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Population Size, Change, and Crime in U.S. Cities

Thomas Rotolo · Charles R. Tittle

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Abstract The sometimes noted contradiction between cross-sectional and longitudinal relationships concerning city population size and crime rates is reexamined using more complex analytic procedures, controlling for extraneous variables, and allowing for non-monotonic relationships. Instead of a simple cross-sectional relationship between population size and crime rates, the more sophisticated analysis reveals either no association or a quadratic relationship. Similarly, instead of a simple lack of longitudinal relationship or a negative one, the more complicated analysis shows a non-monotonic pattern for three of six offenses. However, we contend that these divergent patterns for cross-sectional relative to longitudinal data are not necessarily indicative of an ''anomaly.'' Instead, they represent different aspects of a dynamic process in need of more extensive theorizing. Finally, the cross-sectional results showing that city size and crime rates are either not linked or when linked are in a non-monotonic pattern call into question one of the accepted relationships in criminology that have long guided thinking about crime.

Keywords City · Population · Size · Crime · Change · Crime rate

Introduction

The literature concerning size of cities and crime rates presents something of a puzzle. On one hand, considerable evidence, based on police, victimization, and self-report data (e.g. Ackerman 1998; Archer and Gartner 1984; Clinard and Meier 1985; Elgin et al. 1974; Glaeser and Sacerdote 1999; Land et al. 1990; McCall et al. 1992;

T. Rotolo
Department of Sociology, Washington State University, Wilson Hall, Pullman, WA 00164-4020, USA

C. R. Tittle (✉)
Department of Sociology and Anthropology, North Carolina State University, Campus Box 8107, Raleigh, NC 27695-8107, USA
e-mail: Charles_Tittle@ncsu.edu

Ousey 2000; Sutherland et al. 1992, pp. 176–180; Tittle 1980), suggests a general, positive cross-sectional relationship between the population of settlements and crime rates. A few exceptions to the pattern have been reported for a few sets of cities (Ackerman 1998; Archer and Gartner 1984), for some specific offenses (Conklin 1981; Ousey 2000; Tittle 1980), at some time points (Land et al. 1990; McCall et al. 1992), and in some locales (Berman 1973; Krohn et al. 1984), and some have observed evidence of a curvilinear association (Macionis and Parillo 2004) while others have suggested that the association may be weakening (Ackerman 1998; Shaw-Taylor 1998). Nevertheless, the general positive cross-sectional relationship between city size and crime is often cited as one of the ''facts'' of criminology (e.g. Braithwaite 1989, p. 47; Gottfredson and Hirschi 1990, p. 17; Siegel 2003, p. 61). Moreover, many scholars assume that this cross-sectional association implies a causal effect of urban population size (or urban social environment) on crime rates (e.g. Fischer 1975, 1995; Glaeser and Sacerdote 1999; Mayhew and Levinger 1976; Wirth 1938 [1969]).

However, if urban population size does cause, or affect, crime rates, then, according to standard causal logic, longitudinal analysis should show that changes in population are followed by commensurate changes in crime rates, at least after an appropriate time lag. Yet, the longitudinal relationship between *growth* in population and *increase* in crime rates seems to be problematic (Archer and Gartner 1984; Gurr 1981; Gurr and Grabosky 1976; Johnson 1995; Lane 1969, 1979; Lodhi and Tilly 1973). Studies of specific cities over various time spans (see citations in Johnson, 1995, p. 147; Lane 1979), longitudinal analysis of population change and homicide rate for 34 international cities (Archer and Garner 1984), historical evidence about urbanization trends in different locales (Gurr 1981; Gurr and Grabosky 1976; Johnson 1995; Lodhi and Tilly 1973), and a study of change in Chicago neighborhoods (Bursik and Webb 1982) all suggest that growth in population may not generally be accompanied by increases in rates of crime, though it sometimes is (Freudenburg and Jones 1991). Therefore, comparisons of cross-sectional with longitudinal studies of urban population size and crime rates appear to show an ''anomaly,'' or what Archer and Gartner (1984) refer to as ''the paradox of cities and homicide rates'' (p. 101).

This apparent anomaly between cross-sectional and longitudinal observations about population and crime, however, may reflect methodological artifacts. Previous analyses have not fully[1] taken into account the possibility of non-monotonic, curvilinear relationships nor have they sufficiently controlled for potentially contaminating variables. On the other hand, even with full controls and specific analysis techniques focusing on potential non-monotonic relationships, the previously observed apparent anomaly between cross-sectional and longitudinal associations between city size and crime rates may remain, requiring an explanation. Here we address the apparent inconsistency between cross-sectional and longitudinal relationships, using data from a sample of 348 U.S. cities for four points in time—1960, 1970, 1980, and 1990. We first try to determine if the potential inconsistency between cross-sectional and longitudinal conclusions about population size and crime rates, dramatized by Archer and Gartner (1984), characterizes our sample of U.S. cities. We compare results of cross-sectional analysis for each of four time points with results of longitudinal analyses, focusing on both growth and decline in population

[1] Some studies have used logged population as a predictor, which implies some non-monotonicity.

size and its potential linear and quadratic association with rates of six offenses. An apparent inconsistency in results for cross-sectional and longitudinal analyses does emerge, though it is of a different nature than previously observed and far more complicated than previous analyses would suggest.

However, interpreting comparisons of cross-sectional with longitudinal results is not as straight forward as it might at first seem. Various lines of thought suggest that changes in population size may be linked in different ways with changes in crime rates, and whether such relationships imply consistency or inconsistency with cross-sectional analyses may be a matter of judgment.

Potential Dynamic Patterns

A Potential Positive Linear Relationship

The most obvious possibility, logically corresponding directly with cross-sectional observations about population size and crime rates, is for both increases and decreases in population to be associated with corresponding changes in crime. Various theories provide rationales for expecting such a relationship. For instance, Wirth's (1938 [1969]) well known urbanism formulation, though empirical support for it provided by short term and other limited evidence is mixed (see, e.g. Fischer 1984; Tittle 1989), connects population size with crime (and other "pathologies"). The theory contends that, because of being frequently surrounded by strangers (due to density) and people unlike themselves (heterogeneity), residents of large settlements have trouble establishing and maintaining social ties, thereby weakening social integration and social control. The resulting anonymity, tolerance, and psychic stress both stimulate criminal and other deviant impulses and make it difficult to constrain misbehavior. Hence, urban living presumably affects rates of crime, with the intervening causal mechanisms being poor social integration and consequent weak social control (Freudenburg 1986; Sampson 1986, 1987). Following the logic of the theory, growth of a city's population should produce greater anonymity, render social integration more difficult, and produce weaker social control. Similarly, population declines should reduce anonymity and anomie by reducing the number of strangers with which urbanites must deal, and it should enhance social integration and social control because smaller populations have greater chances of becoming integrated and exercising effective social control. Theoretically, then, reductions in city population sizes should lead to reductions in crime, at least after some (unknown) time lag. The net effect should be a monotonic relationship between population change and corresponding changes in crime rates.

