conditions: Linear and quadratic temporal trends as well as seasonality were all controlled; numbers of males in the relevant age groups were controlled through an exposure variable; district-to-district variation in mean delinquency counts was controlled by introducing incidental parameters for (n–1) districts and by district-centering arrest rates; and district variations in overdispersion of the counts were controlled via the parameters introduced by the fixed-effects negative binomial panel models in Stata (StataCorp, College Station, TX). The results support the deterrence perspective when the gap between arrest and later delinquency is short, and they support the three perspectives expecting more delinquency later when the gap is at least a year and a half.

The only other parameter in the models proving consistently significant was the dummy variable reflecting delinquency in the third quarter (July through September). Delinquency counts were always significantly lower in this period (results not shown). IRRs were between .61 and .84 in the various models, which suggests that, compared with the average delinquency counts from January through March, the third quarter expected average delinquency counts were about 39 to 16 percent lower. In some models, linear or quadratic trend parameters were significant (results not shown).

Graphical Inspection

Hoping to increase confidence in the observed significant lagged impacts of arrest on later delinquency, line graphs were constructed for the 10 districts with the highest average delinquency counts, because they were least likely to have delinquency counts of 0 in successive quarters, and for four other randomly selected districts. Two line graphs were constructed for each of these 14 districts. Both graphs for each district had time on the *x*-axis, in quarters, and the district's count of serious first time male delinquents for that quarter on the first *y*-axis. The two series differed in what was displayed on the second *y*-axis. All graphs used the centered average arrest rate variable (dv_avyr).

In one series, the second *y*-axis showed the centered average arrest rate lined up with the delinquency rate for the subsequent quarter, which corresponded to a lag of one quarter. The above results had shown the strongest negative impact of arrest on later delinquency for this lag for this outcome. For each district, the instances were counted and summed where, from one quarter to the next, the delinquency count and the corresponding lagged arrest rate moved in opposite directions simultaneously (on the graph) as expected by deterrence. Periods where there was no change in either variable were ignored.

In the second graph series, the second *y*-axis displayed the corresponding score for the average arrest rate from seven quarters earlier, the lag where the results had shown the strongest elevating impact of arrest on later delinquency. For each district, the instances were counted and summed in which the delinquency count and corresponding lagged arrest rate both moved in the same direction simultaneously (on the graph), as expected by the other three perspectives.

In each graph in each district, because simultaneous changes were counted and summed, not data points, 15 simultaneous changes were examined.

Examining the 10 districts with the highest delinquency counts, the graphs with the arrest rate lagged by one quarter showed simultaneous changes in the opposite direction, on average, in 7.7 out of 15 change periods. Lagged arrests went up, and delinquency counts simultaneously went down, or the reverse, anywhere from 4 to 11 times across these 10 districts. In four other randomly chosen districts, the average number of times changes went in the opposite direction was 8.75 (max for a district = 12; min = 5) out of 15 change periods.

Turning to the second series where the arrest rate from seven quarters earlier was matched with delinquency counts, for the 10 highest delinquency count districts, arrest and delinquency changes shifted simultaneously in the same direction for 7.3 change periods on average out of 15 (max = 10; min = 5). For the four other randomly chosen districts, the average number of periods when changes went in the same direction was 6.75 (max = 8; min = 5).

For both graph series, those with arrests lagged by one quarter and those with arrests lagged by seven quarters, different change periods lined up theoretically in different districts. In other words, for neither the lag-1 nor the lag-7 series did the theoretically corresponding changes originate from the same segment of the data series across districts.

This graphical inspection on a district-by-district basis, of the delinquency counts by quarter with the appropriately lagged arrest rates suggested, at least for this one operationalization of the arrest rate, that the negative bivariate relationship between these two variables with a short lag, and the positive bivariate relationship with a long lag, were both frequently visually apparent. It also suggested that the negative relationship expected by the deterrence perspective, and the positive relationship anticipated by the other three theoretical perspectives, appeared in bivariate form with about equal frequency.

The results of this visual inspection are presented as supportive, not as definitive. No statistical tests were performed to confirm that the patterns observed exceeded what would be expected by chance. The only point being suggested here is that this inspection provided some visual evidence that both theoretically expected relationships were frequently apparent in bivariate form.

Model Check

Following the recommended "logic check" procedure for lagged models, the five models yielding significant impacts of lagged arrest and the one with a near-significant ($p < .06$) impact were reestimated using a causally impossible lag structure: Later arrest rates were used to predict earlier delinquency counts. Five out of six generated nonsignificant impacts of later arrest on earlier delinquency (p values ranged from .12 to .75). Only one model generated a significant result ($p < .05$).

