

**ASSESSING THE RELATIVE EFFECTS OF MACRO-LEVEL PREDICTORS
OF CRIME: A META-ANALYSIS**

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By:

Travis C. Pratt

B.A., Washington State University

M.A., Washington State University

Committee: Francis T. Cullen (Chair)
John D. Wooldredge
Mitchell B. Chamlin
David Maume (Reader)

ABSTRACT

American criminological theory and research has traditionally focused on individual offenders. Beginning in the 1970s, however, macro-level (or “ecological”) theory and research emerged (or “re-emerged”) and has since earned sustained criminological attention. Prompted by the development of routine activities theory (Cohen and Felson, 1979), the seminal work of Blau and Blau (1982) on inequality and violent crime, the rediscovery of social disorganization theory by contemporary scholars (e.g., see Bursik, 1986, 1988; Sampson and Groves, 1989; Wilson, 1990, 1996), and a renewed interest in macro-level deterrence-rational choice theory (Becker, 1968, 1978), over 200 empirical studies have since been conducted and published in academic journals in an effort to uncover the correlates of aggregate levels of crime.

Despite the theoretical and empirical advances made in this area over the last couple of decades, there has been a dearth of efforts to synthesize and “make sense” of the existing body of scholarship. The present study subjected the body of macro-level criminological literature to a “meta-analysis”—or “quantitative synthesis”—to determine the relative effects of the various macro-level predictors of crime assessed across empirical studies. Particular attention was paid to the variables specified by seven major macro-level theories of crime: social disorganization theory, anomie/strain theory, absolute deprivation/conflict theory, relative deprivation/inequality theory, routine activities theory, rational choice/deterrence theory, social support/altruism theory, and subcultural theory.

The results indicate that macro-level indicators of “concentrated disadvantage” are among the strongest and most stable predictors of crime across empirical studies.

These include racial heterogeneity (measured as the *percent non-white* and/or the *percent black*), *poverty*, and *family disruption*. Conversely, variables related to criminal justice system dynamics (e.g., *policing effects*, the effects of “*get tough*” *policies*) are among the most consistently weak predictors of crime. Overall, these analyses indicate that social disorganization and absolute deprivation/conflict theories have received strong empirical support across existing studies; anomie/strain, social support/altruism, relative deprivation/inequality, and routine activities theories have received a moderate degree of empirical support; and, deterrence/rational choice and subcultural theories are weakly supported by the body of empirical literature. Finally, the implications of the analysis for future research and public policy development are discussed.

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CHAPTER 1

STATEMENT OF THE PROBLEM

Since its inception, American criminological research and theory has predominantly focused on individual offenders. Criminology grew largely out of the classical school and positivist notions of human behavior in the early 1900s. Both of these perspectives focused on the individual correlates of crime and deviance. Whereas the classical school emphasized the rational decision-making processes of individuals, the positivist school looked for physical and/or psychological explanations of criminal behavior. Despite the different assumptions underlying the classical and positivist schools of criminology, it was their shared focus on the individual that guided much of the subsequent theoretical development in criminology.

As such, most of the major criminological theories covered in books, edited volumes, and introductory level textbooks are individual-level theories (Akers, 1997; Jacoby, 1994; Lilly, Cullen, and Ball, 1995; Siegel, 1998).¹ The majority of empirical research in criminology also treats the individual as the unit of analysis. Consider, for example, the ubiquity of research employing individuals' responses to self-report scales in academic journals. The veritable bevy of studies testing various criminological theories that use the National Youth Survey is further evidence of the tendency in criminology to hone in on the individual dynamics of crime and deviance.

¹ Specifically, Siegel's (1998) introductory text devotes only one chapter of seventeen to "social structure theories" of crime (social disorganization, anomie, and relative deprivation theories). Akers' (1997) review of criminological theories contains only one chapter of eleven that combines social disorganization,

In part as a response to criminology's emphasis on the individual, Shaw and McKay's work and the anomie tradition began by Merton had been important contributions in their time.² These formulations outlined aggregate-level conditions of geographic regions (e.g., urban characteristics) and socio-cultural characteristics (e.g., a society's collective emphasis on material success) that may lead to increased *rates* of crime. As Bursik and Grasmick (1992:ix) note, "with the refinement of survey approaches to data collection and the increased interest in social-psychological theories of control, deterrence, learning, and labeling, the focus of the discipline significantly began to shift from group dynamics to individual processes during the 1960s and 1970s" (see also Andrews and Bonta, 1994; Burgess and Akers, 1966; Hirschi, 1969; Bursik, 1988; Krisberg, 1991; Stark, 1987). Even so, beginning in the late 1970s and early 1980s, macro-level (or "ecological") theory and research reemerged and has since earned sustained criminological attention. Indeed, Bursik and Grasmick (1993:ix) argue that "the pendulum has begun to swing in the other direction, and there has been a relatively recent acceleration in the number of studies that have been conducted with an explicit focus on [macro-level] dynamics."

At least in part, this resurgence of interest in macro-level approaches has been encouraged by four primary contributions. The first was Cohen and Felson's (1979) development of routine activities theory. Their theory grew out of the human ecology approach within sociology, which emphasizes "the interdependence among people . . . and the physical environment, especially as people seek to gain sustenance from their

anomie, and strain theories. Lilly et al. (1997) reserve one chapter of eight for social disorganization theory.

² This is not, of course, to say that the approaches taken by Shaw and McKay and by Merton were merely reactions to the previously articulated individualistic theories of crime. Rather, their work started from a

environment” (Felson and Cohen, 1980:390). Cohen and Felson had a particular focus on how variation in social structures may generate the circumstances for rates of criminal victimization. Rates of victimization, in turn, were viewed in the context of routine activities theory as due to the convergence in time and space of motivated offenders, suitable targets, and the absence of capable guardianship.

Given this framework, Cohen and Felson used macro-level data—in particular, aggregate-level variables linking crime rates to the dispersion of activities away from family and household settings—to predict rates of victimization. For example, variables such as aggregate consumption rates and alcohol intake may be treated as proxies for the availability of attractive targets. Further, variables such as the unemployment rate and the proportion of single adult households may tap into the concept of capable guardianship. In all, Cohen and Felson set forth a parsimonious theory to explain rates of victimization that could be tested at the macro-level with data that were easily accessible to criminologists throughout the 1980s. This, in turn, generated considerable empirical research by scholars well into the 1990s (see Felson, 1993).

The second contribution was the seminal work of Blau and Blau (1982) on inequality and violent crime. Blau and Blau (1982) noted an odd paradox in the United States: that rates of violent crimes tend to be correlated with poverty, but that the United States, an affluent nation, has one of the highest violent crime rates in the world. In addressing this paradox, their approach entailed asking “not what kind of individuals tend to commit violent crimes, but what social conditions make it likely that many people commit them” (Blau and Blau, 1982:115). Their theoretical stance was in contrast to the

different premise altogether: that human behavior was guided primarily by social conditions and/or structural constraints, as opposed to inherent biological drives and inhibitions.

neo-Marxian position that violent crime was a product of absolute deprivation (e.g., poverty). Rather, Blau and Blau (1982) asserted that “relative deprivation”—or “inequality”—between aggregated social groups was the chief predictor of rates of violent crime.

To test this proposition Blau and Blau (1982) used SMSAs as their unit of analysis. They constructed variables, such as the “Gini coefficient” for income inequality, and they used race-based socioeconomic status differentials to proxy inequality or relative deprivation. Their analysis revealed considerable support for the inequality-crime relationship, which tended to “wash out” the effects of poverty rates on crime. This approach was important in two respects. First, the measures used in their study have been widely employed in subsequent research as both key theoretical measures and as control variables (Hsieh and Pugh, 1993). Second, the use of such measures helped to advance criminological theory by identifying new types of macro-level processes that may be related to crime rates (i.e., relative versus absolute deprivation). In so doing, the relative deprivation/inequality theory of crime has been adopted and refined by scholars since the mid-1980s (see, e.g., Currie, 1985, 1996, 1997).

The third factor contributing to the reemergence of macro-level criminology was the rediscovery in the 1980s of Shaw and McKay’s social disorganization theory. Research by scholars such as Bursik (1986, 1988), Sampson and Groves (1989), and Wilson (1990, 1996) helped to revitalize, and partially reformulate and extend, the social disorganization tradition. In particular, two central developments helped to resuscitate this perspective.

First, the scope of the theory was adjusted and expanded to constructs beyond the macro-level components originally specified by Shaw and McKay (e.g., residential mobility, racial heterogeneity, low socioeconomic status, spatial density). New variables were specified—such as rates of family disruption and single-headed households—that not only bolstered the empirical validity of the theory, but also enhanced its theoretical clarity (see, e.g., Sampson, Raudenbush, and Earls, 1997; Sampson and Wilson, 1995). In addition to this theoretical extension, the second development was the study conducted by Sampson and Groves (1989) using data from the British Crime Survey (BCS). In their study, the BCS data, which was originally collected at the individual level of analysis, was “aggregated up” to the neighborhood level to test social disorganization theory. This represented an explicit endorsement of the macro-level approach to the study of crime and deviance.

Finally, brought on by rational choice theorists such as Gary Becker (1968; see also Becker, 1978) and by a concern over what impact the growing level of imprisonment has on crime rates, the late 1970s and early 1980s experienced a renewed interest in deterrence-rational choice theory at the macro level (see, e.g., Blumstein, Cohen, and Nagin, 1978; Wilson, 1983). The deterrence perspective is rooted in the assumption that offenders exercise rational judgement and are keenly attuned to the balance of the potential costs and benefits of criminal acts. The reconsideration of the rational choice theory of crime sparked a number of studies testing the validity of “perceptual deterrence” at the individual level using self-report data (Paternoster, 1987). The popularity of the deterrence perspective also resulted in multiple tests of the theory at the macro-level. In this vein researchers have used measures such as “arrest rates” to proxy

the potential “costs” and probable “risks” associated with crime to predict aggregate crime rates (see, e.g., Greenberg, Kessler, and Logan, 1979; Logan, 1975; Wilson and Boland, 1978). Despite the scarcity of sound research supporting the deterrence perspective (i.e., most research has demonstrated either weak or null results for the key theoretical variables specified by deterrence theory), it still maintained an intuitive appeal to the public and policymakers throughout the 1980s (Greenberg and Kessler, 1982; Lilly et al., 1995). As such, macro-level tests of deterrence theory have continued well into the 1990s (Cochran, Chamlin, and Seth, 1994).

The importance of each of these contributions to the development of and continued interest in macro-level criminological theory and research is twofold. First, all of the researchers stated above used macro-level data to develop, test, or extend a particular criminological theory. Second, and perhaps more important, the researchers behind each of these contributions laid out an empirical blueprint for future researchers to follow using macro-level data to test and/or refine macro-level theories of crime.

ORGANIZING CRIMINOLOGICAL KNOWLEDGE

As a result of these important works, there has been continuing research exploring the macro-level predictors of crime rates throughout the 1980s and 1990s. In mainstream sociology, criminology, criminal justice, and economics journals, there have been over 200 empirical studies conducted attempting to identify the predictors of aggregate crime rates. In the process, new theoretical vistas have been explored, such as institutional-anomie theory (Messner and Rosenfeld, 1994) and social altruism-social support theory

(Cullen, 1994; Chamlin and Cochran, 1997; see also Braithwaite's 1989 theory of reintegrative shaming).

Despite the theoretical and empirical advances made, however, there has been a dearth of efforts to synthesize and "make sense" of the existing body of scholarship. Most articles focusing on macro-level issues begin with a review of existing research, but such reviews are often selective in the studies included and discussed. Even more problematic, the reviews contained in most empirical articles are imprecise in their estimates of the relative effects of theoretically relevant macro-level variables.

Broader reviews are also in short supply. For example, Chiricos's (1987) review of 63 studies addressing the unemployment-crime relationship is generally considered the most comprehensive (however, for other limited reviews see Freeman, 1983; Piehl, 1998). His review covered 42 cross-sectional studies and 21 time-series studies. In all, his survey of the empirical research indicated certain trends in the unemployment-crime literature. For example, unemployment had varying effects on crime across levels of aggregation, where the effect of the relationship appeared to be stronger at lower levels of aggregation (e.g., Standard Metropolitan Statistical Areas—or SMSAs—versus states). The unemployment-crime relationship also seemed to be stronger and more consistent in cross-sectional versus longitudinal designs. Finally, the effect of unemployment varied by type of crime rates; the relationship was generally weaker for violent versus property crime rates.

In spite of its comprehensiveness, Chiricos's (1987) review failed to reach any firm conclusions as to the nature, significance, or strength of the unemployment-crime relationship. In his approach, studies were described discursively and were categorized

roughly by design type (e.g., by cross-sectional versus longitudinal research designs). As a consequence of this method, he was left with no way of statistically assessing the degree to which the effect of unemployment on crime rates varied across various units of analysis and under methodological conditions. For example, is the effect of unemployment on crime significantly conditioned by the unit of analysis, by particular measurement techniques, or between cross-sectional and longitudinal research designs?

A limited attempt to statistically synthesize the aggregate-level research on the effects of poverty and income inequality on crime rates was conducted by Hsieh and Pugh (1993). Their analysis focused on 34 studies that generated 76 zero-order correlation coefficients. Overall, their findings revealed a stable and strong positive association between both poverty and income inequality and aggregate crime rates. The mean correlation for both sets of variables across units of analysis (neighborhoods, SMSAs, states, and nations) and dependent variables (violent versus property crime rates) was typically between .30 and .50.