A second urban theory suggesting a potential positive monotonic relationship between settlement size and rates of crime is Claude Fischer's subcultural theory (1975, 1995), for which at least some favorable empirical evidence has been reported (Fischer 1995; Tittle 1989). Fischer portrays population concentrations as facilitating innovation and diffusion of unconventionality through the mechanism of subcultures. According to the theory, urban life is bifurcated into the public and private domains. The public domain is characterized by anonymity, tolerance, and impersonality but in the private domain traditional interpersonal bonds and social integration emerge. Private social networks include subcultures built around specialized interests, including crime and deviance. Because of large populations, cities contain many different ‘‘critical masses’’ of people with specifically shared, especially unconventional, interests. And, because of the anonymity and tolerance, those with unconventional interests can find each other. Interaction among like-minded people with peculiar interests leads to subcultures built around the special interests that bring the people together. Subcultural affiliation, in turn, provides a supportive network and normative expectations that enhance the chances of misbehavior (see Tittle 1989). Moreover, Fischer contends that:

> ‘‘The more urban the place, the higher the rates of unconventionality....because (a) larger places generate more diverse and more specialized subcultures,; (b) critical mass and intergroup friction are likelier in larger places, which produces more intense subcultures, ...; and (c) the greater intergroup contact in larger places diffuses cultural elements of atypical subcultures to others in the area. Empirically, then, we should observe that residents of larger towns vary more in social behavior than do those in smaller towns and also observe that the distribution of social behavior in larger towns is more often skewed in the atypical, unconventional direction.’’

Even though Fischer uses the term ‘‘critical mass’’ to denote that a cluster of people with a specific shared interest is sufficiently large to make a subculture possible, it is clear that his theory proposes continuing linear, rather than threshold, effects of overall population size. First, the population size needed to generate ‘‘critical masses’’ for different unconventional interests varies, depending on the rareness of those interests. Some especially unusual interests necessitate exceptionally large populations before enough people with that particular interest are clustered to make a subculture possible. Other unconventional interests are less rare, enabling smaller populations to generate a critical mass. Further, the number and intensity of subcultures is theorized to be enhanced by the presence of other subcultures. So, the argument seems to be that ever increasing population produces more and more different kinds of subcultures as well as more intense ones.

Second, Fischer defines urban ‘‘solely in terms of population concentration—the greater the number of persons aggregated at a place of settlement the more urban the place’’ (Fischer 1975, p. 1323). Then, he goes on to state his propositions concerning subcultures using ‘‘urban,’’ so defined. Hence, when he uses the word ‘‘urban’’ in his theoretical arguments, one can substitute the notion of ‘‘population size.’’ Thus ‘‘The more urban the place, the greater its subcultural variety (Fischer 1975, p. 1324) can be read as ‘‘The larger the population, the greater the subcultural variety.’’ And, ‘‘The more urban a place, the more intense its subcultures’’ (Fischer 1975, p. 1325) implies ‘‘The larger the population, the more intense are the subcultures within it.’’ Similarly, ‘‘The more urban a place, the more numerous the sources of diffusion and the greater the diffusion into subcultures’’ (Fischer 1975, p. 1327) means ‘‘The larger the size of the population, the more numerous are the sources of diffusion and the greater the extent of diffusion into subcultures.’’ Finally, ‘‘The more urban a place, the higher the rates of unconventionality’’ (Fischer 1975, p. 1328), the proposition that Fischer derives from the above premises, states that ‘‘The larger the population, the higher the rates of unconventionality, including crime.’’ Hence, in a rather straightforward way, subcultural theory sets forth a linear causal argument about size of place and crime rates, with subcultural formation and intensification being the intervening causal mechanisms between size of place and crime rates. This is confirmed by Fischer's mission statement, which is to establish "What cultural and behavioral differences, if any, are generated just by residence in communities of differing levels of urbanization?" (Fischer 1975, p. 1319).

Therefore, although Fischer does not write specifically of the effect of population changes, his arguments clearly imply that growing populations will cause increasing numbers, varieties, and intensities of subcultures, which in turn will lead to greater rates of crime and other forms of unconventionality. By the same logic, declining populations should reduce the numbers, varieties, and intensities of subcultures, leading to reductions in crime. Although subcultures, once formed, may contain internal mechanisms enhancing the chances of their own continuity over time, population decline should nevertheless weaken them. Following Fischer's argument, this should occur because declining populations will have fewer new subcultures being born and those that continue to exist will show less variety. Since the sources of subcultural intensity and vitality are other subcultures, overall, subcultural influences should weaken with population loss. Moreover, this overall trend should hold though any specific, long-established subculture may retain some of its strength for a long time The net long range effect of population decline, then, should be a weakening of subcultural influences with a consequent reduction in crime rates.[2]

A Potential Artifactual Relationship

Some arguments, however, seem to challenge expectations of directionally corresponding relationships between population dynamics and crime changes. For example, Gibbs and his associates (Gibbs and Erickson 1976; Stafford and Gibbs 1980) attribute the elevated rates of crime in larger cities partly to the presence of non-residents. Typically, the issue of city size and crime is framed in terms of rates—crimes in a given time period relative to the number of residents. But, since cities host many workers, shoppers, visitors, business people, those seeking entertainment, and those in search of criminal opportunities, the number of potential victims and criminals at any point in time far exceeds the number of people who actually live there. Therefore, according to the argument, using resident population as the focus in examining crime, distorts and inflates the crime picture, making it appear that larger cities suffer more per capita crime than they actually do. As a result neither the absolute size of the population nor changes in size will necessarily be associated with changes in crime. Instead, crime will be partly linked to the size of the transient population, which is more likely a product of the dominance of a city in an area. The largest or most attractive places in a region attract more transients even if in absolute terms those cities are relatively

[2] An additional argument concerning changes in population suggests an association between population changes and crime rates consistent with the linear cross-sectional patterns often observed, though the coefficients would be somewhat attenuated. Mayhew and Levinger (1976) focus on interaction possibilities, contending that crime, especially violent crime, is a probabilistic product of human contact. The more often humans interact, the more likely is somebody to be offended, harmed, or exploited. Therefore, crime can be predicted from a simple multiplicative function of interaction possibilities. Because increases in population produce even larger increases in interaction possibilities, population growth should lead to spiraling amounts of crime while population declines should lead to reductions in crime at a decreasing rate. Thus the pattern will be a positive association with an increasing slope, which should be manifest as a modest underlying positive coefficient.

small. And, unless city growth is accompanied by changes in relative dominance, it will have no effect on crime.

This same process may underlie Archer and Gartner's (1984) finding that the relative size of places is linked to crime while absolute sizes are not necessarily so linked. In their international sample, the largest cities in an area almost always had the greatest relative amounts of crime though such cities were often considerably smaller in absolute size than places with much more crime in other areas. The authors offer this "relative size" notion as an explanation for their tentative findings that changes in population size are unrelated to changes in crime. Since absolute growth in population does not necessarily correspond with changes in relative size, it may have no connection with changes in crime. However, although Archer and Gartner do not explicitly say so, the dominance notion seems to fit their account. Relative size may be important because it signifies larger transient populations.

If these artifacts are operating, a relationship between increases or decreases in relative amounts of crime and population growth or decline should be problematic, depending on peculiar linkages between growth and dominance of cities. Moreover, even if an association between change in population size and crime is observed, it should disappear when dominance is controlled.