Delinquency Counts for 10–18-Year-Old Males

The dynamics described by some of the theoretical perspectives applied here, especially the community justice framework with its emphasis on socialization, suggest the effects of adult arrest on later delinquency prevalence for 10–18-year-olds should be weaker than its impacts on 10–15-year-olds. Results showed this to be the case. Looking at the five significant and one near-significant models noted in the results, and repeating those same models but looking at serious first-time male delinquents aged 10–18 years, and controlling for the number of males 10–18 years, the observed IRRs were in the same direction as shown in the models for those aged 10–15 years but were somewhat weaker (results not shown). On average, the IRR deviations from a value of 1.00 were 55 percent (median = 56 percent) of what they were in the models focusing on those aged 10–15 years. In support of the deterrence perspective, however, both the deterrence impacts observed with delinquents aged 10–15 years were marginally significant ($p < .06$) when the outcome was the prevalence rate for delinquents aged 10–18 years. Because socialization processes are more clearly implicated in the community justice framework, the weaker impacts observed for 10–18-year-olds may suggest somewhat more support for this perspective compared with either the ecological GST or the ecological procedural justice models. The latter two models, not relying on socialization processes, suggest explanations that are less age graded.

VARIATION ACROSS DISTRICTS IN DELINQUENCY PREVALENCE RATES: 1920s AND 70 YEARS LATER

Turning from prevalence counts to prevalence rates, we mapped current cumulative male (aged 10–15 years) delinquency prevalence rates per 100 males aged 10-15 for 1996–1998, and for 2000–2002, organized into quartiles. These results were compared with mapped cumulative delinquency prevalence rates per 100 males aged 10–15 years for the period 1926–1928, by election ward (Shaw and McKay, 1942: Map 30), which is also organized by quartiles.[12] Comparing current maps with those from 70 years earlier showed several similarities. Some areas with low rates then showed relatively low rates currently: the upper end of the northeast part of the city and the lower portion of the northwest branch of the city. The latter includes Chestnut Hill, Manayunk, and Roxborough. Some areas with relatively high rates then showed relatively high rates currently, which includes areas just north of downtown. Of course, differences were observed as well. Higher relative rates in the mid-north section of the city seemed more likely now than in the 1920s. The current rates were lower on the eastern edge of the downtown compared with the rates in the 1920s,

12. In contrast to the current data, however, the data from the 1920s includes all delinquents. At that time, a substantial portion of the delinquents coming to the attention of the family court were cases of neglect, where parents were asking the court to place their children in a foster home because of extreme poverty (Urban Archives, Temple University).

probably in part because of revitalization and gentrification, which have been taking place east of the downtown core for several decades in Society Hill, Queen Village, and more recently Northern Liberties. Now compared with then, higher relative rates were more likely in West Philadelphia, west of the Schuylkill River; this area changed racially and economically in the 1950s and 1960s.

To a slight extent, the current geographic patterning observed depends slightly on the years chosen. Examining the cumulative 3-year prevalence rates for the years 2000–2002 showed two West Philadelphia districts dropping out of the top quartile (12th and 18th) and being supplanted by two districts in North Philadelphia (23rd and 39th), for example. Overall, however, both the 1996–1998 and the 2000–2002 mappings make similar contrasts to the rates 70 years earlier: The highest delinquency rates moved slightly to the north and west of the city center, with the relative position of these two high rate areas shifting somewhat between these two sectors depending on which current 3-year window was used.[13]

DISCUSSION

THE IMPACTS OF ADULT ARREST ON LATER DELINQUENCY ARE TIME DEPENDENT

Four current theoretical models anticipated a lagged ecological relationship between arrest rates and later delinquency prevalence rates. Three of those models—community justice, ecological GST, and ecological procedural justice—expected a positive relationship. The fourth framework, ecological deterrence, predicted a negative relationship. The current analysis using *all* Philadelphia male first-time serious delinquents aged 10–15 years during a multiyear period observed a *negative* impact of adult arrest on later delinquency for two forms of the arrest variable when arrest and delinquency were lagged by only one quarter, and a *positive* impact of arrest on later delinquency for three forms of the arrest variable—four if a near-significant impact was included—when the lag was six or seven quarters. With all forms of the arrest variable, the impact of arrest was initially negative and increased *monotonically* as the lag lengthened from two to five or six quarters. The impact of arrest on later delinquency was time dependent. What does this mean?