While it was a useful effort to attempt to establish the relative effects of these two predictors, their study was limited in several respects. First, their analysis was restricted to bivariate relationships; only zero-order Pearson's r estimates were used in their synthesis of research. Thus, their estimates of "effect size" (or "magnitude" of specified relationships) are likely to be inflated because the variation in crime rates explained by other factors was not removed. Second, beyond simple comparisons across different types of dependent variables and units of analysis, the impact of other methodological variations was not explored. Accordingly, factors such as model misspecification error and omitted variables bias plague their aggregated estimates. Further, they have no way

of statistically assessing the influence of such methodological variations on the effect size of their key variables.

Thus, the existing reviews of the effect of macro-level variables on crime rates conducted to date share several limitations that may hinder our understanding of aggregate-level crime. First, they are limited in scope, because the effects of only a few predictors are assessed. Second, and relatedly, the reviews have provided no evidence as to the *relative* effect of certain macro-level variables on crime rates. Third, such reviews fail to adequately reveal the effect of methodological variations on the significance and strength of certain macro-level relationships. For example, which macro-level relationships are significantly conditioned by the unit of analysis, or by different measurement techniques, or by differently specified multivariate models? Finally, and perhaps most importantly, the few reviews conducted so far tell us precious little about the relative validity of macro-level theories of crime.

At this stage, then, there is a need for a *systematic* review of the existing body of macro-level scholarship. This dissertation thus proposes to undertake such a review in the form of a “meta-analysis.” Specifically, three issues will be addressed: (1) the relative strength of the empirical relationship to crime rates of the variables, or “predictors,” included in macro-level studies; (2) the impact of methodological variations that may condition the effect of certain macro-level relationships; and (3) the relative empirical status of the main macro-level theories of crime.

RESEARCH STRATEGY

The technique of meta-analysis, or “quantitative synthesis,” entails “the application of statistical procedures to collections of empirical findings for the purpose of integrating, synthesizing, and making sense of them” (Niemi, 1985:5). In an effort to integrate the findings of multiple tests of a similar hypothesis in a more “objective” manner, the technique of meta-analysis treats each empirical study as the unit of analysis so that researchers may draw inferences based on the effect size (or predictive capacity) of variables (Cohen, 1977; Hunter, Schmidt, and Jackson, 1982).

Thus, meta-analysis exists as a quantitative alternative to traditional “narrative”³ (or discursive) reviews of empirical literature in general. In the specific case of the literature on macro-level predictors of crime, a large-scale meta-analysis has four distinct advantages. First, it provides a more precise estimate of the relationship, across all empirical tests, of theoretical variables to crime. Second, it allows for a multivariate analysis in which the researcher can explore whether the effect sizes of macro-level predictors vary under certain methodological conditions (e.g., in longitudinal versus cross-sectional studies; when variables are operationalized differently). Third, because coding decisions are “public,” the meta-analysis could be replicated by other scholars. Fourth, the database is not static but dynamic: as additional studies are published, they can be added to the sample of studies and the relationships can be reassessed.

Although meta-analytic methodology is most commonly applied in the behavioral sciences (e.g., psychology, see Wolf, 1986), over the last two decades meta-analyses have become more frequent in criminal justice and criminology. Researchers have used the

³ The term “narrative” review is used to describe the traditional literature review in this case. In this approach, studies are described discursively and given relative weight and importance according to the reviewer. Further, estimates of the “magnitude” of key theoretical relationships is typically overlooked in such reviews; and, if examined at all, such estimates are generally crude and imprecise.

method primarily to assess either the quality and/or efficacy of correctional treatment interventions (Andrews, Zinger, Hoge, Bonta, Gendreau, and Cullen, 1990; Izzo and Ross, 1990; Lipsey, 1992; Lipsey and Wilson, 1993), or to uncover and rank order certain predictors of individual-level crime and/or recidivism (Bonta, Law, and Hanson, 1998; Gendreau, Little, and Goggin, 1996; Loeber and Stouthamer-Loeber, 1986). Recent efforts have also applied the method to criminal justice system dynamics such as racial sex disparities in sentencing (Daly and Bordt, 1995; Pratt, 1998), attitudes toward victims of sexual assault (Whatley, 1996), the impact of jury size on jury behavior (Saks and Marti, 1997), and the cost-effectiveness of prison privatization (Pratt and Maahs, 1999). Meta-analysis has even been used to assess the empirical status of certain criminological theories (Pratt and Cullen, forthcoming; Walters, 1992).

The appearance of meta-analyses in books, edited volumes, and academic journals is a relatively recent development in criminal justice and criminology. Even so, the body of meta-analytic literature in the fields of crime and deviance has been growing steadily since the late 1980s. Indeed, it is reasonable to contend that there seems to be a movement in criminal justice and criminology toward organizing existing knowledge more systematically. The current study, therefore, would extend this trend to macro-level studies of crime. In so doing, this meta-analysis would be conducted in two stages.

First, the variables used in macro-level studies of crime would be ranked according to the effect size of their relationship to crime. This would allow “tiers” of predictors to be established which would aid in the accomplishment of two objectives: (1) it would help to ensure that future macro-level studies are not potentially misspecified

(i.e., failing to measure the strong predictors of crime); and (2) it would assist in the clarification of the relative predictive power of the major macro-level theories of crime.

The second stage of the meta-analysis would entail quantitatively examining the degree to which the effect size of the relationship to crime of key theoretical variables is conditioned by methodological variations. This has been done only to a limited extent in meta-analyses in criminal justice-criminology (see, e.g., Lipsey, 1992; Pratt, 1998; Pratt and Cullen, 1999). Yet, it is this relatively unexplored facet of the technique of meta-analysis that is considered by certain experts on the method as among its more important potential contributions (Rosenthal, 1984; see also Pratt and Holsinger, 1999).

The methodological controls to be assessed fall under five broad categories. First, the method of operationalization of key theoretical variables will be explored. For example, this would reveal whether the effect size of a variable such as the “racial composition” of a city on crime rates is higher or lower when measured as percent black, percent non-white, or as an index of racial heterogeneity. The second category of methodological controls would be model specification and research design. Here it would be explored whether the effect size of predictor variables significantly differ across factors such as between cross-sectional and longitudinal research designs, or between studies that do or do not control for certain theoretical variables (e.g., variables from a major competing theoretical paradigm). The third category would assess the influence of sample characteristics, such as the level of aggregation in studies (e.g., neighborhoods versus SMSAs or states), on the effect size of variables. The fourth set of methodological controls would be determining the effect size of macro-level predictors for particular dependent variables (e.g., what is the effect size of “arrest rates” on violent offenses

compared to property offenses?). Finally, the meta-analysis would examine the overall predictive power of the multivariate models for each of the independent studies. This would help to determine how whole sets of theoretical variables fair under the methodological characteristics outlined above, and help to provide insight as to the relative predictive power of the major macro-level theories of crime.

THE PLAN OF THE DISSERTATION

Given the objectives of the current study, this dissertation will proceed in Chapter 2 to provide a more detailed discussion of the issues surrounding the technique of meta-analysis. Its potential advantages and limitations will be discussed in comparison to the traditional narrative review of research literature. Further, the major critiques of meta-analysis will be outlined (e.g., that using published studies is biased toward statistically significant findings; that empirical studies are too methodologically diverse to be synthesized; the problem of “independence” across empirical studies). Finally, the methods for either eliminating or reducing the potential biases associated with such critiques will be discussed.

Chapter 3 provides a discussion of the eight major macro-level theories of crime. These theories include social disorganization theory, anomie/strain theory, the conflict/Marxian model of absolute deprivation, the relative deprivation/inequality paradigm, routine activities theory, deterrence/rational choice theory, the emerging social support-social altruism theory, and subcultural theories. Each of these theoretical perspectives will be discussed in terms of: (1) their key propositions and substantive

content, and (2) the variables specified by the theory and how they are typically measured in empirical studies.

Chapter 4 details the methodological procedures for the meta-analysis. Included within this section is a discussion of the sampling criteria, the effect size estimates to be used in the analysis, the controls for methodological variations across studies, the techniques for statistical analysis, and the diagnostic procedures that will be employed for reducing potential biases.

Chapter 5 contains the results of the meta-analysis, which are presented in two stages. First, the mean effect size estimates for each macro-level predictor of crime are calculated. These estimates may then be interpreted as proxies for the relative “strength,” or predictive power, of each variable. Second, the influence of methodological variations will be assessed for each of the overall mean effect size estimates. These analyses will provide insight as to the relative “stability” of the various macro-level predictors.

Given these two sets of analyses, Chapter 6 provides a discussion of the empirical status of each of the major macro-level criminological theories. Drawing on the relative strength and stability of the variables specified by each theory, conclusions will be made in terms of the extent to which the theory has been tested, what those tests reveal about the validity of each theory, and where future criminological research may be directed. Finally, Chapter 7 provides concluding remarks, with a discussion of the implications of this research for both the direction of independent studies and quantitative syntheses of criminological research to come.

CHAPTER 2

ORGANIZING KNOWLEDGE THROUGH META-ANALYSIS

The empirical status of the research on any particular topic in criminal justice or criminology is generally established via the traditional “narrative review” of the research literature. In this method studies are described discursively, categorized, and given relative importance according to the reviewer. Although useful, this approach has its limits. Aside from being based, at least in part, on the qualitative judgements of those conducting the review to determine what existing studies actually find, narrative reviews often only provide crude estimates of the degree to which certain key variables are related. In particular, the “magnitude” of empirical relationships is rarely explored.

As stated earlier, an alternative to the traditional narrative review is “meta-analysis,” or “quantitative synthesis.” This approach entails “the application of statistical procedures to collections of empirical findings for the purpose of integrating, synthesizing, and making sense of them” (Niemi, 1986:5). Meta-analysis attempts to integrate the findings of multiple independent tests of a similar hypothesis in a more “objective” manner by treating the independent study as the unit of analysis. Researchers may then draw inferences based on the effect size (or predictive capacity) of relationships between variables (Cohen, 1977; Hunter, Schmidt, and Jackson, 1982).

As stated previously, meta-analytic methodology is most commonly applied in the behavioral sciences (Wolf, 1986). Reflecting this trend, over the last two decades meta-analyses have become more frequent in criminal justice and criminology. Researchers have used meta-analysis to synthesize the research literature on the quality and/or

efficacy of correctional treatment interventions (Andrews, Zinger, Hoge, Bonta, Gendreau, and Cullen, 1990; Izzo and Ross, 1990; Lipsey, 1992; Lipsey and Wilson, 1993); to uncover and rank order certain predictors of crime and/or recidivism (Bonta, Law, and Hanson, 1998; Gendreau, Little, and Goggin, 1996; Loeber and Stouthamer-Loeber, 1986); and to explore criminal justice system dynamics such as racial sex disparities in sentencing (Daly and Bordt, 1995; Pratt, 1998), attitudes toward victims of sexual assault (Whatley, 1996), the impact of jury size on jury behavior (Saks and Marti, 1997), the cost-effectiveness of prison privatization (Pratt and Maahs, 1999), and even the empirical status of certain criminological theories (Pratt and Cullen, 1999; Walters, 1992).

Despite this expansion in its application and the growing roster of researchers employing the technique, some lingering doubts remain about the utility and validity of meta-analysis in criminal justice and criminology. To be sure, methodological inconsistencies have yielded independent meta-analyses of the same set of literature that reach radically different conclusions (e.g., see Andrews et al., 1990; Lab and Whitehead, 1990; Whitehead and Lab, 1989).⁴ The primary result of such discrepancies has been the charge that many meta-analyses are methodologically “soft” (Logan and Gaes, 1993). Even so, meta-analyses are gaining in popularity among researchers and policymakers largely due to their parsimonious format: a large body of literature can be summarized in relative short order (Hunter and Schmidt, 1996).

⁴ The mean “treatment” effect size estimates for the Whitehead and Lab (1989) and Andrews et al. (1990) studies were similar—both revealed a treatment effect of approximately .20. Their disagreement centered primarily around the substantive interpretation of a treatment effect of that size (i.e., whether a 20 percent reduction in recidivism is “good enough”).

Given the emergence of this new method and its potential for organizing our knowledge of the causes of crime and the criminal justice system's responses to it, there are three major components to this chapter. First, the potential advantages of meta-analysis relative to the traditional narrative review of literature are discussed. Second, a discussion of the potential limitations and biases of meta-analysis is provided. This discussion is also placed in the context of how researchers have attempted to reduce and/or correct the potential problems facing the technique. Finally, based on the potential advantages and disadvantages of meta-analysis, a framework for maximizing the level of analytical and methodological rigor in a meta-analytic study is set forth. This framework, in turn, will serve to guide the present meta-analysis of macro-level predictors of crime.

THE POTENTIAL ADVANTAGES OF META-ANALYSIS

Wolf (1986) notes that although hundreds of studies in the social and behavioral sciences have examined a wide array of research questions, these studies often use different samples, variables, and methods, and they also often draw conflicting conclusions. Such inconsistencies provide no clear policy implications from the body of work on a particular subject and instead simply usher in calls for additional research (Kulik, 1983). Furthermore, as stated above, traditional narrative reviews of research literature largely depend on the subjective judgements, preferences, and biases of the reviewers (Wolf, 1986). Conflicting interpretations of the existing empirical evidence on a given research question are not uncommon, and even consistent interpretations could conceivably be attributable to similar biases and misreadings of the literature (Glass, 1977; Jackson, 1980; Light and Smith, 1971; Pillemer and Light, 1980).

In response to the apparent paradox of the traditional narrative review of scientific literature having been typically conducted in an unscientific manner, Glass, McGaw, and Smith (1981) contend that the technique of meta-analysis can bring the same level of analytical and methodological rigor found in independent studies to the literature review. The authors argue that “Contemporary research reviewing should be more technical and statistical than it is narrative The findings of multiple studies should be regarded as a complex data set, no more comprehensible without statistical analysis than would hundreds of data points in one study” (Glass et al., 1981:12).