A Potential Non-Monotonic Curvilinear Relationship

A third possible relationship between changes in population and changes in crime rates is suggested by what has come to be known as systemic theory. It focuses on population stability and the resulting social networks that grow from regular interaction (Bursik and Webb 1982, p. 39). Originally applying only to neighborhoods within cities, ideas from systemic theory have in recent decades been extended to larger population aggregates (see in particular Crutchfield et al. 1982; Sampson 1986; Miethe et al. 1991; also see Ousey 2000. pp. 285–288 for a more comprehensive review of the literature on this point) and more recent statements represent syntheses of several theoretical streams. The first source of systemic ideas is Shaw and McKay's social disorganization argument (Bursik 1988; Shaw and McKay 1942 [1969]), which focused on weak social control. Shaw and McKay contended that population instability in inner city deteriorated areas involved attraction of low income people to low rent housing, an attraction that lasted only until their improving economic fortunes allowed them to move. Even those whose economic circumstances made mobility unlikely nevertheless dreamed of leaving, and because of such dreams failed to invest themselves emotionally in the community. Thus, in addition to poor socio-economic conditions directly affecting ability to exercise social control in the family and neighborhood, such conditions also have indirect consequences for overall community organization. By extension, theorists have suggested that population change, no matter whether it occurs in deteriorated or non-deteriorated areas affects the strength of community organization (Berry and Kasarda 1977; Bursik and Grasmick 1995; Bursik and Webb 1982; Carr 2003; Sampson 1988).

A second set of ideas helping to flesh out systemic theory stresses stability within social units (Kasarda and Janowitz 1974; Sampson, 1988). The basic idea is that people tend to form communities around shared activities and interests. Thus, if occupants have sufficient interaction time that is not disrupted by population change and residential mobility (Berry and Kasarda 1977), any environment will produce networks of interconnected social worlds. The key factor in emergence of these interconnected social worlds is population stability.

Third, the ‘‘systemic’’ notion has been reinforced by research and theorizing about social networks (Bursik 1999; Coleman 1988; Portes 1998), and the effects of overlapping interpersonal and community social bonds (Ahmed et al. 2001; Braithwaithe 1989; Briar and Piliavin 1965; Felson 1986; Freudenburg 1986; Hirschi 1969; Nye 1958; Reckless 1967; Reiss 1951; Toby 1957).

Overall, then, systemic theorists have come to expect that community networks based on frequent and sustained interaction and interdependency will produce strong social ties and effective social controls, some part of which stem from networks of shaming (Ahmed et al. 2001; Braithwaite 1989), others of which derive from restraints of attachment, commitment, investment, belief, and mutual reinforcement for self-images. This argument stands out by suggesting that changes in population may not be directionally linked with changes in crime rates. Instead, systemic theory implies that *any* change in population size, either increase or decrease, likely will lead to *greater* crime in the area where the change is occurring. The main idea is that social organization and accompanying social control tends to grow naturally when individuals remain in the same geographic area for a sufficient period of time. Through repeated interactions with the same people, long term residents become enmeshed in networks of social linkages and bonds, provided those with whom they might establish such ties also stay in the immediate area long enough for social relationships to develop. Social bonds so formed produce individual commitments to the maintenance of the norms prevailing in such locales and they make individuals sensitive to the potential reactions of others should they misbehave (Braithwaite 1989; Coleman 1988). Consequently, those who are integrated into local social networks are less likely to violate the norms, and the greater the proportion of people so integrated, the less the overall amount of crime.

However, population change, particularly from residential mobility, tends to disrupt restraining social bonds for new and previous dwellers and for their communities. Movers become disengaged from local social networks and thereby become free to misbehave, at least temporarily until they re-establish stable social relationships in the area to which they move (Tittle and Paternoster 1988). Meanwhile the previous community suffers weakened commitments and less social control because the mover is one less participant to serve as a conforming role model, to observe and report misbehavior, to impose shame or other informal sanctions, and to fulfill mutual responsibilities that would generate commitment. According to systemic theory, the key to greater conformity is population stability. As a result, communities with frequent population change, from natural increase or decrease, or from out migration or in migration, or from combinations of these sources, will lack ‘‘closure’’ for their social networks (Coleman 1988, pp. 105–108), and will have difficulty generating social cohesion and effective social control (Crutchfield et al. 1982).

This systemic argument has been challenged both theoretically and empirically. Various contemporary scholars contend that effective social control can be achieved even in communities without strong interpersonal or community social bonds (Baumgartner 1988; Granovetter 1974; Sampson et al. 1997; Sampson et al. 1999), and some research questions the linkage between integration and social control

(Bellair 1997; Carr 2003; Morenoff et al. 2001; Warner and Rountree 1997; see Ousey 2000, pp. 291–294 for a review of the somewhat contradictory evidence about this). Moreover, there might be other exceptions to the systemic argument in communities fully oriented around criminal behavior where instability disrupts established social networks exercising social control that produce criminal behavior, leading to less crime.[3]

Nevertheless, based on systemic theory, cities with unstable populations and those shown to have weaker integration should exhibit more criminal and other misbehavior, as shown by some research (Chamlin 1989; Crutchfield et al. 1982; Miethe and Meier 1994). Therefore, to the extent that the systemic theory applies to larger units than neighborhoods, as assumed by Crutchfield et al. (1982), Sampson (1986), Miethe et al. (1991), and Ousey (2000, pp. 285–288), and to the extent that population size is linked to population instability, the theory seems to explain the frequently observed relationship between size of place and relative crime. Moreover, it also explains why changes in population might not be associated in directionally corresponding ways with changes in crime rates. Population growth and population decline should both lead to greater crime because they both produce social and community disorganization.

Overview

We have identified three potential sets of relationships between changes in cities' population sizes and changes in crime. They include (1) corresponding directional effects (increases and decreases in population associated respectively with increases and decreases in relative crime), (2) either no association or spurious relationships of various forms, and (3) any change in population, regardless of direction, and increases in relative crime. As will be seen, our simple cross-sectional analyses prove to be consistent with most previous studies of urban population size and crime rates, showing a positive, monotonic relationship. Hence, if the apparent anomaly between cross-sectional and longitudinal analyses previously identified in the literature is present here, we should find that our simple longitudinal analyses do not reveal a positive, monotonic association between changes in population and changes in crime rates. Instead, if the paradox holds, and we rely on extant theory, we should find either no relationship between change in population and change in crime rates (per the artifact argument) or a pattern in which any changes in population, either increases or decreases, will be linked to changes in crime rates (per the systemic argument).

[3] However, there does not appear to be hard evidence of whole cities being organized around and supportive of criminal norms. Certainly, criminal subcultures sometimes exist within larger communities but they stand in opposition to generally held conventional norms (see Tittle and Paternoster 2000, Chapter 4). In addition, there are instances of cities with corrupt governments that tolerate some forms of criminal activity as well as cities that are largely controlled by criminal syndicates. Yet, in neither instance is it likely that the community structure and intertwined informal organization are basically criminally oriented. Moreover, even if such cities do exist, they are probably not numerous enough to affect the generally predicted relationships here being described.

Methods

Sample

The cities in our analysis were included in a larger set of 584 used by Miethe et al. (1991) in their study of the effect on crime rates of changes over two decades in variables representing routine activities and social disorganization theories. The original cities were those of at least 25,000 population in 1960 for which complete data could be obtained for their analysis. We added to the variables included in the Miethe data set[4] and extended it to include data for 1990. In this expansion the sample was reduced because complete data for all four data points could not be obtained for all cities. While not a random sample, these cities all had populations exceeding 25,000 at each of the four data points and among them the independent and dependent variables all show substantial variation (Table 1), allowing us to conduct a meaningful analysis.