Broadly speaking, two ecological pathways seem plausible, one keying on differences between two types of localized groups, and the other keying on changes among those in the same types of groups. Following a between-group focus, within many of these districts there may have been

13. The two current maps can be found online at http://www.rbtaylor.net/del_maps.htm.

two types of localized juvenile groups affected by earlier arrests. Those in the first type may have been deterred from initial serious delinquent involvement following higher arrest rates by localized but relatively quick cycling processes not yet specified. In the same districts, those in the second type of group, through different and more slowly cycling localized processes not yet identified, perhaps became increasingly inclined to behave in ways that drew official sanction. A variation on the different group explanation would be that the relative frequency of these two group types varied across districts.

Another perhaps simpler possibility is that members of the *same* type of local group were first deterred by higher arrests because of relatively quick-cycling, localized processes linked to earlier higher arrest rates. Later, as a result of different but slower cycling localized processes linked to those earlier arrests from the same period, those in this same type of group acquired higher chances of serious initial delinquent involvement. In both of these scenarios, the dynamics suggested are ecological and group based, not individual based.

Such scenarios, of course, are only speculative. Only substantial empirical work sensitive to causal dynamics at the individual, household, small-group, and locality levels will allow identification of the most relevant *local* and *ecological* dynamics.

HOW MUCH SUPPORT FOR WHICH PERSPECTIVES?

Does the pattern of evidence help decide which type of arrest impact—the dampening effect on delinquency anticipated by ecological deterrence or the elevating impact expected by the other three perspectives—was more strongly supported? The negative impacts (IRRs < 1) of adult arrest on later delinquency counts with a short time lag were about the same size as the positive impacts (IRRs > 1) given a longer time lag. The graphical inspection of the individual district-by-district data series suggested that both the positive and the negative bivariate links were observed about equally often. Although deterrent impacts were observed for only two forms of the arrest variable, and positive impacts were observed for three, the deterrent impacts but not the elevating impacts also appeared ($p < .06$) when delinquency counts for 10–18-year-olds were examined. In sum, it seems that the initial suppressing effects of adult arrest on later delinquency and the exacerbating effects on later delinquency were roughly comparable overall.

The pattern of elevated delinquency after arrest shown here does not strongly favor any one of the three theoretical perspectives anticipating a positive arrest–delinquency link. The community justice/mass incarceration view is *perhaps* somewhat more strongly supported than the other two simply because the processes it references (Clear, 2007; Rose and Clear, 1998) take time to bear fruit. The removal of adult socializing agents from a locale results in more delinquency after a series of intervening shifts in supervision patterns as well as in teen and preteen activity patterns. It is not clear whether an ecological GST or an ecological procedural justice frame imply ecological processes that might take time to develop. More importantly, however, future work will need to employ additional indicators if we are to learn which of these three views proves most relevant to the positive arrest–delinquency link shown here.

FUTURE EVIDENCE NEEDS FOR ADDITIONAL THEORETICAL SPECIFICATION

Proponents of the community justice model will want to model in detail how preteens' and teens' supervisory networks have been altered as a result of the removals through arrest followed, for many arrestees, by long jail times, which are followed later, for many arrestees, by long incarceration times. Precision about who is removed for how long from what networks, and about communication between the teens and the removed adults, are needed. Simultaneous consideration of supervision and return rates, and their impacts on teens and preteens, is also warranted.

GST proponents could collect indicators to capture collective perceived stress-related qualities of these arrests for local groups of teens and preteens, especially those with above-average risks of delinquency. How do these arrest patterns and their sequelae affect the broader collective stress reactions and sentiments for juveniles in a locale who are socially connected to arrestees?

Ecological procedural justice theorists could collect community-wide indicators that reflect volume, average perceived fairness, and average perceived severity of arrests witnessed or heard about and, as importantly, how those direct and/or indirect experiences might have altered juveniles' *shared* views about law and agents of justice. Although research has shown that legal cynicism can vary ecologically (Sampson and Bartusch, 1998), it is not clear how fluid these views are over time at the community level, or how specific ecological attributes, like arrest rates and other justice agency practices at the community level, might shape such views.

Until research is conducted with indicators to capture the different mediating ecological dynamics linking more arrests to more subsequent delinquency, we cannot know which of these three views better explains the positive arrest–delinquency pattern observed. The key features of these mediating dynamics will be the amount of agreement within communities reported by the teens and preteens, especially by those at high risk for delinquency, and the degree to which dynamics vary ecologically and temporally.

Nevertheless, even without that additional information, the current results make a contribution to the ecological coercion and crime literature. Worries about potentially adverse consequences of higher arrest rates seem warranted, even when using relatively strict analytical approaches.