This call has been heeded only to a limited extent in criminal justice and criminology. It is still the normative practice to undertake a narrative-review approach with regard to most research areas, including the criminal justice policy literature (Walker, 1994; Walker, Spohn, and Delone, 1996) and the empirical status of criminological theories (Akers, 1997; Burton and Cullen, 1992; Kempf, 1993; Lilly, Cullen, and Ball, 1995). There is, however, an emerging trend toward quantitatively synthesizing existing knowledge in criminal justice and criminology through meta-analysis. This trend is largely due to the perception that relative to the traditional narrative review of research literature, meta-analysis may have a number of distinct advantages.

Precise Estimates of Effect Size

First, meta-analysis generally provides a single, precise estimate of the magnitude (or, “effect size”) of a specified empirical relationship between two variables. In contrast, narrative reviews of literature, at best, may employ a form of “vote counting”

(Burton and Cullen, 1992; Chiricos, 1987; Kempf, 1993). In this method, the statistical significance-nonsignificance results of key theoretical relationships across studies are tallied up into univariate percentages (e.g., the percent of published studies that reveal support for a specified relationship). Thus a key advantage of meta-analytic reviews of research literature is that they may go beyond the simple significant-nonsignificant dichotomy to enable comparisons of the relative *strength* of key theoretical relationships.

Assessing Methodological Variations

Meta-analysis is also capable of yielding the effect size of empirical relationships under different methodological conditions. For example, there are certain controversies surrounding the empirical validity of a link between unemployment and crime at the aggregate level (e.g., see the discussion by Pratt and Lowenkamp, 1999). In particular, questions remain concerning how the unemployment-crime (U-C) relationship varies across units of analysis (e.g., cities versus states), for different types of crimes (e.g., violent versus property crimes), and across different types of research designs (e.g., cross-sectional versus longitudinal designs). To date, the narrative reviews of the U-C literature (e.g., see Chiricos, 1987) have only highlighted “trends” in how such methodological variations may influence the U-C relationship. For example, the effect of unemployment rates on crime rates “appears” to be stronger at the city level, for property crimes, and in cross-sectional research designs. A meta-analysis of the same body of literature would go beyond such clinical “eyeballing,” and would be able to assess whether these apparent differences are statistically significant, and would provide precise

estimates of *how much* the U-C relationship varies according to methodological differences across empirical studies.

“Public” Coding Decisions

As with any empirical study, the coding decisions made in a meta-analysis are “public.” In short, how studies are coded and categorized may be listed and described in the same manner as the empirical studies being reviewed. This facet of meta-analysis makes such reviews amenable to replication. This is especially important when scholars come forth who are skeptical of the results of the meta-analysis. These researchers may take the “blueprint” of coding decisions from the original meta-analysis, make any changes or additions deemed necessary, and re-do the analysis accordingly.

Dynamic Database

Related to the advantage of “public” coding decisions, the database for a meta-analysis is not static, but rather it is dynamic. As new studies are published, they can be added to the database of empirical studies. Upon doing so, the empirical relationships formerly under investigation may be reassessed. The new analyses may also take into account any new methodological variations that may have emerged following the initial meta-analysis.

CRITIQUES OF META-ANALYSES IN CRIMINAL JUSTICE AND CRIMINOLOGY

Not all scholars are firmly convinced of the potential advantages of meta-analysis for integrating and synthesizing research in criminal justice and criminology. This skepticism is due in no small part to the primary subject matter upon which meta-analysis has been applied in the field of crime and deviance: the effectiveness of correctional treatment/rehabilitation (e.g., see Andrews et al., 1990; Lipsey, 1992; Lipsey and Wilson, 1993).

The effectiveness of correctional rehabilitation has been essentially contested since the early 1970s (Cullen and Gilbert, 1982; see also Pratt, Maahs, and Stehr, 1998). Although the philosophy and practice of rehabilitation took a beating in the late 1960s and 1970s, a revival of sorts has taken place since the mid-1980s (1982; Gendreau and Ross, 1987). Palmer (1992:5) notes that much of the credit for the return of the rehabilitative ideal should go toward the meta-analyses of treatment effectiveness and how they have contributed to the “scientific legitimacy” of correctional treatment interventions.

The renewed interest in rehabilitation, however, has been met with considerable opposition. The long-held, almost biblical faith in the *ineffectiveness* of correctional treatment held by certain researchers has sparked a number of critiques against rehabilitation’s primary weapon: the meta-analysis.⁵ Although these critiques vary in their validity, scope, and content, they tend to fall into one of three broad categories: (1) the failure to include all relevant studies in the meta-analysis (i.e., the “file drawer problem”); (2) the inappropriate inclusion of fundamentally different studies into the

⁵ The following discussion regarding the ideological sources of the critiques of meta-analysis is intended only to provide a context for understanding how the critiques developed. Accordingly, the primary focus of this chapter is to evaluate the validity of the critiques of meta-analysis themselves, irrespective of their origin.

same meta-analysis (i.e., the “apples and oranges problem”); and (3) the inclusion of multiple estimates from a common source (i.e., the “independence problem”). Each of these sets of critiques are discussed below in terms of their sources and validity, and in terms of how meta-analysts have responded to them.

Unmeasured Studies: The “File Drawer Problem”

Critics of meta-analyses contend that academic journals (and their editors) may be biased in favor of statistically significant findings, and therefore that literature reviews may not uncover every study of a particular hypothesis that has been conducted (Glass et al., 1981). Rosenthal (1979) refers to this as the “file drawer problem” due to the tendency of studies failing to reject the null hypothesis to be buried away in file drawers. The omission of null-model studies may limit the utility of meta-analyses that are conducted exclusively on published research. Anti-meta-analysis scholars have extended this argument to hold that published research represents only a small fraction of existing tests of any given research question, and that the error associated with the publishing-bias is non-random (e.g., see Logan and Gaes, 1993). This systematic error, argue critics, dooms meta-analyses to contain an *a priori* bias toward statistically significant relationships.

This critique emerged in criminal justice and criminology largely in response to the meta-analyses of the correctional treatment literature that indicated a statistically significant “treatment effect” under certain conditions (see, e.g., Andrews et al., 1990; Lipsey, 1992). Anti-rehabilitation advocates argued that the positive findings of the

meta-analyses of the treatment literature were due, at least in part, to the significance-bias of published research (Kraemer and Andrews, 1982).

Criminologists are likely to continue to debate the relative competence or ineptitude of state-based crime control efforts (cf. Cullen, Wright, Brown, Moon, Blankenship, and Applegate, 1998; Whitehead and Lab, 1989). Nevertheless, the fact remains that the “file drawer problem” critique must be viewed as methodologically valid if a substantial number of tests of a particular hypothesis could have conceivably been excluded from the sample in a meta-analysis. In response to this criticism, Rosenthal (1979) developed a statistical estimate for the number of unmeasured studies that would have to contain a “null finding” to reverse a conclusion that a statistically significant relationship exists. Put differently, this statistic, called the “Fail Safe N” (see also Cooper, 1979), represents “the number of additional studies in a meta-analysis that would be necessary to reverse the overall probability obtained from our combined test to a value higher than our critical value for statistical significance, usually .05 or .01” (Wolf, 1986:38).⁶

While the debate surrounding the “file drawer problem” still continues, the “Fail Safe N” statistic has added a degree of methodological sophistication to meta-analyses that has pacified portions of the anti-meta-analysis community (Presby, 1978). A more damaging set of critiques, however, is directed toward the *overall* level of analytical and methodological rigor in meta-analyses, which may be the greatest barrier to the wider

⁶ The equations provided by Rosenthal (1979) for the “Fail Safe N” estimates first require converting whichever effect size estimate being used into a z-score. Then, the following equations yield the estimate at the .05 and .01 levels:

$$N(.05) = (\sum z\text{-scores}/1.65)^2 - N$$
$$N(.01) = (\sum z\text{-scores}/2.33)^2 - N$$

acceptance of meta-analytic methodology in criminal justice and criminology. Specifically, critics contend that (1) the results of meta-analyses are uninterpretable because “bad” studies are included with “good” studies, and (2) logical conclusions cannot be drawn from meta-analyses because of variations in the measurement techniques, samples, and model specifications across empirical studies (see the discussion by Wolf, 1986). These critiques can be combined under the heading of the “apples and oranges problem.”

Methodological Diversity: The “Apples and Oranges Problem”

Logan and Gaes (1993:247) have clearly articulated the problems associated with attempting to deal with the methodological diversity across empirical studies in meta-analyses by referring to the technique as “some kind of alchemy.” In critiquing the meta-analyses of the effectiveness of rehabilitation they state that meta-analytic methodology simply represents “an attempt to turn the lead of inadequate experiments into the gold of established knowledge” (Logan and Gaes, 1993:247).⁷

This critique of meta-analyses, often dubbed the “apples and oranges problem,” also grew out of the correctional treatment effectiveness debate and is grounded in the belief that empirical studies are too methodologically diverse to be coherently combined into the same sample. In particular, critics adhering to this potential limitation of meta-analysis contend that, even in the case of experimental or quasi-experimental research designs, differences in factors such as sample composition, treatment approaches, follow-

⁷ To their credit, Logan and Gaes (1993:246) are honest about their ideological bias in their critique of meta-analysis. The authors admit up front that “we intend to argue that regardless of what such research shows [regarding the effectiveness of rehabilitation], punishment is preferable to rehabilitation as an aim of criminal justice.

up periods, and recidivism measures across individual treatment studies make any and all comparisons across such studies inappropriate.

With regard to correlational research designs—such as those typically used in empirical tests of criminological theories—the “apples and oranges” problem is compounded, critics argue, by differences in model specification. The magnitude of the relationship between key theoretical variables may differ across studies in a systematic manner according to which control variables are, or are not, included in the multivariate statistical model. Thus, mean effect size estimates generated from a pool of studies using correlational research designs may be biased by the inclusion of artificially inflated or deflated effect size estimates from improperly specified statistical models, since, as stated above, experimental or even quasi-experimental research designs are rarely used in criminological research.

This is admittedly a legitimate basis for exercising caution in interpreting the results of non-experimental empirical studies. Even so, it does not necessarily render correlational research designs impotent or incomparable across studies. It is important to note that considerable dissimilarities also exist among the other units of analysis typically investigated by scholars in criminal justice and criminology. For example, individuals, neighborhoods, cities, SMSAs (Standard Metropolitan Statistical Areas), states, and nations differ in several substantively important ways when it comes to predicting crime and delinquency. In correlational designs, key relationships are evaluated/isolated by controlling statistically variations in the theoretically relevant characteristics of whatever the unit of analysis may be that could influence the study’s dependent variable(s). Recent meta-analyses have followed this trend and have begun to statistically assess and control

variations associated with sample characteristics, research designs, and measurement issues across studies in a meta-analytic sample (Pratt, 1998; Pratt and Cullen, forthcoming). Thus, the “apples and oranges problem” should only be viewed as a problem in instances where there is an absence of statistical controls for methodological variations across studies that could influence the meta-analysis’s overall effect size estimate(s) (Wolf, 1986).

Multiple Estimates: The “Independence Problem”

A final set of critiques of meta-analysis has been raised not necessarily by ideologues opposed to rehabilitation but rather by meta-analysts themselves. In an effort to improve the precision of effect size estimation, meta-analytic researchers have noted that, in certain contexts, multiple effect size estimates may be provided in a particular empirical study. This can occur for two reasons. First, an empirical study may estimate effects for multiple dependent variables (e.g., violent crime, property crime). Second, researchers may conduct multiple analyses on different subsamples (e.g., males, females). In either case, the result is a potential problem of a lack of statistical independence of effect size estimates within such studies. In particular, multiple effect size estimates gathered from the same study may be similar by virtue of being produced by the same data set. This may deflate the variance estimates associated with the effect size estimates, which would make finding statistically significant aggregated effect size estimates artificially easier (i.e., it may produce bias toward statistical significance).

There are, however, methods that may be employed to avoid the potential “independence problem.” The most straightforward approach would be that, when faced

with multiple effect size estimates reported in an empirical study, researchers could choose one effect size estimate per study according to some predetermined criteria. Although simple, this approach is problematic in two respects. First and most important, selecting only one effect size estimate from each study would severely limit the possibility of examining how methodological variations across studies potentially influence the effect size estimates. For example, in choosing only one effect size estimate substantively important information may be lost as to whether the effect size of variables differ according to factors such as how they are measured, or between cross-sectional and longitudinal research designs. This loss of information is particularly problematic since one of the potential strengths of meta-analysis may be its ability to uncover how certain methodological approaches may be biased toward a particular empirical outcome (e.g., see the discussion by Wolf, 1986).

Second, it would be difficult to develop a “rule” that would guide which effect size estimate should be selected from any one empirical study. Single data sets are used in multiple published studies, each of which potentially includes a unique set of variables in the multivariate analyses that are presented in the articles’ tables. Selecting only one effect size estimate from these different analyses could introduce, wittingly or unwittingly, a “researcher” bias.

As an alternative to choosing one effect size estimate from studies reporting multiple estimates, methods have developed to correct for the potential independence biases that may emerge from selecting multiple estimates from the same study. These methods, referred to as “random” and “fixed” modeling techniques, may (1) allow for multiple effect size estimates from a particular study to be included in the meta-analysis,

and (2) control for potential biases in estimation due to within-study similarities across effect size estimates (see Hedges, 1983; Raudenbush, 1994; Raudenbush, Becker, and Kalaian, 1988). The appropriateness of each technique, however, is less clear in the context of subjecting criminological theories to a meta-analysis. Thus, the following is a discussion of the contexts in which each method of correcting the independence problem may be most appropriate.