Dependent Variables

We consider six major crimes: homicide, burglary, assault, robbery, rape, and auto theft. ‘‘Crimes known to the police’’ were extracted from Uniform Crime Reports for the census years or from data tapes obtained from the U.S. Federal Bureau of Investigation. Our primary statistical analyses model changes in crime rates (1960–1970, 1970–1980, and 1980–1990) for each of the six offenses.

Scholars have debated extensively about standardizing variables such as criminality that are logically intertwined with the number of people in a unit but which may nevertheless be causally affected by size of population (see Chamlin and Cochran 2004; Firebaugh and Gibbs 1985). Most discourse uses number of crimes divided by the population and multiplied by 100,000. Such standardization is important because even if a city of 500,000 contains the same degree of criminality among its people as does a city of 25,000, it will almost certainly have much more crime simply because there are 20 times more people to express that criminality. Analysis using such ratio variables where population size is an independent predictor as well as being the denominator for standardization, however, is complicated by the possibility of either autocorrelation suggesting a false effect or inadvertent reduction of actual effects.

One procedure suggested for overcoming that potential problem employs components rather than ratios (see Firebaugh and Gibbs 1985), with at least one study showing that the two approaches, at least cross-sectionally applied, produce quite different results (Chamlin and Cochran 2004). Yet, using simple ratios, the best established and most widely practiced approach (Firebaugh and Gibbs 1985), assumes that the dangers of artificial or misleading results are minimal if certain conditions hold and specific precautions are employed. Therefore, we follow the usual practice of using rates, calculated as crimes known to the police divided by the population, rather than including the components separately in the analyses. We do so for several reasons. First, we want to preserve the linguistic and conceptual meaning implied by the term ‘‘rate’’; that is, of a standardized measure implying that

[4] We are grateful to Terry Miethe for generously sharing this beginning data set with us. See Miethe et al. 1991.

Table 1 Descriptive statistics for (a) independent and dependent variables, (b) control variables

Variable	Minimum	Maximum	Mean	SD	Variable	Minimum	Maximum	Mean	SD
(a)									
Pop size					*Burg rate*				
1960	25,000	7,781,984	154,179	505,277	1960	42.55	1866.04	538.23	289.87
1970	25,439	7,895,563	164,399	511,036	1970	217.56	4381.62	1313.95	684.58
1980	26,027	7,071,639	162,191	468,828	1980	472.84	5896.17	2261.62	887.26
1990	25,084	7,322,564	171,306	487,133	1990	179.70	4946.56	1695.70	843.94
Pop chng (%)					*Rob rate*				
1960–1970	− 0.33	2.03	0.15	0.29	1960	0.00	375.59	40.60	47.41
1970–1980	− 0.24	1.21	0.03	0.19	1970	0.00	1561.58	168.39	195.33
1980–1990	− 0.23	0.89	0.06	0.16	1980	6.56	2083.38	315.18	316.53
					1990	0.00	2279.19	342.36	340.71
Homicide rate					*Rape rate*				
1960	0.00	36.42	4.35	4.65	1960	9.79	743.02	213.13	143.42
1970	0.00	58.47	7.97	8.13	1970	41.32	3403.43	581.93	470.18
1980	0.00	63.43	10.77	10.03	1980	53.30	3792.74	645.14	468.21
1990	0.00	77.77	10.90	11.88	1990	19.19	5033.69	840.45	692.73
Assault rate					*Auto rate*				
1960	0.00	799.41	70.10	94.40	1960	0.00	50.51	6.36	6.85
1970	0.00	855.29	189.08	156.87	1970	0.00	101.67	19.49	15.68
1980	6.85	1843.04	374.56	292.32	1980	0.00	173.08	48.82	33.93
1990	1.85	3915.81	581.00	484.77	1990	0.00	269.02	57.64	42.27
(b)									
Dominance					*% Age 15–29*				
1960–1990	0.00	2777.0	83.31	222.79	1960	9.96	52.17	20.16	4.47
					1970	14.47	65.74	25.49	5.38
					1980	18.51	68.56	29.30	5.42
					1990	14.42	66.26	25.29	5.28
Metro					*Med income*				
1990	0.00	1.00	0.72	0.45	1960	3,018	13,793	6,051	1225.66
					1970	4,885	20,440	9,767	1924.61
					1980	10,650	41,148	19,441	3899.56
					1990	16,889	83,272	33,729	8838.59
Density					*% Below pov*				
1960	561	40,139	5501.22	4335.35	1960	3.58	50.01	17.61	8.15
1970	300	44,081	4951.79	4372.08	1970	3.81	41.71	14.29	5.83
1980	101	39,709	4456.56	4095.56	1980	3.39	40.34	17.44	6.35
1990	145	44,043	4369.68	4088.62	1990	0.70	38.50	12.48	6.10
Region					*Gini income*				
South	0.00	1.0	0.31	0.46	1960	0.21	0.83	0.35	0.06
East	0.00	1.0	0.22	0.42	1970	0.25	0.73	0.35	0.05
Midwest	0.00	1.0	0.28	0.45	1980	0.28	0.84	0.37	0.05
West	0.00	1.0	0.19	0.39	1990	0.29	0.53	0.39	0.04
% Black					*% Unemploy*				
1960	0.10	57.40	10.49	11.75	1960	0.70	12.70	5.20	1.83
1970	0.00	71.10	12.39	13.15	1970	1.40	9.20	4.64	1.46
1980	0.20	83.22	15.58	16.03	1980	2.16	19.96	7.06	2.90
1990	0.20	89.95	17.65	17.40	1990	1.70	16.70	7.28	2.63
% Div male									
1960	0.57	7.34	2.49	1.08					
1970	0.81	8.26	3.25	1.19					
1980	1.99	13.05	6.43	1.70					
1990	2.67	14.81	8.46	1.95					

N = 348 cities

crime is not an artificial function of population size, while at the same time assessing the impact of population size as a theoretically based potential causal variable in affecting crime. Second, we want to be consistent with most other scholars who have used ratios in their examinations of ecological indicators of crime. Third, our analysis is not actually of the form with the greatest potential for artifactual findings to which the Firebaugh and Gibbs (1985) safeguards apply.[5] Fourth, even when we used alternative estimation techniques and compared results with ratio and component variables, the results led to the same substantive conclusions.

Though our analysis is not strictly of the ratio form about which so much has been written, and even though it involves dynamic rather than the static analyses for which the guidelines laid down by Firebaugh and Gibbs (1985) were designed, we nevertheless followed their logic, using, where necessary, analogs of their guidelines. For instance, we use regression analysis rather than correlation (their guideline #1) in addressing the simple cross-sectional relationship between city size and crime rates. In addition, since the common term in our case is population size, its disturbance term is likely to be more homoscedastic (guideline #3), and its measurement is fairly reliable (guideline #4). Further, we experimented with a dynamic-analysis analog of the 1/population term recommended by Firebaugh and Gibbs (guideline #5). With the analog term included, all results remained similar to those presented.