Perhaps counterbalancing such worries, however, are some expected benefits: ecological deterrence. Although recent progress toward a fuller understanding of the roles of individual-level mediating mechanisms and moderators of deterrence has been reported (Nagin and Pogarsky, 2003; Pogarsky, 2002), the ecological deterrence work continues to lack clarity about the relevant time frame within which deterrence processes "cycle" (Cousineau, 1973). As noted, Chamlin et al. (1992) found deterrent impacts of arrests on later crime after lags of a month and of a quarter, depending on the crime in question. The current work underscores Chamlin et al.'s (1992) suggestion that ecological deterrence can happen in a short time frame, and it extends the previous work by suggesting deterrence may apply to subsequent delinquent involvement and not just to subsequent adult criminal activity. Hopefully, future ecological deterrence-and-delinquency theorists can specify the relevant mediators and moderators of this ecological relationship, as has been happening in individual-level deterrence research.

LIMITATIONS AND STRENGTHS

The current work of course is afflicted with numerous limitations. These data were limited to only one city and to only a small number of years, which makes them in effect a case study. All the cautions surrounding studies of this design should be kept in mind. Second, the current work has not completely "solved" the endogeneity problem (Engle, Hendry, and Richard, 1983) or the related selection problem (Heckman, 1979). In a longitudinal context such as the one observed here, it is probably impossible to resolve these concerns with anything short of random assignment (Duncan, Magnuson, and Ludwig, 2004). Lacking that, interim solutions like two-stage, least-squares estimators for arrest or removal rates, or sophisticated panel designs (Do and Finch, 2008), may prove helpful.

A third clear limitation is that there were not enough cases to model female delinquency prevalence rates separately. Given other gender differences on both delinquency pathways and rates (Hagan, Simpson, and Gillis, 1987), alternative ecological dynamics for girls seem likely and deserve investigation.

Several features of the current investigation may prove, *after* subsequent empirical investigation (Taylor, 1994: 164), to be limits on the external validity of current findings. Even though these features are not inherent study limitations, we note that self-reported delinquency indicators (Short and Nye, 1957), different-sized spatial units, arrest rates concentrating on misdemeanors and felonies separately,[14] or different nonarrest indicators of criminal justice coercion all could have produced different results.

Perhaps partially offsetting clear or potential limitations are some positive features of the current work. This study sought a different, hopefully cleaner solution to the nonrecursivity problem. It controlled for temporal and seasonal trends. It focused only on within-district changes over time, making each district, in effect, serve as its own control. This was done in two ways: by introducing a string of (n – 1) incidental parameters for the different districts and by centering all arrest variables by their respective district means. District-level differences in overdispersion were accommodated by the fixed-effects, negative binomial model. Investigation suggested this was *not* a misspecified model even though district-level demographics were excluded, because the demographic changes in district structure over time did not significantly alter the ordering of the districts on fundamental features of community fabric. Additionally, *all* serious male delinquents aged 10–15 years arrested for the first time and mandated to treatment rather than just supervision during a multiyear period, in a large city of more than a million, were included for analysis. This group *included* those mandated to secure facilities. Finally, each significant feature of the pattern of results replicated across at least two different operationalizations of arrest rates.

CLOSING COMMENT

The current study counterpoints earlier mentioned research on the ecology of delinquency. Although that work viewed static delinquency rates or decade-long changes in rates, the current study considered shorter term delinquency changes. The four theoretical perspectives considered here each made a case for how, during a few months or a year or more, local criminal justice actions can alter community delinquency prevalence rates. In support of this expectation, the work here showed that community delinquency prevalence can shift in a short span, and those shifts can connect in one of two ways with earlier arrest rates.

14. Given the current analysis, the only way that the arrest variable could create bias would be if any of the following—the ratio of misdemeanor to felony arrests, the ratio of arrests resulting in convictions, or the average sentence length for those convicted—1) changed over time *and* 2) those changes took significantly different form in different districts. We know of no theoretical basis on which to expect such a time × district interaction effect on the components of, or results from, the arrest rate.

Such findings do not contradict earlier findings about multidecade stability of high- or low-delinquency areas, or previous findings about the structural covariates and consequences of decade-long shifts in relative delinquency rates. Rather, in the same way that decade-long relative shifts in delinquency rates (Bursik, 1986b; Shannon, 1991) simultaneously nest within, reflect, and perhaps alter broader multidecade stable locational patterns of high and low delinquency areas (Shaw and McKay, 1942), here too, these shorter term delinquency changes nest within, reflect, and perhaps alter the shape of decade-long relative changes in delinquency rates. A significant challenge for ecological delinquency researchers is to understand how these multidecade, decade-long, and shorter term delinquency patterns and changes connect with one another, and how the nets of causes and consequences of delinquency shift but perhaps also link in ways that depend on the time scale considered.