Random Effects Models. As stated above “random effects” statistical models are intended to allow for multiple effect size estimates from a particular study to be included in a meta-analysis, and to control for potential biases in estimation due to within-study similarities across effect size estimates. Random effects techniques for independence correction generally proceed in two stages. First, a “within-study” model is estimated to account for the lack of statistical independence brought on by multiple effect size estimates from the same study. Second, the “corrected” estimates from each of the within-study models are used to calculate “between-study” estimates of the aggregated effect size of the relationship under investigation.

The logic underlying the within-study correction procedure is seen in the following equation (equation 1):

$$\text{Mean ES} = \gamma + b(\text{Vsc}) + b(\text{ESa}) + e \quad [1]$$

In equation 1, the estimated aggregate mean effect size (Mean ES) is seen as a function of an estimated intercept (γ), slope estimates for the vector of controls for study characteristics (Vsc) and the aggregated effect size estimates (ESa), and an error

component generated by the unobserved effects of the lack of statistical independence (e). The error component, which is often referred to as a “random component,” is viewed as being produced by the effects of the unobserved characteristics of the studies being synthesized.

The random component generated by each of the within-study models is then used in the second step in the random effects method of correcting for the potential problems caused by the lack of statistical independence. In the between-study model, the intercept generated by equation 1 (γ) may then be treated as a “second level” predictor of the effect size estimates:

$$ADJes = \gamma' + b\gamma + U \quad [2]$$

In equation 2, the mean effect size estimate that has been adjusted for the lack of statistical independence (ADJes) is seen as a function of an estimated constant (γ'), adjustments made for the slope estimates of the random effect (γ), and a random error component that is assumed to be equal to zero (U).

This method is most useful when the research question(s) involves what the aggregated effect of a *particular* independent variable may be on *multiple dependent variables*. For example, in the behavioral sciences, researchers may wish to estimate the overall effect of one type of treatment (e.g., cognitive-behavioral treatment) on multiple outcomes (e.g., deviant behavior, school performance, delinquent attitudes) at the same time. In essence, using a random effects statistical model would indicate that the researchers are not necessarily concerned with why the effect of the treatment given

would differ across the multiple outcome measures, but rather the concern would be only to control for such differences in estimating one unified effect size estimate for the treatment.

In essence, the use of a random effects statistical model would indicate that the researcher is not necessarily concerned with why the effect of the treatment given would differ across the multiple outcome measures. Rather, the concern would only be to control for such differences in estimating one “unified” effect size estimate for the treatment. This would also indicate that the meta-analyst is largely unconcerned with assessing the degree to which the effect size estimates are conditioned by methodological variations across studies. Indeed, the possibility of doing so is essentially eliminated upon the construction of the single estimate. If, however, a researcher is interested in why the effect size of a specified relationship differs under certain methodological conditions (e.g., for different dependent variables, using different measures of key theoretical variables), and what the mean effect size is across such methodological variations, an alternative approach is needed: a “fixed effects model” (Hedges, 1994).

Fixed Effects Models. In this approach, the researcher assumes that multiple effect size estimates gathered from the same study are potentially similar. It differs conceptually from the random effects model in that there may be theoretical reasons to believe why a relationship between two variables will systematically differ under different conditions beyond simply the unobserved characteristics of the studies being synthesized (Kalaian and Raudenbush, 1996). In such instances, the error associated with the problem of independence is patterned after the methodological variations across the studies themselves, rather than being randomly generated.

For example, in their meta-analysis of the treatment effectiveness literature, Andrews et al. (1990) provided a set of “principles of effective intervention” with offenders. In particular, they posited that correctional interventions are most likely to produce significant reductions in recidivism when treatment efforts: (1) are focused on higher-risk offenders, (2) address those factors known to be related to recidivism, and (3) are behavioral in nature. Andrews et al. (1990) then went on to estimate separate effect size estimates for those types of treatment interventions that followed these principles of effective intervention, and compared them to those that did not. In essence, the authors of this study assumed that the effect size of treatment efforts would differ under certain conditions, and that these differences were not random, but predictable for theoretical reasons (i.e., “fixed”).

The way this approach is translated into situations where multiple effect size estimates are gathered from a particular study by constructing what have called “independence-adjusted effect size estimates” (see Pratt and Cullen, forthcoming). To compute these estimates, it is necessary to statistically model the interdependencies between the coefficients by removing the variation in a set of coefficients that could be explained simply by their production from a common source. This process is similar to removing serial correlation (i.e., shared variation across successive residuals) in OLS regression analysis (Hanushek and Jackson, 1977).

First, estimates of serial correlation are calculated for each data set (or, in the case of aggregate research, the level of aggregation) where multiple effect size estimates are drawn using equation 3:

$$\Sigma[e(t) - e(t-1)]^2 / \Sigma[e(t)]^2 = 2 - 2\rho \quad [3]$$

In this instance, $e(t)$ and $e(t-1)$ represent successive error terms estimated from OLS regression models predicting the effect size estimates using the methodological control variables as independent variables. Upon calculating values for the left side of the equation (which estimates values for the Durbin-Watson test statistic), the next step is to solve for “ ρ ” (rho), the estimate of serial correlation. A weight is then created, $1 - \rho$, to give lesser weight to those effect size estimates with large degrees of serial correlation. In so doing, the resulting effect size estimates contain uncorrelated residuals. As an additional consequence, the weighting procedure, which essentially reduces the degrees of freedom in the construction of the standard errors, widens the error variances for the predictor domains; thus, tests for statistical significance with these estimates will be more conservative.

This “weight” is then used in separate “fixed effects” analyses of the effect size for key theoretical relationships under the different methodological conditions. For example, this approach would allow a researcher to determine the mean effect size of a variable such as “inequality” on crime rates separately across methodological variations such as states versus SMSAs, or for violent versus property crime rates. In short, this procedure allows for the correction of the potential bias that a lack of statistical independence may have produced that: (1) avoids the arbitrariness of choosing one effect size estimate per data set, and (2) allows for the precise estimation of effect sizes across theoretically-relevant methodological variations.

Summary on Critiques of Meta-Analysis

It is true that, while potentially useful, there have been a number of criticisms leveled at the technique of meta-analysis. Most of these critiques grew out of the “what works” in correctional treatment debate. At minimum, the underlying motivation behind the articulation of the “file drawer” and the “apples and oranges” problems was more ideological than empirical. Regardless of how potentially disingenuous this debate may be, the chief concern of this dissertation has to do with the strengths and weaknesses of the meta-analytic method. Accordingly, meta-analysts have taken the critiques and potential limitations of the meta-analysis seriously. The result has been the development of a growing arsenal of sound statistical methods for correcting such potential biases.

It is this slice of realism that keeps critics of meta-analyses from being monolithically pessimistic about the validity of the results provided by the technique. Indeed, even Logan and Gaes (1993:251) are willing to show respect to certain meta-analyses that are “more sophisticated statistically, more rigorous methodologically, and without the evangelical zeal of other meta-analytic research.” For example, Lipsey’s (1992) meta-analysis of over 400 evaluations of treatment programs for juvenile delinquents contained rigid sampling criteria and weighting procedures, an extensive literature search, controls for methodological variations (e.g., sample characteristics, variable measurement), and multivariate statistical techniques (multiple regression) in the analysis. Not surprisingly, Lipsey’s (1992) study was spared from the carnage that Logan and Gaes (1993) attempted to inflict on the meta-analytic method.

Thus it appears that what separates those meta-analyses that survive the intense ideological criticisms from the rest of the pack are those that are also capable of

withstanding a detailed methodological assault. In short, if the technique of meta-analysis is going to be expanded in criminal justice and criminology and gain a measure of methodological legitimacy, future meta-analytic studies will need to maximize their levels of analytical and methodological rigor. The remainder of this discussion is devoted to constructing a framework for maximizing the level of analytical and methodological rigor in a meta-analysis. This framework will then serve as a guide for the proposed meta-analysis of macro-level predictors of crime.

IMPROVING ANALYTICAL AND METHODOLOGICAL RIGOR IN META-ANALYSIS

There are a number of steps that researchers can take to improve the level of analytical and methodological rigor of their meta-analyses. The techniques outlined below do not necessarily represent an exhaustive “how to” list for a valid and reliable meta-analysis. Rather, they may be viewed as a brief set of guidelines for minimizing estimation errors and, in turn, for increasing the probability that a meta-analysis will accurately reflect the empirical status of the research questions under investigation and withstand inevitable criticism (both ideological and methodological).

Sampling

Both of the major sets of “problems” associated with meta-analyses (the “file drawer” and “apples and oranges” problems) can be minimized through careful sampling procedures. To reduce the probability that relevant tests of a particular hypothesis have

not been excluded from the sample for the meta-analysis, the guidelines set forth by Petrosino (1995) are instructive. He suggests that in addition to the traditional electronic database searches, researchers should also examine prior narrative and meta-analytic reviews of the research hypothesis, conduct manual hand searches of major journal volumes, examine published bibliographies and solicitations, contact major investigators, and compile citations from other bodies of literature (Petrosino, 1995). In addition, Glass et al. (1981) suggest that meta-analysts, prior to even starting the literature search, should establish theoretically-relevant criteria for which types of studies should, and should not, be included in the sample. This may help to minimize the heterogeneity in research approaches taken across empirical studies, and therefore reduce the degree to which the “apples and oranges” critique would be applicable.

Controls for Methodological Variations

Consistent with the study conducted by Lipsey (1992), a well-done meta-analysis should make every attempt to quantitatively assess the degree to which methodological variations across studies influence the overall effect size estimate (the meta-analytic equivalent of the dependent variable). For example, in a recent meta-analysis on the empirical status of Gottfredson and Hirschi’s self-control theory, Pratt and Cullen (forthcoming) coded each of the studies on their measurement of the dependent variable, model specification and research design characteristics (e.g., whether variables from competing criminological theories were controlled statistically, and, if so, which ones; whether the study’s design was cross-sectional or longitudinal), and sample characteristics (e.g., the racial, gender, and age composition of the sample; the

geographic location of the respondents) (c.f. the lack of such controls in other meta-analyses, e.g., Whitehead and Lab, 1989).

Extensive methodological controls such as these have two distinct advantages. First, more studies could be included in the sample, where methodological variations across studies could be statistically controlled as opposed to simply excluding such studies from the sample. This would allow for the application of more complex statistical models and provide more stable effect size estimates. Second, the relevance of meta-analysis for understanding which types of methodological approaches are more or less likely to yield a particular empirical outcome would be highlighted. This may open up a new line of research for meta-analysis as an explicit method of assessing the impact of methodological variations on research outcomes, which is an area that has yet to be seriously explored (a notable exception is Lipsey's discussion of the "variability of effects" in his 1992 meta-analysis).

Higher-Order Statistical Techniques

The unit of analysis in meta-analysis is the empirical study. Accordingly, similar to when the unit of analysis is the individual or some social aggregate, higher-order statistical techniques can be applied to meta-analytic samples. Increasing the level of analytical and methodological rigor in meta-analyses, therefore, will require that researchers make every attempt to employ the statistical techniques that are traditionally used in other types of research. This has already been done in a few meta-analyses in criminal justice and criminology, where recent studies have used statistical techniques ranging from analysis of variance (Pratt, 1998) and confidence intervals (Bonta et al.,

1998) to correlation analysis (Pratt and Maahs, 1999), multiple regression analysis (Lipsey, 1992), and hierarchical linear modeling techniques (Kalaian and Raudenbush, 1996; Raudenbush, Becker, and Kalaian, 1988). Aside from providing more reliable estimates of complex empirical relationships, continuing this trend of bringing the same level of statistical sophistication to meta-analyses that is typically found in most empirical studies should help to bolster the overall legitimacy of the technique of meta-analysis.

Decisions Should Be “Public”

Finally, after the decisions regarding the sample criteria, statistical controls for methodological variations, and statistical analysis techniques have been made, it is important that each of these decisions be made “public.” Indeed, they should be clearly stated in the text of the meta-analysis. Doing so would result in two potential benefits. First, the database of studies could remain dynamic—that as additional studies are published, they can be added to the sample of studies and relationships reassessed. Second, and just as important, the analysis could be replicated by those skeptical of the findings of the meta-analysis.

CONCLUSIONS

Researchers in criminal justice and criminology are faced with numerous theories and sets of propositions, often in conflict with one another, that attempt to explain the nature of certain empirical relationships. Despite the plurality of potential explanations

for any given research question, we may assume that only the most radical post-modernists would deny the claim that empirical data *should* play a role in our process of understanding how the world works. Determining precisely what the data say about the diversity of studies that address a common research hypothesis, however, is a difficult task.

Part of the problem for establishing the empirical status of the literature in a particular research area is rooted in the nature of how theories are constructed in criminal justice and criminology. Few theories are presented as a set of formal, logically interrelated propositions that can be either falsified or supported; rather, they are usually conveyed discursively over tens, if not hundreds of pages, and often over several works and at times over many years (Gibbs, 1989; see also Pratt and Cullen, forthcoming). Indeed, some go as far as to argue that most theoretical formulations of crime and criminals are similar in that:

their postulates (key assumptions) are not clearly identified; their central concepts are ill-defined; their major propositions (truth claims) are often murky; and there is often much confusion as to which parts of them are assumptions and which are propositions, open to empirical scrutiny (Leavitt, 1999:394).

As a result, little consensus may exist regarding whether a specified relationship does or does not exist, or whether it has even been adequately tested empirically.

The suggestion being made by advocates of the technique of meta-analysis is that another barrier to discerning the empirical status of bodies of research literature is how the discipline of criminal justice and criminology goes about organizing the knowledge

derived from the pool of empirical studies. As stated above, traditional narrative reviews of research literature tend to produce no definitive conclusions as to the *degree* to which certain key variables are related. One possible unintended consequence of the lack of more sophisticated literature reviews is that many researchers may only have a general, and perhaps inaccurate, understanding of which theoretical perspectives have earned the most empirical support (e.g., see the discussion by Burton and Cullen, 1992).