We depart from the Firebaugh and Gibbs's prescription to use ratio variables consistently throughout the analysis (guideline #7). As we argue later, there are good reasons to use absolute change in crime rates rather than the percent change that we use for population. Moreover, alternative analyses using non-ratio forms for the dependent variable, including change in the number of crimes committed and the log transformation of the change, produce results that strongly parallel the findings we present here. We also do not follow the Firebaugh and Gibbs guideline #7 because the form of our dependent variable is not a simple proportion, given that we are examining the change in the crime rate.

Independent Variables

Our independent variables are (1) the population of the cities and (2) changes in populations. Information on the population of each city was taken from the decennial censuses for each data point (U.S. Census, 1960, 1970, 1980, 1990). In examining the cross-sectional relationship between city size and crime, we use the city population in a given census year as the independent variable. In assessing

[5] Firebaugh and Gibbs discuss problems surrounding the estimation of a ratio variable model, where the dependent variable is constructed with population size and this variable is included as an independent variable. The model resembles:

$$\frac{\#Crimes}{Population} = Population + Controls$$

Our models differ distinctly from the general form discussed by Firebaugh and Gibbs. In the models we estimate, the dependent variable is the difference between two ratios:

$$\left[\frac{\#Crimes_T}{Population_T}\right] - \left[\frac{\#Crimes_{T-1}}{Population_{T-1}}\right]$$

with the key independent variable being relative change in population size, a measure that does not enter into the construction of the dependent variable.

change in population as a predictor of change in crime we use the relative change in size of cities:

$$(\text{city pop}_{t+1} - \text{city pop}_t)/\text{city pop}_t)$$

between three different pairs of data points: 1960–1970, 1970–1980, and 1980–1990. We are forced to use 10-year lag periods because the data are from the census and we cover only a 30-year period of time. It is possible that too little change actually occurs in these 10 years spans to reveal much about effects on crime rates. However, as Table 1 shows, change ranged from – 0.33 to 2.03% between 1960 and 1970, from – .24 to 1.21% between 1970 and 1980, and from – .23 to 0.89% between 1980 and 1990. Moreover, correlations between city size and population change show statistically significant associations: 1960/60–1970, $r = 0.11$ ($P < 0.05$); 1970/70–1980, $r = -0.69$ ($P < 0.01$); 1980/80–1990, $r = 0.36$ ($P < 0.01$). Therefore, it appears that enough change occurs to permit meaningful conclusions.

We also explored other measures of change, including raw change scores, logged transformations, and residual change scores (e.g. Bursik and Webb 1982). Using these alternatives produces no substantive differences from the results we present here.

Control Variables

We control for 11 variables in order to make sure the potential differences between the cross-sectional and longitudinal relationships among the measures of population and crime rate changes are not due to extraneous antecedent conditions. They are (1) region in which a city is located, (2) whether a city is in a metropolitan area in 1990, (3) density, (4) decade, (5) city dominance in an area, and (6) six structural variables (median family income, percentage of families living below the official poverty line, the Gini index of family income inequality, the percent of children age 18 or under not living with both parents, percentage of males divorced, percentage of the population ages 15–29, and the unemployment rate) identified by Land et al. (1990)[6] We control for region because it may signify cultural patterns that could affect how and why cities grow or decline as well as the meanings people attach to changes in population. For example, in regions that are traditionally very urban, people may be accustomed to more rapid patterns of, and expectations for, adjustment, thereby diminishing potential crime generating periods of adaptation to population changes. In addition, in some regions, city population changes are more likely to have resulted from natural increase or deceases, implying greater time for socialization of new residents. Our regional dummy codes designate the Northeast as the suppressed category.

Controlling whether a city is in a metropolitan area or not is important because changes in city population may be routine in metropolitan areas, occurring with movement of people within localized contexts. As a result, residents of cities to and from which movement occurs may be less affected by population changes, with fewer consequences for crime.

[6] Though the original data set with which we are working included some of these measures, to be safe, we extracted from ICPSR the exact figures used by Land et al. for 1960, 1970, and 1980 to which we added comparable figures for 1990.

Further, some population change is attributable to expansion or shrinkage of territory within city limits. Population gains or losses may have quite different consequences, depending on whether they imply increases or decreases within a limited territory or simply expansion of territory. To help eliminate this source of contamination, we control for changes in density. Some cities may have gained in density over the three decade period because they annexed denser areas, but most increases in city density probably came from actual growth in population within a stable territorial unit. By contrast, though loss of density may imply an actual decline in population for a stable territorial unit, it is also often brought about by an increase in territory that is less densely populated (urban sprawl). By holding density constant, we try to allow the influences of population change for stable geographic units to emerge more clearly.

We control for decade by including dummy variables for the 1960–1970 and the 1970–1980 time periods, with 1980–1990 as the omitted category. It is possible that a connection between population change and crime might be a historical artifact of events occurring mainly within a particular decade. For instance, the 60s were a time of great political and demographic turmoil. It is possible that the forces affecting change in city populations may have been coincidentally connected with crime, which was generally accelerating for almost all population units. If so, then the strength of that spurious relationship might give a misleading impression that population change is connected with changes in crime in a particular way. The use of the decade dummy enables us to ascertain if our results are period-specific.

Since relationships between population size and/or dynamics and variations in crime may be an artifact of the way crime rates are calculated that permit transient populations to influence the rates, our analysis requires an indicator of city dominance. Ideally, we would use a scale or index combining such things as the relative proportion of people within given distances who shop and work in the city, geographic distribution of city newspaper circulation, relative proportion and frequency of people within given distances who seek recreation in the city, or proportion of air travelers within given distances who use the city as a point of departure or arrival. Unfortunately, such information is generally unavailable for cities that are not metropolitan central places. Therefore, we use a simple indicator—the distance in 1990 from each city to another city of equal or larger size.

This measure of urban dominance is not ideal. First, it compounds the notion of dominance with the size of place itself, which would necessarily be the case with any measure of dominance because dominance is conceptually linked to population size in most cases. In our data, the association between the dominance measure and population size in 1990 is approximately 0.39. However, as will be seen later, controlling for dominance, as we measure it, does not eliminate the anomaly that is the focus of attention here. This suggests that the dominance measure is not entirely or even largely about size per se else the effects of size would have been rendered insignificant.

There are other potential weaknesses of the dominance index. First, it assumes that (1) a settlement is more dominant when people have to travel farther to find an alternative locale with equal services or attractions, and (2) cities of equal or greater size offer activities and services of equal or greater appeal. A smaller and closer rival city could possibly attract and influence people more than a larger and more distant city, thereby making the smaller city more dominant. For example, in an area there might be a modest-sized, blue collar city 200 miles from a larger blue collar city but between the two is a smaller government center that provides both leadership and influence for the entire region. In such a case, our indicator, which would rank the larger blue collar city as the most dominant and the mid-point political city as less dominant, would be in error. However, such instances would seem rare and in any case their existence would be purely speculative since there is no systematic hard evidence of the type needed to establish actual dominance for all of the cities in our sample.

In addition, our measure of dominance might produce incorrect conclusions if relative distances of the larger cities changed from 1960 to 1990. Perhaps some cities grew faster over the three decade period so that they came to exceed the size of previously larger nearby cities. To check on this possibility, we calculated the distance indicator for a 10% random selection of cities in our sample in 1960 to compare with our 1990 calculation. The 1960 and 1990 indicators show a correlation of 0.997, so we feel reasonably confident that dominance among our cities, as we measure it, did not change much over the time period covered by the analysis.