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APPENDIX A: MODELING ISSUES AND IMPLICATIONS

Because each district had several observations, cross-sectional time-series or dynamic panel models were appropriate. Within this framework, several modeling issues needed attention. First, we ask the following: Were the distributions of counts following Poisson distributions, or were they overdispersed, with the variances greater than the means (i.e., following negative binomial distributions)? A graphical analysis confirmed that both the distributions of first-time serious male delinquents aged 10–15 years and those aged 10–18 years followed negative binomial distributions more closely than Poisson distributions.[15]

The second question was whether fixed-effects or random-effects models were more appropriate. For cross-sectional time-series models, the question is whether the unit-specific (here district-specific) error terms were random and met the iid assumption (Hausman, 1978). Comparing cross-sectional fixed versus random Poisson models showed multiple significant Hausman tests ($p < .05$). So, fixed-effects models were used.

Using fixed-effects models, however, surfaced a third issue. Fixed-effects, cross-sectional time-series models with negative binomial distributions are implemented differently than fixed effects in other count models. Usually, fixed-effects models discard unit (here district) means and concentrate on within-unit variation (Baum, 2006: 220). But with a negative binomial distribution, the fixed-effects model instead allows two things: overdispersion and a unit "specific variance to mean ratio" (Hausman, Hall, and Griliches, 1984: 924). Unit-specific means remain in the model. In short, this "is not a true fixed-effects model" (Allison and Waterman, 2002: 248), save under very specific conditions (Guimaraes, 2008).

One can control for between-unit variation on the outcome by introducing unit-specific dummy variables in the model. Here, dummy variables for each district save the 39th were introduced.

Adding these dummies, however, raised a fourth issue that has been debated (Greene, 2001): Do the dummy variables introduce an incidental parameters problem? If the dummy variables are considered incidental and the main focus is on time-varying covariates, then introducing the incidental parameters may under certain conditions increase the asymptotic variance of the estimates for the time-varying covariates, meaning wider confidence intervals (Neyman and Scott, 1948: 26). There is also the question of whether the estimates for the coefficients of interest would be consistent from one sample to the next, given different impacts of shifting sets

15. Graphical analysis was carried out using nbvargr, Philip B. Ender's program available from http://www.ats.ucla.edu/stat/stata/ado/analysis.

of incidental parameters in different models. For the latter question, as of 2002, there was "no proof of this one way or the other" (Allison and Waterman, 2002: 256). Based on simulation data, Allison and Waterman (2002: 258) concluded, however, "there is little evidence for incidental parameters bias" with either Poisson or negative binomial cross-sectional time-series models.

There are several implications of the above modeling decisions. First, adding (n – 1) dummy variables for districts removed mean differences on the outcome. Group mean centering of arrest rates removed between-district mean differences on these key predictors. Second, district-level community demographic variables were not needed. The incidental parameters—the unit-specific dummy variables—were used instead following econometric convention. Last, must the models assume no changes over time in the structure of the districts? No. If all the districts were changing over time at roughly the same rate on any one of the key structural features of community that would *not* be problematic provided it preserved the relative ordering across the districts on these key features. Given the approximation of a "true" fixed-effects, cross-sectional time-series analysis implemented here, the only way fundamental community demographic fabric could confound these results would be if on a key feature, one or more districts were 1) changing over time *and* 2) the direction or extent of change was different in different districts such that at the end of the period there had been a significant shift in the rank order of the districts on this feature. To gauge this possibility, decennial 1990 and 2000 census data were compiled at the district level. A status index (median household income, median house value, percent adult population with at least college, and percent above the poverty line), a stability index (percent of owner occupied households and percent of households living at the same address since 1985/1995), and percent African American (in population weighted percentile form) were examined. The rank-order (Spearman) correlations between each district's 1990 and 2000 score were extremely strong: .98 for status, .95 for stability, and .88 for percent African American. A careful examination of the respective scatterplots showed extremely close corresponding 1990 and 2000 scores for status and stability, and the data also indicated departures from monotonicity for the race variable that were minor and originated from a small number of closely scoring districts switching ranks. Given this pattern, the authors maintain that the fundamental demographic structures of the police districts did not selectively shift in ways that could create confounding in these analyses.