Using meta-analysis to quantitatively synthesize the extant empirical literature should not be viewed as a panacea for these problems. It may, however, be a potentially valuable technique for organizing our knowledge about prevailing issues in criminal justice and criminology. Through the use of meta-analysis, we may develop comparative information on how tests of particular hypotheses fair under similar methodological conditions (e.g., types of samples, types of research designs). Further, we may learn which theories or issues have been insufficiently tested and where additional research may be necessary. Finally, a knowledge base could be developed that could be updated as new independent empirical tests emerge, and that could be replicated through independent meta-analyses by those skeptical of the results.

Even so, it must be recognized that there is still considerable resistance among researchers in criminal justice and criminology to the application of meta-analysis. Much of the lingering criticism has to do with the perception that meta-analytic literature reviews are “soft” methodologically. One of the objectives of this chapter was to provide a set of guidelines for improving the level of analytical and methodological rigor in a meta-analysis. In essence, the ability of meta-analysis to effectively organize knowledge in criminal justice and criminology largely hinges on the ability of meta-analysts to

employ the same level of scientific rigor in their reviews that is found in the empirical studies they are reviewing. In so doing, charges against meta-analyses such as comparing “apples and oranges” may at least be plead down to that of comparing “apples and apple sauce.”

CHAPTER 3

MACRO-LEVEL THEORIES OF CRIME

Early perspectives on criminological theory and research were guided by the assumption that the explanation for crime can be found within the individual. These theories differed, however, in terms of their propositions regarding what it is about individuals that *should* cause crime. For example, some criminological theorists maintained that crime could be a function of biological determinism (Dugdale, 1877), of sub-standard intelligence or “feeble-mindedness” (Goddard, 1914), or of dissocial manifestations of psychic forces (Aichorn, 1925 [1979]). Others were even so bold as to assert that individuals’ criminal propensity can be indicated by the existence of “criminal bumps” on their heads (Lombroso-Ferrero, 1911; see also Gould, 1996). Regardless of their differences, each of these early perspectives shared the underlying premise that individual variations, not social conditions, were responsible for criminal behavior.

However, social and economic changes beginning in the early 1900s, which were largely fueled by mass industrialization, resulted in new social problems and new ways of thinking about the sources of criminal behavior. Rapid increases in urbanization, residential mobility, and the rise of racially heterogeneous neighborhoods in American cities seemed to occur in concert with increases in crime rates. In particular, crime became visibly concentrated among the urban poor. In the midst of the Progressive movement, a liberal reform movement that occurred in the early twentieth century America, criminologists began to reject the notion that the poor were somehow biologically inferior and that they therefore deserved their meager lot in life as the natural

outcome of their collective pathology (Cullen and Gilbert, 1982; Rothman, 1980). Instead, the Progressives “preferred a more optimistic interpretation: The poor were pushed by their environment, not born, into a life of crime” (Lilly, Cullen, and Ball, 1995:39). As a consequence, a number of new formulations of criminological thought began to emerge that sought to shift the assumed “causes” of crime “from the personal to the social plane” (Matza, 1969:47).

The first of these new traditions was the theory of social disorganization articulated by Chicago school of criminology researchers Shaw and McKay (1942). Following their research on juvenile delinquency in Chicago, they developed a neighborhood-level theory crime that placed little to no blame on the individuals residing in high-crime neighborhoods. Rather, “Shaw and McKay believed that juvenile delinquency could be understood only by considering the social context in which youths lived” (Lilly et al., 1995:44). In a similar vein, what followed were the other macro-level theoretical traditions of anomie/strain (Merton, 1938), the conflict-Marxian model of absolute deprivation (Turk, 1969), the relative deprivation-inequality paradigm (Blau and Blau, 1982), routine activities theory (Cohen and Felson, 1979), deterrence-rational choice theory (Becker, 1978), and the emerging social support-social altruism theory (Cullen, 1994; see also Chamlin and Cochran, 1997). Like the individual theories of crime that came before them, these perspectives differ in their explanations as to *how* certain types of social conditions are criminogenic. What is important is that they each harbor the notion that understanding the nature of the ecological contexts in which individuals live—not the individuals themselves—is necessary for the explanation of criminal behavior.

With this perspective in mind, this chapter discusses each of the major macro-level theoretical traditions mentioned above in terms of their key propositions and substantive content. In addition, the variables specified by each theory and how they are typically measured in empirical studies will be outlined. In essence, this chapter serves the purpose of organizing macro-level variables around particular theoretical traditions.⁸ This organization, in turn, will guide the portion of the meta-analysis that addresses the empirical status of the major macro-level theories of crime.

SOCIAL DISORGANIZATION THEORY

Key Propositions and Substantive Content

The social disorganization tradition grew out of the research conducted in the early 1900s in Chicago by Shaw and McKay (see Shaw and McKay, 1972). Upon studying Chicago's juvenile court records over a period of several decades, Shaw and McKay noted that rates of crime were not evenly dispersed across time and space in the city. Rather, crime tended to be concentrated in particular areas of the city—namely, slum neighborhoods. Further, crime rates were highest in these neighborhoods regardless of which racial or ethnic group happened to reside there at any particular time; and, as the previously crime-prone groups moved to other lower-crime areas of the city, their rate of criminal activity decreased accordingly. These observations led Shaw and McKay to the conclusion that crime was likely a function of neighborhood dynamics and not

⁸ Since the organization of macro-level variables is the central purpose of this chapter, it is not intended to serve as an exhaustive review of all ecological crime studies. Rather, studies will be highlighted and discussed based on their unique contributions to a particular macro-level theory's conceptual development,

necessarily a function of the individuals within such neighborhoods. The question that remained was: what are the characteristics of the “slum neighborhoods” that set them apart from low-crime neighborhoods, and therefore seemed to foster criminal activity?

In answering this question, Shaw and McKay focused on the urban areas experiencing rapid changes in their social and economic structure—or, in the “zone of transition.”⁹ In particular, they looked to neighborhoods that were low in socioeconomic status, with high rates of residential mobility, and had higher degrees of racial heterogeneity. These neighborhoods were viewed as “socially disorganized,” meaning that conventional institutions of social control (e.g., schools, churches, voluntary community organizations) were weak and unable to regulate the behavior of the neighborhood’s youth. Shaw and McKay (1972) also noted that, aside from the lack of behavioral regulation, socially disorganized neighborhoods tended to produce “criminal traditions” that could be passed across successive generations of youths. This system of pro-delinquency attitudes could be easily learned by youths through their daily contact with older juveniles (see also Kornhauser, 1978). Thus, a neighborhood characterized by social disorganization provides fertile soil for crime and delinquency in two ways: through a lack of behavioral control mechanisms, and through the cultural transmission of delinquent values.¹⁰

and/or to its influence on how subsequent researchers went about testing the theory. The systematic quantitative review of all macro-level studies of crime will be contained in Chapter five.

⁹ This focus was based on Burgess’s (1967) theory of urban development. The “zone in transition” was defined as the true “inner city,” where residents were often displaced to live in slum-like conditions because of their inability to afford to live elsewhere.

¹⁰ As another testament to the influence of the work of Shaw and McKay, these two “prongs” of social disorganization theory provided the basis for individual-level theories such as social control/social bond theory and Sutherland’s differential association theory (see Lilly et al., 1995).

It is important to note that Shaw and McKay did not specify a *direct* relationship between social disorganization and crime (see the discussion by Bursik, 1988). Rather, the socially disorganized neighborhoods observed by Shaw and McKay could indirectly influence neighborhood rates of crime in two ways. First, areas characterized by social disorganization tended to experience high levels of population turnover, which meant that they were abandoned by most as soon as it was economically possible (see also Wilson, 1987). In addition, such neighborhoods often experienced rapid changes in racial composition (i.e., racial heterogeneity) that made it difficult to resist the influx of new racial or ethnic groups. Taken together, these characteristics hindered the ability of socially disorganized neighborhoods to effectively engage in “self-regulation” (Bursik, 1986). In short, Bursik (1988:521) states that “the dynamics of social disorganization lead to variations across neighborhoods in the strength of the commitment of the residents to groups standards” and, as a result, variations in community crime rates.

The social disorganization perspective remained both popular and influential throughout the 1950s and 1960s. As Bursik and Grasmick (1992:ix) note, however, “with the refinement of survey approaches to data collection and the increased interest in social-psychological theories of control, deterrence, learning, and labeling, the focus of the discipline significantly began to shift from group dynamics to individual processes during the 1960s and 1970s.” This trend away from macro-level criminological theory and research saw the social disorganization tradition fall into relative disfavor among criminologists, many of whom viewed it as irrelevant or, at best, marginal to modern criminology (see e.g., Arnold and Brungardt, 1983; Davidson, 1981; cf. Byrne and Sampson, 1986).

Even so, social disorganization theory was “rediscovered” in the 1980s. Research by scholars such as Bursik (1986, 1988), Sampson and Groves (1989), and Wilson (1987, 1990, 1996) helped to revitalize, and partially reformulate and extend, the social disorganization tradition. In doing so, a number of problems leveled against the theory have been addressed effectively (see the discussion by Bursik, 1988). For example, research has been conducted to test for the “reciprocal effects”¹¹ of social disorganization (Bursik, 1986), and to test for the potential impact the levels of social disorganization of “surrounding communities” may have on neighboring communities (Heitgerd and Bursik, 1987).¹²

In addition, the scope of the theory was adjusted and expanded to include constructs beyond the macro-level components originally specified by Shaw and McKay (e.g., low socioeconomic status, residential mobility, racial heterogeneity). New concepts have been added that have enhanced its theoretical clarity. In particular, recent research has explicitly tested for “intervening mechanisms” between the traditional social disorganization variables and crime rates. The intervening mechanisms noted by researchers include the effect of social disorganization on rates of family disruption and “collective efficacy” (see Sampson and Groves, 1989; Sampson, Raudenbush, and Earls, 1997), which, in turn, directly influence crime rates. Now more fully specified and

¹¹ The “reciprocal effects” mentioned by Bursik (1986:64) have to do with the assumption that “within the context of ongoing urban dynamics, the level of delinquency in an area may also directly or indirectly cause changes in the composition of an area” due to “out-migration.” Thus Bursik estimated the simultaneous effects of social disorganization on crime, and crime on social disorganization. Similar approaches can be found in the work of Bursik and Webb (1982) and Morenoff and Sampson (1997), where changes in population characteristics were being predicted by changes in crime/delinquency rates.

¹² Heitgerd and Bursik (1987) showed that a relatively socially organized community may experience high rates of delinquency simply by virtue of being located geographically adjacent to a socially disorganized community.

extensively tested empirically, the variables specified by social disorganization theory and how they are typically measured in empirical studies may be assessed.

Variable Specification and Measurement

Empirical tests of social disorganization theory fall into two camps: those that fail to include intervening mechanisms in their statistical model, and those that do include such variables. In either case, most tests of the theory begin by specifying and measuring variables associated with the original social disorganization formulation of Shaw and McKay. This section of the chapter discusses the variables specified by traditional social disorganization theory and the recently articulated intervening mechanisms, and how they are typically measured in empirical studies. A discussion of the different types of dependent variables used by researchers is also provided.

Traditional Social Disorganization Variables. Virtually all published tests of social disorganization theory include measures neighborhood socioeconomic status, residential mobility, and racial heterogeneity. Accordingly, the “neighborhood” is typically the unit of analysis (or level of aggregation) across these studies. The *socioeconomic status* of a neighborhood, especially in those studies where individual-level data are “aggregated up” (see, e.g., Greenberg, 1986; Sampson and Groves, 1989; Sampson et al. 1997), is usually measured by either a factor or summed scale of economic variables (Sampson and Groves, 1989; Sampson et al., 1997). Other studies have used a combination of income inequality measures and unemployment rates¹³ to

¹³ The income inequality measure used by Sampson (1986) was the Gini Index of Income Concentration. Unemployment rates, on the other hand, are measured less consistently. Sampson (1986) used the percent unemployed aged sixteen or older; Bursik (1986) used the percent of males who were unemployed in

proxy the relative economic health of a neighborhood (e.g., see Bursik, 1986; Heitgerd and Bursik, 1987; Sampson, 1986). *Residential mobility* has typically been measured as the proportion of residents in a neighborhood living in the same dwelling for the last five years (Heitgerd and Bursik, 1987; Sampson, 1986; Sampson et al., 1997).¹⁴ Finally, *racial heterogeneity* has typically been measured as the percent black (Sampson, 1986) or percent non-white (Bursik, 1986)¹⁵ in a neighborhood.

In addition to the variables specified by Shaw and McKay, Sampson (1986) noted that much of the empirical work on social disorganization theory has failed to consider the impact of other social disorganization variables, such as *family structures and stability*. He suggested that traditional social disorganization variables may influence community crime rates when taking into account the effects of levels of family disruption. This may occur by (1) removing an important set of control structures over youths' behavior, and (2) creating greater opportunities for criminal victimization (i.e., through the lack of capable guardianship). Essentially, Sampson (1986) recognized the importance of how social disorganization theory relates to control theory and routine activities/lifestyle theory.

Sampson used three measures of family structure. First, he included a measure of the percent of residents in a neighborhood who were ever married that were either

conjunction with the percent of males who were employed in particular fields (e.g., the percent employed in "professional" occupations) and the median neighborhood education levels.

¹⁴ This measure differed from that used by Sampson and Groves (1989), who used data from the British Crime Survey. Their measure of residential mobility was the percent of residents in a neighborhood who grew up within a fifteen-minute walk from their current home.