To provide further checks on our dominance measure, we employed an alternative, indirect approach. We analyzed a subsample consisting of the cities that experienced the largest average growth (the 75th percentile) over the four time points. This approach assumes that the most dominant cities experienced the most long term growth. That assumption may not be correct, so this alternative approach may be no more compelling than the one using the distance measure. Nevertheless, it is reassuring that this alternative approach did not yield any findings that depart from what we observe with the distance measure.

Finally, we control for the additional structural variables identified by Land et al. (1990) because they have now become widely accepted as potential influences on crime rates. Table 1a and b show descriptive statistics for the variables used in the analysis. We use mean-centered versions of population and population change in models where a quadratic specification is used. Collinearity diagnostics suggest that only the association between median family income and the percentage of families living below the official poverty line warrants concern. These control variables are not our primary focus, however, and additional analyses where we eliminated one of the two control variables alternatively suggest that the association between the two variables is not influencing our results. Moreover, readers should note that while many of these variables exhibit strong cross-sectional associations, such high correlations do not appear among our independent variables because we are using dynamic measures for most of them. For instance, density and population size show a correlation of approximately 0.3 in both 1980 and 1990, but the correlation between relative change in population size and change in density between 1980 and 1990 is only −.05.

Analysis Techniques

Our first set of analyses explores the cross-sectional association between city size and crime rates using simple bivariate regression coefficients in which city size predicts crime rates and simple change in city size predicts change in crime rates. In later analyses we include a squared term in our cross-sectional analyses as well as in our longitudinal analyses to allow for non-monotonic, curvilinear relationships. In addition, in later analyses concerning the effects of population change, we use pooled cross-section time series models corrected for within-panel (city) autocorrelation and heteroscedasticity by parameterizing the variance matrix of the disturbance terms such that the variance for each city is different, and the error terms of the cities are correlated.

Note that in our analyses we use percent change for our main independent variables but we use absolute change in our measures of crime rates. Percent change scores are especially appropriate for population size because their use controls the size of the population at the beginning of the time series and avoids distortions that would result from comparing small cities, who would likely have relatively small absolute changes in population with large cities that might have very large absolute changes in population.

However, because crime rates, particularly homicide rates, are relatively low to begin with (calculated per 100,000 population), using percent change would lead to large distortions if cities with low crime rates experienced even modest changes in number of crimes. The disadvantage of using absolute change scores is the absence of a control for the initial level of crime. But including such a control would be equivalent to examining percent change, thereby creating the problem of distortion mentioned before. Nevertheless, we conducted additional analyses using percent change in crime, as well as the change in the total number of each of the specific crimes committed and logged crime rate change. All of these alternative dependent variable specifications produced results that strongly parallel the findings we report here.

Results

Our first objective is to ascertain if the usually observed cross-sectional association between population and crime rates characterizes our cities while being inconsistent with the relationship between changes in population and changes in crime rates. Table 2 shows (1) bivariate cross-sectional regression coefficients representing the associations between city size and each of the six crime rates at each time point, and (2) bivariate longitudinal regression coefficients for associations between changes in city population and changes in crime rates between adjacent time points. Consistent with most previous research, the cross-sectional coefficients are overwhelmingly positive and statistically significant. Twenty of 24 coefficients are positive and statistically significant. Thus, by the typical mode of analysis, larger population is almost always accompanied by higher crime rates. However, as is also typical, the parallel coefficients for change tell a different story than do those reflecting simple size/rate relationships. Whereas almost all the cross-sectional coefficients in Table 2 are significant, over half of the longitudinal coefficients (10 of 18) fail to reach statistical significance, and all but one of those that are statistically significant are *negative* rather than positive (7 of 8), indicating that increases in population are associated with declines in crime rates while decreases in population are associated with increases in crime rates. Thus, consistent with some past research (Archer and Gartner 1984; Gurr 1981; Gurr and Grabosky 1976; Johnson 1995, 1979; Lodhi and Tilly 1973), change in population is not predictive of directionally corresponding change in crime rates. Hence the apparent anomaly noticed by others—a general positive cross-sectional relationship between population size and crime rates not being matched by a positive relationship between change in size and change in crime rates—is confirmed in these data.

Table 2 Bivariate regression coefficients expressing the linear cross-sectional and longitudinal relationships between crime rates and population size, by year (N = 348)

Crime	Cross-sectional				Longitudinal		
	1960	1970	1980	1990	1960–1970	1970–1980	1980–1990
Homicide	0.10* (0.05)	0.36* (0.08)	0.65* (0.11)	0.67* (0.13)	– 4.61* (1.21)	1.58 (1.98)	– 6.93* (2.64)
Assault	3.50* (0.99)	5.70* (1.62)	5.94 (3.34)	12.49* (5.31)	– 9.00 (23.86)	34.96 (66.02)	– 271.94* (118.78)
Burglary	7.54* (3.06)	26.09* (7.06)	18.98 (10.12)	11.29 (9.29)	– 105.29 (105.49)	– 294.28 (181.04)	– 483.13* (231.81)
Robbery	3.82* (0.46)	17.43* (1.83)	23.08* (3.41)	23.91* (3.53)	– 113.66* (30.58)	– 94.86 (55.37)	– 28.95 (50.02)
Rape	0.36* (0.07)	0.74* (0.16)	1.13* (0.38)	– 0.51 (0.47)	– 1.95 (2.65)	0.76 (7.79)	– 29.74* (11.29)
Auto theft	6.53* (1.48)	21.35* (4.81)	22.77* (5.23)	38.95* (7.35)	– 250.72* (73.89)	157.75 (90.27)	1778.68* (786.60)

* $P < 0.05$ (two-tailed test)

Standard errors in parentheses

However, simple cross-sectional and longitudinal coefficients can mislead. As noted before, they do not allow for non-monotonic associations, they do not rule out extraneous variables that might lead to distorted results, and they may conceal dynamic relationships that occur simultaneously across the three decades, potentially creating a false impression that the cross-sectional results contradict the longitudinal ones.

Therefore, we re-estimated the cross-sectional relationships featured in Table 2 with (1) 11 extraneous control variables in the models, (2) allowing for a non-monotonic relationship by including both a linear term and a squared term for population change. We also re-estimated the longitudinal relationships (1) using a series of pooled cross-section time series models to capture more accurately the longitudinal effects of population changes on crime rates, and (2) allowing for a non-monotonic relationship by including both a linear term and a quadratic term for population change. If the linear term turns out to be significant while the quadratic term is not, this indicates that the relationship between population and crime rate change is linear. However, if the quadratic term is significant and significantly improves the model, then the relationship is curvilinear regardless of the significance of the linear term. We use a likelihood ratio chi-square test to assess whether the quadratic term improves model fit, and we use an *F*-test of model fit for the cross-sectional models.

Table 3 reports a summary of the results of these re-analyses, while Appendix tables 4–7 contain the actual coefficients. The table summarizes the effects of population size on crime rates for the cross-sectional and longitudinal analyses. When significant improvements in model fit ($P < 0.05$) are observed, the sign in the cell indicates the sign of the squared coefficient. Cells with ''ns'' indicate that the inclusion of the quadratic term did not significantly improve model fit over a linear model.

These results challenge our previous results derived from simple analyses and they challenge the extant body of evidence about city size and crime rates. First, whereas simple cross-sectional analysis shows almost all coefficients between population size and crime rates to be significant, half of the cross-sectional coefficients with controls included are now insignificant. And, while the simple analyses

Table 3 Summary of results of cross-sectional (regression) and longitudinal linear and curvilinear relationships (pooled cross-sectional time series regression) with 11 variables controlled ($N = 348$)

Crime	Cross-sectional				Longitudinal
	1960	1970	1980	1990	1960–1990
Homicide	ns	–	–	–	+
Assault	ns	ns	ns	ns	ns
Burglary	ns	ns	ns	ns	+
Robbery	–	–	–	–	+
Rape	–	ns	ns	ns	ns
Auto theft	–	–	–	–	ns

''ns'' indicates addition of squared term does not improve model fit. Tests of model fit for cross-sectional results are conducted with an *F*-test; tests of model fit for longitudinal models are conducted with a likelihood ratio chi-square test

When significant improvements in model fit are observed, the sign in cell indicates sign of the squared coefficient indicate linear relationships between population size and crime rates, analyses allowing for quadratic relationships show the significant coefficients, even the longitudinal ones, to be non-monotonic in form; that is, the squared terms are significant.

The results, therefore, suggest that the widely embraced assumption of a positive monotonic association between city size and crime rates may be misleading. Controlling for extraneous variables and allowing for a quadratic relationship suggests either no significant association or a non-monotonic association between city size and crime rates. When size of population is related to crime rates, it appears that increasingly larger cities, up to a point, have higher crime rates but cities larger than that have lower crime rates.

Further, the typical but counter-intuitive findings like those in Table 2 showing that the associations between change in population and change in crime rates are problematic or when found are negative (increases in population lead to decreases in crime while decreases in population lead to increases in crime) are contradicted by the more sophisticated findings in Table 3 of quadratic relationships in the longitudinal models. Those results show that for three crimes (homicide, burglary, and robbery), any changes in population are likely to result in increases in crime rates of a non-monotonic form.

To make sure these result are not influenced by a few cities in the tails of distributions, we reanalyzed the data omitting cases that might be considered as outliers. The results proved to be robust.

Discussion

With controls included, and using more complex analytic procedures than is customary, including the possibility of quadratic relationships, we still find an inconsistency between cross-sectional associations between population size and crime rates and longitudinal associations for change in population size and change in crime rates (what some refer to as an anomaly). However, the nature of the inconsistency is somewhat different from that usually observed. Instead of a positive, monotonic cross-sectional association of city size and crime rates, we observe non-monotonic relationships, and the longitudinal relationships are generally (3 of 6) non-monotonic in form as well. These results, therefore, cast suspicion on the notion of a positive, monotonic cross-sectional relationship between city size and crime rate, suggested by the urbanism and subcultural arguments. However, the longitudinal findings of a significant quadratic relationship between change in population and change in crime rates generally support the systemic theory, which suggests that any change in population, up or down, will lead to an increase in crime. And, the fact that half of the cross-sectional coefficients and almost all of the longitudinal relationships remain significant with the dominance measure controlled suggests that city size/crime associations are not artifacts. Thus, the results appear to be most consistent with the systemic argument though neither it nor any other extant theoretical formulation appears adequate to account for both the cross-sectional and longitudinal relationships.

It is important to remember, however, that the objective of our analyses has not been to try to test extant theories or to explain patterns of change in crime rates.

Neither has it been to explain cross-sectional associations between population size and crime rates. Rather, we set out to ascertain if there is, in fact, a discrepancy between cross-sectional and longitudinal patterns of association between population and crime rates and if so, then to offer a potential explanation for that discrepancy.

Having documented a discrepancy, albeit in a different form than we expected to find, we now suggest a possible interpretation: The observed differences between cross-sectional and longitudinal patterns probably do not constitute an "anomaly" at all. Rather, it may be a natural, almost inevitable consequence of comparing static and dynamic features of the same phenomenon. The result may be like comparing a snapshot with a motion picture. The processes portrayed by the motion picture will be frozen for any given snapshot, perhaps suggesting that the action is different than it really is. Thus, a snapshot of a runner in motion may show both feet on the ground, one foot in the air, or both feet in the air, depending on when in the motion the picture was taken. All would be consistent with the underlying action that is revealed by the motion picture but all would portray a distorted view of that action. Each would be both correct and misleading. Similarly, it may be perfectly reasonable to find one kind of cross-sectional pattern but a different longitudinal pattern.

Therefore, it is probably not profitable to continue to discuss an "anomaly" between cross-sectional and longitudinal patterns of city size and crime rates, or to refer to it as a "paradox." Rather, the task now is to address the various quadratic patterns that emerge, both cross-sectionally and longitudinally. If our results are accurate and are replicated with other samples of cities, the so-called "facts" of criminology may have to be altered. It may be inaccurate to claim that city size is monotonically related to crime rates or to assume that increases or decreases in population may portend corresponding increases or decreases in crime rates. Present urban theories do not offer accounts that will suffice to explain why city size is linked to crime rates in a curvilinear pattern though the systemic theory may account for why changes in population are linked to changes in crime rates in a positive curvilinear pattern. Clearly, more theoretical work is required.

Limitations

These findings must, of course, be entertained with great caution. First, our sample of cities is restricted. Conceivably, analysis with the complete list of U.S. cities, were it feasible, might produce different results, and it is possible that cities in other parts of the world might show different patterns. Second, our crime data are from official sources. The accuracy of such data depends on the willingness of citizens to activate the police, a characteristic that probably varies among cities, as well as on the policies of local police departments (see Mosher et al. 2002; Tittle and Paternoster 2000, pp. 287–290). It is well known that police data do not come close to representing the actual amount of crime that occurs, especially for offenses such as assault, robbery, and rape and that this difference may not be constant among cities (Mosher et al. 2002). Perhaps victimization or self-report data, had they been available for large enough samples from most of the our cities, would have produced other results. Third, our measure of dominance is not as strong as desired. As noted before, the concept of urban dominance may be too broad and complicated to be captured by a simple indicator of distance between various sized places.

Finally, population and crime rate changes may involve some reciprocal processes that we were unable to take into account. Some evidence suggests that crime, victimization, or general perceptions of crime may influence whether residents move or remain in an area (see South and Messner 2000, for a review). Therefore, though, as our analyses show, population changes can influence the level of crime, it is also possible that crime, in turn, can influence the size of the population. Although our focus here has been on ascertaining the form of relationships between population size and crime (and changes in each) rather than on explaining such a relationship, it is not certain that a potential reciprocal relationship necessarily implies similar forms in both directions. Hence, our findings of a general curvilinear relationship between change in population and rates of three crimes may not be matched by a similar form of relationship between changes in those three crime rates and population size. Since we cannot assess the impact that feedbacks might have on our estimation of the effects of population changes on crime rates, we urge caution in interpretation. Feedback effects of a different form than initial effects would certainly pose a theoretical challenge. Current urban theory does not even fully anticipate or explain our findings, much less anticipating and explaining more complicated possibilities.