¹⁵ Bursik (1986) also included a measure of the percent of foreign-born whites as a measure of racial heterogeneity. Bursik and Webb (1982) and Heitgerd and Bursik (1987) used residual change scores for the same variables in their study.

divorced or separated. The second measure of family structure was the percent of female-headed families. Finally, he included a measure of the percent of primary (or single)-headed households. His analyses revealed that independent of the traditional social disorganization variables, the family structure variables each had a direct significant effect on community crime rates. Thus, Sampson's work identified an important and additional source of social disorganization (implicit in the work of Shaw and McKay) that had been previously overlooked in empirical studies.

It is also important to note that Shay and McKay viewed social disorganization variables as having an "indirect" effect on community crime rates. As such, a body of literature has emerged that attempts to specify the intervening mechanisms between social disorganization variables and crime rates. While this pool of studies is still relatively small, their findings are substantively important and thus warrant special consideration.

Intervening mechanisms. The trend toward specifying the indirect effects of social disorganization variables on crime was begun by Sampson and Groves (1989). Their study used aggregated data from the British Crime Survey. The intervening mechanisms between social disorganization variables¹⁶ and crime rates specified in their study include informal control mechanisms such as youths' local friendship networks, the prevalence of unsupervised peer groups, and the level of organizational participation in the neighborhood.¹⁷

¹⁶ The measure of "family disruption" used by Sampson and Groves (1989), unlike in the study conducted by Sampson (1986), was treated as one of the traditional social disorganization variables, and not as an intervening mechanism.

¹⁷ Each of these variables were created by aggregating up individuals' responses to a number of scaled items.

The most recent direction the specification of intervening mechanisms for social disorganization theory can be seen in the work of Sampson et al. (1997). Their study argued that socially disorganized neighborhoods are likely to be low on *collective efficacy*, which is defined as “the willingness of local residents to intervene for the common good” (Sampson et al., 1997:919). The authors go on to state that community residents are “unlikely to intervene in a neighborhood context in which the rules are unclear and people mistrust or fear one another. It follows that socially cohesive neighborhoods will prove the most fertile contexts for the realization of social control.” (p. 919). Using aggregated data from the Project on Human Development in Chicago Neighborhoods, they found that the traditional social disorganization variables explained 70 percent of the variation in their collective efficacy measures which, in turn, effectively mediated much of the direct effects of the social disorganization variables.

Dependent variables. Social disorganization variables have been used to predict rates of multiple types of crimes. For example, the theory has been tested to predict rates of general juvenile delinquency (Bursik, 1986), rates of personal violent victimization (Sampson, 1986; Sampson and Groves, 1989; Sampson et al., 1997), and rates of property victimization (Sampson, 1986; Sampson and Groves, 1989). Social disorganization variables have also been found to be significantly related to communities’ levels of fear of crime (Greenberg, 1986). For a tabular summary of the empirical tests of social disorganization theory see Appendix 1.

Summary of Social Disorganization Theory

Since its first articulation by Shaw and McKay in the early 1900s social disorganization theory has fallen both in and out of favor with criminologists. Despite being somewhat marginalized during the 1960s and 1970s, recent research has revived the theory and it continues to make an important contribution to criminological thought. In its current iteration, researchers have specified the links through which traditional social disorganization theory variables (e.g., low socioeconomic status, residential mobility, racial heterogeneity) may influence community crime rates. In particular, levels of social disorganization may affect informal control and criminal opportunity mechanisms (e.g., unsupervised peer groups, collective efficacy), which, in turn, directly influence neighborhood crime rates. Thus, in its contemporary form, social disorganization theory continues to be a parsimonious, yet dynamic, explanation of crime at the macro-level that has received considerable empirical support.

THE ANOMIE/STRAIN TRADITION

Key Propositions and Substantive Content

Robert Merton published his “Social Structure and Anomie” in 1938. In this brief article, Merton set forth a theoretical framework for explaining crime rates that differed from that of the Chicago school criminologists. For example, theorists such as Shaw and McKay held that urban slum areas foster criminal behavior through the generational transmission of deviant cultural values. Thus, social disorganization theory assumes that the *rejection* of conventional middle class values results in high rates of crime in urban

slum communities. Merton, on the other hand, argued that it was the rigid *adherence* to conventional American values that caused high rates of crime and deviance. In essence, he believed that the widespread conformity to American culture in general—and the American obsession with economic success in particular—produced high levels of serious crime.¹⁸ Of course, understanding why Merton made such a claim requires an understanding of how he viewed American society.

Anomie in American Society. Merton noted that, as opposed to other Western industrialized nations, the United States places an unusual emphasis on economic success. Even more unique is how this emphasis seems to be universal—that all members of American society, from the well-to-do to the impoverished, ascribe to the “American dream,” and that if one were simply willing to work hard enough, he/she would inevitably reap the economic rewards of such labors. The problem, according to Merton, is that despite the widespread belief in the possibility of upward social mobility, the American social structure limits individuals’ access to the goal of economic success through legitimate means. For example, while the probability of attaining economic success would be enhanced by getting a college education or by taking advantage of some strategic nepotism, not all members of American society are able to do so. Those lower on the socioeconomic ladder are particularly vulnerable due to their relatively disadvantaged starting point in the race toward affluence.

In essence, Merton’s work contained separate (but related) discussions of how “culture” and “social structure” could cause high crime rates (Messner, 1988). Merton

¹⁸ This is not to say that Merton viewed the deviant rejection of middle class values as unimportant for explaining crime. Instead, he provided an explanation as to why crime, although concentrated in urban slum areas, is still widely dispersed elsewhere (e.g., suburban or rural areas).

noted that the American culture, as stated above, places economic success at the pinnacle of social desirability. The emphasis on attaining economic success, however, is not matched by a concurrent normative emphasis on what “means” are legitimate for reaching the desired “goal.” This problem is then exacerbated by the social structural component discussed by Merton, which highlights the structural barriers that limit individuals’ access to the legitimate means for attaining the goal of economic success. The disjunction between culturally ascribed goals (i.e., economic success) and the availability of legitimate means to attain such goals (i.e., social structural limits) in turn puts pressure on the cultural norms that guide what means should be used to achieve the culturally prescribed goal.¹⁹

Merton referred to this weakening of cultural norms as “anomie.” His adoption of the term “anomie” is based on Durkheim’s (1897 [1951]) reference to the weakening of the normative order in society; or, put differently, how institutionalized social norms may lose their ability to regulate individuals’ behavior. In particular, Merton noted that institutionalized norms will weaken, and anomie will set in, in societies that place an intense value on economic success. When this occurs, the pursuit of success is no longer guided by normative standards of right and wrong. Rather, Merton (1968:189) noted, “the sole significant question becomes: Which of the available procedures is most efficient in netting the culturally approved value?”

Thus, the exceptionally high crime rates experienced by the United States are seen through the lens of anomie theory as being a natural consequence of American culture

¹⁹ Although less obvious in Merton’s discussion, he also raises the possibility that the intense emphasis on the goal of economic success, in and of itself, could cause anomie. Thus, his theory really specifies two potential pathways by which levels of anomie in society may result in high crime rates (Cullen, 1984).

(see also the discussion by Currie, 1997). By dangling the carrot of a universally shared goal in the faces of many who cannot reach that goal through legitimate means, the anomic quality of American society “produces intense pressure for deviation” (Merton, 1968:199). Although Merton specified a number of ways in which *individuals* may adapt to the “strains”²⁰ brought on by the inability to secure pecuniary success (many of which are non-deviant), his perspective is ultimately a macro-level explanation for why the United States leads all other Western nations in rates of crime.

Messner and Rosenfeld’s Institutional Anomie Theory. Anomie theory remained influential throughout the 1950s and 1960s. Guided by Merton’s emphasis on social stratification, the theory was even extended to notions of “status discontent” and delinquency (see, e.g., Cohen 1955; Cloward and Ohlin, 1960). Despite its popularity, however, it was the subject of considerable criticism in the 1970s and 1980s. At this time, theorists began to take issue with some of the core theoretical assumptions of the theory; namely, that the discrepancy between individuals’ life aspirations and achievements causes delinquency (Kornhauser, 1978; cf. Burton and Cullen, 1992). Even more important was the changing social context of the 1970s and 1980s. Rosenfeld (1989) notes that during this time the liberal consensus that characterized the postwar era began to weaken, the welfare and antipoverty programs of the “Great Society” came under attack, and crime rates continued to rise. As such, “the necessary supports for a theory universally regarded as advocating liberal social reform as a way to reduce crime withered away” (Messner and Rosenfeld, 1997:14).

²⁰ Although the terms “strain” and “anomie” have been used to describe Merton’s theory, the two are not necessarily interchangeable. Their separation is attributable to their application to different levels of analysis: “strain theory” generally refers to the individual, or micro, level of analysis, whereas “anomie theory” is the macro-level variant.

Criminologists' interests in the anomie/strain tradition were again piqued, however, when Messner and Rosenfeld published their *Crime and the American Dream* in 1994. In this important work, Merton's anomie/strain theory was extended and partially reformulated. Although Messner and Rosenfeld agreed with Merton's view of American culture, they found his analysis of social structure incomplete (see also Messner, 1988). In particular, Merton held that the American system of *stratification* was responsible for restricting individuals' access to legitimate opportunities for upward socioeconomic mobility, which, in turn, resulted in high levels of criminogenic anomie in society. What was missing from the anomie tradition, argued Messner and Rosenfeld (1997a:xi), was an understanding of how "The American Dream promotes and sustains an institutional structure in which one institution—the economy—assumes dominance over all others." This apparent "imbalance" in the institutional structure limits the ability of other social institutions, such as the family, education, and/or the political system, to insulate members of society from the criminogenic pressures of the American Dream or to impose controls over their behavior. Thus, what Messner and Rosenfeld have created is a version of anomie/strain theory that sees crime rates as a function of the American Dream's cultural emphasis on economic success in *combination* with an institutional structure dominated by the economy.

Variable Specification and Measurement

Most of the empirical work assessing the anomie/strain tradition has been conducted at the individual level (see the review by Burton and Cullen, 1992) with a social-psychological flavor (Agnew, 1985, 1992). This is due, in no small part, to the

ease with which individual-level self-report data can be gathered on “strain” variables. On the other hand, gaining access to data reflecting the anomic tendencies of American “culture” or “institutional structure” at the aggregate level has proven to be a daunting task for researchers (Messner, 1988). In particular, locating “direct” operational measures of macro-level variables such as the “commitment to the American Dream,” the “dominance of the economic structure,” and the “strength of non-economic institutional controls” has been treated as the cost-benefit equivalent of *The Money Pit* (e.g., see the discussion by Sampson and Groves, 1989). Nevertheless, using “indirect” measures of the central concepts there have been three²¹ explicit tests of the theory at the macro-level—both of which are based on Messner and Rosenfeld’s institutional anomie theory. A tabular summary of these tests can be found in Appendix 2.

Chamlin and Cochran (1995) conducted the first “partial” test of Messner and Rosenfeld’s institutional anomie theory. Rather than subjecting the theory to a comprehensive empirical test, they derived specific propositions to examine. They noted that “Messner and Rosenfeld’s institutional anomie theory holds that culturally produced pressures to secure monetary rewards, coupled with weak controls from noneconomic social institutions, promote high rates of instrumental criminal activity” (Chamlin and Cochran, 1995:413). Thus, their analysis focused on the “multiplicative effects of economic conditions and structural indicators of noneconomic institutions on instrumental crime” (p. 415).

²¹ This is not to say that other macro-level studies have not been conducted that may bear, to a degree, some importance to anomie theory. Indeed, many studies have included measures of economic deprivation—in some form—to predict crime rates (e.g., for a comprehensive review of the poverty and inequality literature see Hsieh and Pugh, 1993; and see Chiricos, 1987 for a review of the unemployment-crime literature). However, these studies tend to be couched in terms of either absolute deprivation/conflict theory or the inequality/relative deprivation model. As such, these studies are discussed more fully in those sections of this chapter.

As their measure of “instrumental crime” Chamlin and Cochran used rates of robbery, burglary, and larceny at the state level as their dependent variable. Levels of “economic deprivation” were measured as the percent of families below the poverty line. To proxy the strength of non-economic institutions, three variables were specified in their analysis: family structure (measured as the ratio of divorce rates to marriage rates), religious participation (measured as the rate of church membership), and political involvement (measured as the percent of eligible voters voting in the last congressional election). While also controlling for racial heterogeneity (percent black) and age structure (percent of the population aged 18 to 24), Chamlin and Cochran constructed interaction terms between the economic deprivation variable and each of the indicators of the strength of non-economic institutions. Their results revealed general support for institutional anomie theory—specifically, that the effect of economic deprivation on crime rates is contingent on the relative strength of non-economic social institutions (i.e., the interaction terms were significant predictors of property crime rates).

The second test of institutional anomie theory was conducted by Messner and Rosenfeld themselves in 1997. In a cross-national comparison of homicide rates, they predicted that rates of homicide would vary inversely with the “decommodification of labor” (Messner and Rosenfeld, 1997b:1393). “Decommodification,” in this sense, refers to “the empowerment of the citizenry against the forces of the market”; or, in other words, “the granting of services and resources to citizens as a matter of right, thereby reducing their reliance on the market for sustenance and support” (pp. 1394-1395). In essence, Messner and Rosenfeld contend that societies that make overt efforts to insulate

their members from the whims of the market (e.g., through social welfare policies and programs) will experience lower rates of violent crime.

To test this proposition Messner and Rosenfeld examined the homicide rates for a sample of 45 nations. A “decommodification index” was created for each country, which covered access to welfare policies, their income-replacement value, and their expansiveness of coverage (i.e., a comprehensive index of social security programs). To isolate the effect of the decommodification index on homicide rates, they controlled for each nation’s sex ratio (males per 100 females), income inequality (Gini index), socioeconomic development (an index of sociodemographic factors), and economic discrimination. They found that, net of the statistical controls, the decommodification index was inversely (and significantly) related to homicide rates. Thus, empirical support for institutional anomie theory was once again revealed.