Conclusion

Simple analyses confirm an apparent contradiction, which has been noted before, between cross-sectional and longitudinal relationships concerning city population size and crime rates. However, more complex procedures using time series analysis techniques, controlling for various extraneous variables, and allowing for quadratic relationships reveal a different kind of discrepancy. Instead of a simple linear cross-sectional relationship between population size and crime rates, the more sophisticated analysis reveals either no association or a non-monotonic, quadratic one. Similarly, instead of a simple lack of longitudinal relationship or a negative one, the more complicated analysis shows substantial evidence of a quadratic, non-monotonic relationship. However, we contend that these divergent patterns for cross-sectional relative to longitudinal data are not necessarily indicative of an ‘‘anomaly.’’ Instead, they represent different aspects of a dynamic process in need of more extensive theorizing. Finally, the cross-sectional results showing that city size and crime rates are either not linked or exhibit a non-monotonic association call into question one of the accepted relationships in criminology that have long guided thinking about crime.

Appendix

Table 4 Regression coefficients expressing the cross-sectional relationships between crime rates, population size and population size-squared, with controls included, by year

	1960		1970		1980		1990	
	Population	Population^2	Population	Population^2	Population	Population^2	Population	Population^2
Crime rate								
Homicide	0.03 (0.11)	– 0.00 (0.01)	0.46* (0.17)	– 0.01* (0.00)	1.19* (0.22)	– 0.02* (0.00)	1.30* (0.23)	– 0.02* (0.00)
Assault	3.12 (2.35)	– 0.03 (0.03)	– 3.68 (4.25)	0.07 (0.06)	– 11.77 (8.50)	0.20 (0.14)	– 3.51 (12.56)	0.13 (0.20)
Burglary	12.74 (7.38)	– 0.18 (0.11)	0.33 (16.78)	0.04 (0.24)	– 34.90 (22.19)	0.51 (0.36)	19.47 (18.52)	– 0.28 (0.28)
Robbery	9.53* (1.03)	– 0.12* (0.01)	25.70* (3.82)	– 0.26* (0.05)	25.62* (6.86)	– 0.26* (0.11)	33.54* (6.69)	– 0.37* (0.10)
Rape	0.62* (0.19)	– 0.01* (0.00)	0.67 (0.38)	– 0.01 (0.01)	0.53 (0.77)	– 0.01 (0.01)	– 1.59 (1.07)	0.01 (0.02)
Auto theft	12.33* (3.74)	– 0.18* (0.05)	28.29* (12.60)	– 0.43* (0.18)	46.22* (12.97)	– 0.69* (0.21)	101.96* (17.01)	– 1.48* (0.26)

* $P < 0.05$ (two-tailed test)

Standard errors in parentheses

Table 5 Estimates from pooled cross-section time series regression models (N = 348)

	Homicide			Assault		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Population change	– 5.99* (0.06)	– 6.26* (0.04)	– 2.82* (0.31)	– 21.28* (3.96)	– 31.44* (3.42)	8.91 (5.11)
(Population change)^2	5.16* (0.32)	5.78* (0.38)	1.35* (0.36)	– 23.21* (7.32)	– 16.34* (6.96)	6.68 (7.81)
Dominance		0.003* (0.00)	0.002* (0.00)		0.03* (0.01)	– 0.001 (0.01)
South			0.94* (0.16)			39.48* (3.08)
Midwest			– 0.09 (0.12)			– 8.55* (2.57)
West			1.15* (0.13)			32.78* (2.91)
Metro			0.96* (0.09)			34.99* (1.63)
Density change			– 0.00* (0.00)			– 0.01* (0.00)
1960–1970			4.29* (0.37)			– 264.55* (6.23)
1970–1980			3.88* (0.30)			– 161.10* (4.22)
% Black change			0.28* (0.02)			6.62* (0.25)
% Div male change			– 0.24* (0.06)			15.26* (1.16)
% Child change			0.25* (0.02)			3.82* (0.32)
% Age 15–29 change			– 0.05* (0.03)			8.66* (0.41)
Med inc change			0.00 (0.00)			– 0.01* (0.00)
% Poverty change			– 0.14* (0.02)			0.22 (0.28)
Gini change			5.06* (2.35)			– 183.60* (29.48)
% Unemploy change			– 0.17* (0.02)			– 5.27* (0.34)

* $P < 0.05$ (two-tailed test)

Standard errors in parentheses

Table 6 Estimates from pooled cross-section time series regression models (N = 348)

	Burglary			Robbery		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Population change	– 629.80* (35.30)	– 632.34* (35.79)	– 136.98* (33.31)	– 171.43* (3.68)	– 187.91* (3.16)	– 56.52* (7.60)
(Population change)^2	758.04* (47.45)	752.53* (43.48)	164.71* (33.25)	161.62* (5.90)	159.32* (4.93)	58.59* (10.36)
Dominance		– 0.09 (0.06)	– 0.13* (0.02)		0.06* (0.01)	0.02 (0.01)
South			188.50* (11.44)			10.98* (3.46)
Midwest			– 88.37 (9.93)			– 65.28* (3.31)
West			23.79* (13.85)			7.39 (4.13)
Metro			95.61* (8.34)			28.53* (2.92)
Density change			– 0.01* (0.00)			0.00 (0.00)
1960–1970			1155.78* (26.60)			71.72* (6.13)
1970–1980			910.16* (20.56)			41.87* (3.79)
% Black change			34.41* (1.35)			9.88* (0.20)
% Div male change			156.72* (5.15)			3.86* (1.35)
% Child Change			– 3.90* (1.53)			7.79* (0.33)
% Age 15–29 change			30.20* (2.02)			4.88* (0.36)
Med inc change			– 0.01* (0.00)			0.00 (0.00)
% Poverty change			6.94* (1.56)			2.26* (0.34)
Gini change			1495.79* (180.37)			258.71* (50.22)
% Unemploy change			16.14* (1.91)			– 2.05* (0.49)

* $P < 0.05$ (two-tailed test)

Standard errors in parentheses

Table 7 Estimates from pooled cross-section time series regression models (N = 348)

	Rape			Auto theft		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Population change	– 8.20* (0.78)	– 8.47* (0.84)	2.63* (1.04)	409.05* (102.95)	– 422.74* (104.87)	114.93* (26.86)
(Population change)^2	5.50* (0.73)	6.45* (1.08)	– 0.62 (0.86)	– 1024.02** (144.73)	– 1045.04* (186.60)	6.14 (14.08)
Dominance		0.0006 (0.001)	– 0.00* (0.00)		– 0.03 (0.13)	– 0.03 (0.02)
South			4.12* (0.33)			544.31* (12.00)
Midwest			– 2.55* (0.35)			0.21 (7.30)
West			2.99* (0.31)			– 352.44* (13.08)
Metro			5.02* (0.23)			313.15* (8.64)
Density change			– 0.00* (0.00)			– 0.01* (0.003)
1960–1970			– 1.16 (0.87)			– 3968.48* (27.02)
1970–1980			– 3.06* (0.91)			– 4248.63* (25.84)
% Black change			1.28* (0.05)			31.16* (1.08)
% Div male change			8.61* (0.19)			132.61* (5.06)
% Child change			0.03 (0.05)			– 60.54* (1.42)
% Age 15–29 change			1.21* (0.07)			23.85* (1.48)
Med inc change			– 0.00* (0.00)			– 0.05* (0.001)
% Poverty change			– 0.06 (0.06)			28.23* (1.49)
Gini change			1.27 (5.20)			786.00* (170.01)
% Unemploy change			0.15 (0.08)			– 7.06* (1.33)

* $P < 0.05$ (two-tailed test)

Standard errors in parentheses

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