The most recent test of institutional anomie theory was conducted by Piquero and Leeper Piquero (1998). Building mostly on the test by Chamlin and Cochran (1995), Piquero and Leeper Piquero’s (1998) analysis also used states as the unit of analysis. Using the percentage of the state’s population below the poverty level as a proxy for the strength of the economy, the authors constructed interaction terms between the “economy” variable and multiple measures of the health of the non-economic institutions of the family, the polity, and education. Their study yielded mixed support for the proposition that poor economic conditions would influence crime rates most when coupled with weak non-economic institutions. Most salient, the statistical significance of the interaction terms depended to a degree on model specification (e.g., predicting property versus violent crime rates) and differences in how key variables were measured

(e.g., measuring “education” as the percentage of high-school dropouts versus the mean teachers’ salaries across states).

Summary of Anomie/Strain Theory

Merton’s anomie/strain theory has enjoyed considerable influence since the early 1900s. Despite falling somewhat out of favor beginning in the 1960s, the theory’s rather bold contention that American society itself is criminogenic has remained provocative to this day “for it suggests, in a sense, that society gets the crime it deserves” (Lilly et al., 1995:73). Now in its current iteration of institutional anomie, the theory has the potential to become even more influential with its emerging body of empirical support. Nevertheless, empirical tests of anomie theory (either in its original or extended form) are still few in number. On one hand, this may diminish our confidence in the overall explanatory power of the theory, especially at this stage in the game of theoretical validation. On the other hand, however, this also hints toward a fruitful research agenda for interested and motivated criminologists to pursue.

ABSOLUTE DEPRIVATION/CONFLICT THEORY

Key Propositions and Substantive Content

Both social disorganization and anomie/strain theories point to the structural characteristics that may allow criminal activity to flourish. While these theories differ in their propositions regarding how certain social conditions may produce high crime rates, they are in tacit agreement that “crime,” as a social phenomenon, is an objective social

reality. In other words, social disorganization and anomie/strain theories implicitly assume that a rough consensus exists within societies about what types of behaviors should, and should not, be considered “criminal.”

Thus, neither perspective devotes much attention to how certain behaviors come to be labeled by society as “criminal.” Indeed, although both of the theories previously discussed share the contention that groups of individuals are mired in a system of social stratification, they overlook the degree to which power differentials between the relatively powerful and powerless groups in society may shape not only individuals’ behaviors, but also the social perceptions of those behaviors. Conflict theory attempts to fill this theoretical void.

At its most basic level, conflict theory draws on the Marxian tradition and is most concerned with “[focusing] attention on struggles between individuals and/or groups in terms of power differentials” (Lilly et al., 1995:132-33). In essence, conflict theory sees crime as a socially constructed label that powerful groups are able to place on the behaviors of groups or individuals who hold, in comparison, less social power and/or political authority (Chambliss and Seidman, 1971; Quinney, 1970; Turk, 1969). Crime, therefore, is viewed through the lens of conflict theory as being morally relativistic. Since, although both members of the upper and lower social classes may engage in morally questionable behaviors, the legal system—which is assumed by conflict theorists to be a tool wielded only by the upper classes—tends to punish only the deviant activities of the lower classes (Bonger, 1916 [1969]).

Variations and Critiques of Conflict Theory. It is important to note that conflict theory places its iron in multiple fires. Some versions of conflict theory are concerned

only with the dynamics of conflict between social groups (e.g., see the discussion by Turner, 1978). For example, early works by Simmel, and by Vold (1958) focused on how social groups in conflict tended to maintain an equilibrium that eventually led to social stability and order. Other versions of conflict theory attempt to specify methods for which conflict may be eliminated—such as through trading in the evils of capitalism for the bliss of communism (Marx and Engels, 1848 [1992]).

The plurality of approaches falling under the heading of conflict theory has, to a certain extent, contributed to its somewhat shaky empirical foundation and to its theoretical murkiness. In particular, many conflict theorists have focused explicitly on “political” crimes (e.g., protest movements, conflicts between labor and management) as indicators of how powerful groups may shape and define substantive law (Vold, 1958). Other conflict theorists attempt to marshal in their defense laws against “victimless” crimes, such as “vagrancy,” as evidence that the “normal” everyday activities of politically disenfranchised groups of individuals are routinely demonized by the rule-making upper classes (Chambliss, 1964, 1969). Granted, each of these approaches may be useful for illustrating how “law” may be used as a method of social control. The central problem facing conflict theorists, however, is one of attempting to convince the criminological community that certain violent offenses, such as rape or murder, are socially constructed labels placed on the preferred behaviors of the lower classes (Akers, 1997).²²

²² However, certain conflict theorists (see, e.g., Reiman, 1995) argue that even with regard to violent crimes, members of the upper classes are typically viewed as less responsible for their actions, and tend to receive relatively favorable treatment from the criminal justice system. For example, negligent business practices that result in multiple employee deaths (e.g., mining accidents) are often couched in the language of “liability” instead of “homicide.” Reiman (1995) goes on to note how this may be treated as *de facto* evidence that the wealthy are able to “get away with murder.”

The debates surrounding these elements of conflict theory are unlikely to be resolved any time soon. There are, however, other versions of conflict theory that are relatively unconcerned with the more post-modern endeavor of delineating the “definitions” of criminal and non-criminal behavior. Indeed, certain versions of conflict theory attempt to provide an explanation, setting aside “value judgments” regarding the “definition” of crime, as to the causal mechanisms by which social conflict leads to high rates of crime (Bonger, 1916 [1969]; Turner, 1978). It is this final variant of conflict theory that is the focus of the remainder of this section since its primary concern is the *explanation* (and therefore prediction) of variations in crime rates.

Conflict Theory and Crime Causation. In its simplest terms, conflict theory views the “causes” of crime in the following manner (see the discussion by Turner, 1978). First, a capitalist economic structure is likely to produce a condition of widespread poverty, where large groups of people will experience significant “resource deprivations” in terms of money and property. This absolute deprivation, in turn, heightens and reinforces the animosity harbored by the groups living in conditions of poverty toward those in the leisure class (see, e.g., Veblen, 1889 [1934]). Finally, the condition of poverty may be criminogenic, by itself, in two ways.

First, poverty may *directly* cause crime among the “subordinate classes” as members of such groups seek daily survival. In other words, certain criminal offenses (e.g., theft) may be necessary for some individuals to simply “get by” (see, e.g., the discussion by Bonger, 1916 [1969]; see also Lilly et al., 1995). Second, poverty may lead to crime *indirectly* as members of the poverty-stricken groups may eventually come to question the validity of the social arrangement they have been handed. Feeling as

though they have been given a “raw deal,” these groups “would then be more likely to organize and to bring the conflict out in the open, after which there would be polarization and violence” (Lilly et al., 1995:134). Thus, the main theoretical proposition concerning the macro-level causes of crime from the conflict perspective is that impoverishment itself is criminogenic, and that there should be a direct link between variables which proxy economic conditions, such as poverty rates, and crime rates.²³

Variable Specification and Measurement

Although purely economic variables tend to be the preferred vehicles of conflict theory and research, other relationships have been specified by the theory. For example, consistent with the diversity in theoretical directions taken by conflict theorists, much of the macro-level empirical tests of conflict theory do not treat “crime rates” as the dependent variable. Rather, a number of studies grounded in the conflict theory literature have examined the “threat hypothesis” of crime control (Jacobs, 1979; Liska, Lawrence, and Benson, 1981). In this body of work, researchers have drawn on the proposition from conflict theory that members of racial and economic minority groups are viewed as a threat by the dominant groups in a social system. A corollary assumption is that the

²³ Many macro-level theories of crime specify, to some extent, a relationship between economic conditions and crime. Both social disorganization and anomie/strain theories held that there should be *some* relationship between variables indicating certain economic properties and crime rates. Conflict theory is no exception. Although three major sets of variables are often used as a proxy for economic conditions in empirical studies—poverty, inequality, and unemployment—they are often treated as conceptually distinct. Indeed, separate theoretical and empirical bodies of work have been developed around the relationship between absolute deprivation (e.g., poverty), relative deprivation (e.g., inequality), and crime rates (in addition, unemployment seems to be a “pirate variable” claimed by multiple theories). As such, only the literature dealing with “absolute deprivation” is discussed, in the present case, under the heading of conflict theory. Despite the fact that multiple researchers have the effects of both absolute and relative deprivation on crime simultaneously (cf. Bailey, 1984; Messner, 1982; Williams, 1984), the theoretical and empirical literature addressing the relative deprivation/inequality paradigm will be given its own discussion in this chapter.

“powerful groups and strata are able to translate their perceptions of threat into public policy and thereby affect the size and administration of crime control apparatuses (Chamlin, 1989:355). Typically, these studies examine the effect of the relative size of a social aggregate’s racial minority population on its capacity to provide crime control—often through police expenditures (see, e.g., Greenberg, Kessler, and Loftin, 1985; Jackson, 1986; Jackson and Carroll, 1981).

Chamlin (1989) took this perspective a step further and examined the relationship between “threat hypothesis” variables and a particular type of crime—rates of police killings. Using state-level data, four measures were constructed to measure the presence of threatening groups: the percentage of families below the poverty level, income inequality (measured as the Gini index of income concentration), the percentage of blacks, and the percentage of individuals with Spanish surnames. After controlling for arrest rates (for index offenses), total index crime rates, police size, and the divorce rate, the threat hypothesis variables were consistently related to rates of police killings. Further, it was not uncommon for the threat hypothesis variables to be the strongest predictors of police killings in the regression models, with standardized effects (beta weights) ranging from .31 to .56.²⁴

Again, aside from this work on the racial threat hypothesis, most of the empirical studies related to conflict theory have examined the effects of absolute economic deprivation variables, such as “poverty rates,” on crime rates. The measures of “poverty rates” across studies is fairly consistent: the percent below the poverty threshold as

²⁴ A number of separate regression models were estimated by Chamlin (1989) to avoid the potential problems associated with collinearity between predictor variables (between the threat hypothesis variables). Thus, separate multiple regression equations were estimated for each of the threat hypothesis variables individually.

defined by the Social Security Administration—which has been adopted by the U.S. Census Bureau—is typically used.²⁵ Other methodological characteristics, however, exhibit considerable variation across studies. For example, studies of poverty and crime have been conducted at the neighborhood level (Messner and Tardiff, 1986; Mladenka and Hill, 1976), at the city level (Crutchfield, 1989; Jackson, 1984; Messner, 1983; Watts and Watts, 1981; Williams and Flewelling, 1988), on standard metropolitan statistical areas—or, SMSAs—(Balkwell, 1990; Blau and Blau, 1982, Jacobs, 1981; Lieberman and Smith, 1986; Peterson and Bailey, 1988; Smith and Bennett, 1985), on states (Baron and Straus, 1988; Parker and Smith, 1979), and on countries (Arthur, 1991).

Studies also differ in terms of the types of offenses comprising the dependent variable. Conflict theory-oriented studies of poverty and crime have attempted to predict general rates of violent crime (Arthur, 1991; Baron and Straus, 1988; Crutchfield, 1989; Jackson, 1984; Lieberman and Smith, 1986; Mladenka and Hill, 1976; Watts and Watts, 1981), homicide rates alone (Balkwell, 1990; Messner, 1983; Messner and Tardiff, 1986; Parker and Smith, 1979; Williams and Flewelling, 1988), or multiple rates of different types of violent and/or property crime rates (e.g., rape, robbery, assault; see, e.g., Blau and Blau, 1982; Crutchfield, 1989; Jackson, 1984). A tabular summary of these studies is provided in Appendix 3.

²⁵ Other approaches have, however, been taken. For example, Loftin and Hill (1974) used a “structural poverty index” (SPI) that is based on aggregate indicators such as the percentage of children living with one parent, and the percentage of the population failing the Armed Forces Mental Test (see also the work of Huff-Corzine, Corzine, and Moore, 1986; Smith and Parker, 1980; and the review by Hsieh and Pugh, 1993). Also, Messner and South (1986) used an annual household income of \$4,000 as a cutoff point for “absolute poverty.” This estimate closely resembled the official poverty level for a family of four (\$3,698) during the time of their data collection.

Not surprisingly, the heterogeneity in methodological approaches taken in studies of poverty and crime has produced few definitive conclusions about the relationship thus far. Indeed, existing reviews of this literature (both narrative and meta-analytic) have yet to reach any firm conclusions as to (1) what the “true” effect of poverty on crime may be, especially in the context of other variables meant to proxy economic conditions, or (2) the degree to which the relationship between poverty rates and crime rates is conditioned by the methodological variations outlined above (e.g., level of aggregation, type of crime rates examined). Although some reviews hint toward certain “trends” (e.g., the effect may be stronger at lower levels of aggregation), little effort has been devoted toward systematically assessing whether the differences in the “strength” of the relationship between poverty and crime are statistically significant across such differences in research designs. In other words, we do not yet know whether the fluctuations in “effect sizes” for this relationship are due to random error across studies, or if they are a systematic product of the way researchers have gone about testing this hypothesis.

Summary of Absolute Deprivation/Conflict Theory

On a more optimistic note, however, especially compared to the paltry body of empirical work on anomie/strain theory, the absolute deprivation/conflict theory model has been extensively tested empirically. This makes the questions of whether poverty rates are independently related to crime rates, and under what methodological conditions, well-suited to a meta-analysis (the technique being employed in the present investigation). Indeed, the current lack of consensus regarding the nature of the poverty-crime relationship is due, in part, to the ubiquity of studies using various methodological

techniques. Fortunately, meta-analysis is a particularly useful method of determining precise estimates of the aggregated effect size of the poverty-crime link, and how certain approaches to the study of poverty and crime may be biased toward finding a stronger or weaker relationship between the two variables.

RELATIVE DEPRIVATION/INEQUALITY THEORY

Key Propositions and Substantive Content

In their landmark study of metropolitan structure and violent crime, Blau and Blau (1982) set forth an explanation as to the “cause” of violent crime that differed from the absolute deprivation/conflict theory model. In short, Blau and Blau (1982:116) noted that the “poverty” model of crime argues that urban slums tend to create a particular subculture where youths value “toughness, smartness, excitement, and fatalism” which, in turn, “bring young persons into contact with the law.” Thus, the macro-level perspective on absolute deprivation/conflict theory “interprets delinquency not in terms of individual poverty but in terms of the shared cultural values that tend to develop in the impoverished conditions of urban slums” (Blau and Blau, 1982:116).

Blau and Blau (1982), however, viewed this position as problematic. Specifically, based on Blau’s (1977) previously articulated general macrosocial theory, Blau and Blau (1982) held that a number of Marxian perspectives and theories of opportunity are at least implicitly concerned with the effects of economic inequality, or “relative deprivation,” on crime rates. For example, early works by Bonger (1916 [1969]) and the more contemporary writings of Quinney (1974) both focus on the

exploitation of the poor by the rich in terms of property relations and living conditions and on the inevitability of crime as a result of such inequalities. Even more explicit is the statement by McDonald (1976:22) that “Inequalities in power, economic or political, were ultimately responsible for the nature of the criminal law established, its enforcement, and the pattern of criminal behavior appearing.”

As a first step toward differentiating the effects of inequality on crime rates relative to the effects of poverty, Blau and Blau (1982) analyzed the violent crime rates from a sample of 125 of the largest American SMSAs. Using measures of socioeconomic inequality between racial groups and economic inequality in general, the results suggested considerable support for the inequality/relative deprivation hypothesis. In particular, after controlling for the effects of inequality, poverty—the key variable for absolute deprivation/conflict theory—was not significantly related to total violent crime rates or to disaggregated rates of murder, rape, or assault. While poverty was significantly related to rates of robbery, the magnitude of its effect was substantially weaker than the effects of inequality (the beta weight values for inequality and poverty were .49 and -.30, respectively). Thus, the main theoretical proposition being made by Blau and Blau (1982:126) is that “aggressive acts of violence seem to result not so much from lack of advantages as from being taken advantage of, not from absolute but from relative deprivation.”

Variable Specification and Measurement

Similar to the body of empirical work on the relationship between poverty and crime, studies of the relationship between absolute deprivation/inequality variables and

crime rates differ considerably in terms of the methodological approaches taken since the study conducted by Blau and Blau (1982). In particular, researchers have employed a number of different measures of the central concept of “inequality.” While “general inequality” is typically measured by the Gini coefficient for family income,²⁶ studies vary as to whether racially homogeneous or heterogeneous measures of inequality are investigated.²⁷ This may pose a potential problem, some argue, since racial inequality, as opposed to inequality in general, may be the chief predictor of rates of urban violence (see, e.g., the discussions by Blau and Blau, 1982; Sampson and Wilson, 1995). Accordingly, researchers have also argued that racially disaggregated crime rates should be used in studies of inequality and crime, since the social and economic histories of different racial groups (e.g., blacks and whites) may have conditioned the relationship between inequality and crime (Messner, 1983; Messner and Golden, 1992).

Studies of the inequality-crime relationship have also focused on different levels of aggregation. Analyses have been conducted at the neighborhood level (Messner and Tardiff, 1986; Patterson, 1991), at the city level (Bailey, 1984; Carroll and Jackson, 1983; Hartnagel and Lee, 1990; Loftin and Parker, 1985; Messner, 1983), using SMSAs as the unit of analysis (Balkwell, 1990; Jacobs, 1981; Kennedy, Silverman, and Forde, 1991; Peterson and Bailey, 1988), at the state level (Huff-Corzine, Corzine, and Moore, 1986;

²⁶ This measure is generally computed for heads of households that are families, and is based on the combined income of all family members (see Blau and Blau, 1982). The Gini index includes wages or salary, income from self-employment, social security, public assistance funds, retirement benefits/pensions, and all other types of income (e.g., stock dividends, interest).

²⁷ Racially heterogeneous measures of income inequality are often difficult to use in the context of multiple regression analysis. When using measures such as the “eta” (the square root of the correlation ratio) of the relationship between the measure of racial composition and income, the problem of multicollinearity typically arises, especially as the level of aggregation gets larger. A common alternative to the correlation ratio approach is that taken by Blau and Blau (1982) is the simple measure of the difference in the mean socioeconomic status between racial categories.

Loftin and Hill, 1974; Smith and Parker, 1980), and at the national level (Avison and Loring, 1986; Groves, McCleary, and Newman, 1985; Hansmann and Quigley, 1982; Kick and LaFree, 1985; Krahn, Hartnagel, and Gartrell, 1986; Krohn, 1976; Messner, 1980, 1982, 1989).

Studies of inequality and crime also vary in terms of what types of crime rates are being predicted. For examples, some studies of the inequality-crime link focus on rates of violent crime in general (Baron and Straus, 1988; Blau and Blau, 1982; Carroll and Jackson, 1983; Hartnagel and Lee, 1990; Patterson, 1991), homicide rates (Avison and Loring, 1990; Bailey, 1984; Balkwell, 1990; Baron and Straus, 1988; Blau and Blau, 1982; Groves et al., 1985; Hansmann and Quigley, 1982; Huff-Corzine et al., 1986; Kennedy et al., 1991; Kick and LaFree, 1985; Krahn et al., 1986; Krohn, 1976; Loftin and Hill, 1974; Loftin and Parker, 1985; Messner, 1980, 1982, 1983, 1989; Smith and Parker, 1980), or rates of individual types of violent and/or property crime rates (Blau and Blau, 1982; Carroll and Jackson, 1983; Jacobs, 1981; Peterson and Bailey, 1988). A tabular summary of these studies is provided in Appendix 4.

Summary of Relative Deprivation/Inequality Theory

The heterogeneity in methodological approaches taken in studies of inequality and crime, not unlike that found across studies of poverty and crime, has contributed little to a firm understanding of the relationship between the two variables. On the surface, it appears that the ability of measures of relative deprivation/inequality to predict crime rates is a bit more consistent than that of its “absolute deprivation” cousin. Nevertheless, despite the impressive roster of studies which have tested the inequality-crime

relationship, we are left with only a few “hints” as to the methodological conditions under which the effect size of inequality variables on crime rates may be relatively strong or weak. For example, unanswered questions still remain concerning whether the effect of inequality on crime is greater for racially homogeneous or heterogeneous measures of inequality or crime rates, at higher or lower levels of aggregation, or when predicting different types of crime rates. Again, however, the answers to such questions may be found by a systematic, quantitative review of these empirical studies.

ROUTINE ACTIVITIES THEORY

Each of the macro-level perspectives on crime causation discussed thus far, while differing in their central theoretical propositions, have shared a common purpose: each attempts, in some way, to outline a theory of why collectives of individuals may be “motivated” to commit crimes. In the late 1970s, however, Lawrence Cohen and Marcus Felson began to examine the prediction of crime rates differently—by assuming the existence of motivated offenders and attempting to predict why and when individuals will be more or less likely to become their victims of crime. What followed was a theory of how the relationships between the basic elements of time, location, and people may either increase or decrease the likelihood that individuals will be the victims’ of predatory (personal or property) crime. While this perspective also sparked the development of theory and research in the areas of “environmental criminology” (see, e.g., Bottoms, 1994; Brantingham and Brantingham, 1991) and the analysis of “hot spots” of crime

(Sherman, Gartin, and Buerger, 1989), the following discussion focuses more closely on the original routine activities framework set forth by Cohen and Felson.

Key Propositions and Substantive Content

Cohen and Felson's (1979) work grew out of the human ecology approach within sociology, which emphasizes "the interdependence among people . . . and the physical environment, especially as people seek to gain sustenance from their environment" (see Felson and Cohen, 1980:390). Their research had an explicit focus on how variation in social structures may generate the circumstances for rates of criminal victimization. In particular, Cohen and Felson (1979) identified three broad categories of variables that they believed were most relevant to the prediction of rates of criminal victimization: (1) the presence of *motivated offenders*, (2) the existence of *suitable targets* for victimization (either personal or property), and (3) the absence of *capable guardianship* of persons or property.

Their central theoretical proposition, therefore, is that the rate of criminal victimization should increase when there is a "convergence in space and time of the three minimal elements of direct-contact predatory violations" (Cohen and Felson, 1979:589). Cohen and Felson go on to contend that it is the "routine activities," or daily schedules of people, that place them in social contexts where the likelihood of predatory victimization is enhanced. More specifically, Cohen and Felson (1979:593) define "routine activities" as the "recurrent and prevalent activities which provide for basic population and individual needs, [such as] formalized work, as well as the provision of standard food, shelter, sexual outlet, leisure, social interaction, learning, and child-bearing."

Accordingly, Cohen and Felson (1979; see also Felson and Cohen, 1980; Felson, 1993, 1994) advocate the use of macro-level variables²⁸ linking crime rates to the dispersion of activities away from family and household settings (e.g., employment, leisure activities such as frequenting bars) to predict rates of criminal victimization.

By taking a different theoretical route—that of examining the nature of aggregate rates of criminal *victimization* as opposed to *offending*—Cohen and Felson’s routine activities theory has little to say about how to reduce the pool of motivated offenders in a given ecological region.²⁹ Rather, the theory simply assumes that such offenders exist, and that they are, by definition, predisposed to engage in a particular pattern of offending in the absence of some external reason not to.³⁰ In other words, presented with opportunities (suitable targets) divorced from capable guardians (either formal or informal), “crime happens.”

Variable Specification and Measurement

As stated above, a key feature of the routine activities model is the notion of criminal opportunity. Such opportunities are presumably generated by “attenuated

²⁸ A similar perspective exists at the individual level (see, e.g., Cohen, Kluegel, and Land, 1981), which is often dubbed either “lifestyle” or “opportunity” theory. Multi-level models have also been specified linking the micro- and macro-level dimensions of routine activities and lifestyle theories (see Sampson and Wooldredge, 1987). While the more micro-level variant of routine activities theory has received considerable theoretical and empirical attention, the present discussion is concerned only with the macro-level formulation based on the original work of Cohen and Felson.

²⁹ Cohen and Felson (1979:605) do contend, however, that their theory “might in the future be applied to the analysis of offenders and their inclinations as well.”

³⁰ Despite being rooted mostly in notions of the ecological distributions of criminal opportunities, victims and offenders, the concepts of choosing easy targets, and avoiding risky opportunities where guardians may be plentiful often causes routine activities theory to be lumped in with rational choice/deterrence theories. Nevertheless, due to its roots in human ecology and its explicit recognition of structural characteristics that may influence rates of criminal victimization beyond those connected to the criminal justice system

guardianship”—or, the absence of informal social control (Bryant and Miller, 1997:83; see also Cohen and Felson, 1979; Cohen and Land, 1987; Felson, 1987). In a number of tests of routine activities theory (e.g., Cohen, Felson, and Land, 1980; Cohen and Land, 1987; Jackson, 1984), criminal opportunity has been measured by the *household activity ratio*. This measure is constructed as the sum of the number of married female workers and the number of non-husband and wife (or “primary individual”) households divided by the total number of households.³¹

The two variables comprising the numerator of the household activity ratio—the number of married female workers and the number of “primary individuals”—are assumed to proxy the presence of criminal opportunities for two reasons. First, higher scores on the ratio should indicate higher rates of activities outside of the home, and therefore less guardianship of the home, which may heighten the risk of property victimization (e.g., burglary, theft). Second, greater values of the household activity ratio may indicate higher rates of interpersonal contact between individuals (especially for women), which may increase the probability of predatory victimization (either between strangers or acquaintances).

Aside from the more proximate influence that the components of the household activity ratio are assumed to have on the “guardianship” of persons and property, Felson and Cohen (1980) also note that their measure may be a forerunner of attenuated informal social control for the following reasons. First, the number of females in the workforce may lead to high concentrations of unsupervised children which, in turn, create problems

apparatus, it is treated as a distinct theoretical paradigm from the rational choice/deterrence perspective (which is given its own discussion in this chapter).

³¹ Although the same measure, Cohen et al. (1980) refer to this as the “residential population density ratio.”

for the informal social control of such youths. Second, and perhaps less directly relevant to the issue of criminal opportunity *per se*, high rates of primary individuals—or heads of households without relatives living with them—may increase their level of anonymity which would make crimes easier to commit (Felson and Cohen, 1980).

Given this conceptual basis, many of the tests of routine activities theory as originally formulated by Cohen and Felson afforded it considerable empirical support. Tests have emerged that have shown variables from routine activities theory to be significant predictors of multiple types of offenses such as rates of burglary (Robinson and Robinson, 1997), arson (Stahura and Hollinger, 1988), and homicide (Kennedy and Silverman, 1990; Messner and Tardiff, 1985). Empirical analyses have also revealed support for the theory at the national level (Bennett, 1991), when restricted to urban areas (Messner and Blau, 1987), for census tracts (Copes, 1999), and at the block level (Roncek and Bell, 1981).

Nevertheless, researchers have pointed out potential problems with both the theory's central propositions and the way in which researchers have typically tested the theory. For example, Carroll and Jackson (1983) argued that the effect of criminal opportunity on rates of predatory crimes should be mediated by social stratification (i.e., inequality) for two reasons. First, the expansion of women into the workforce since World War II has occurred disproportionately for white rather than black women. This disparity is likely to result in wider racial inequality. Second, the number of female-headed households has increased over the same time period, which may have amplified conditions of income inequality since female-headed households are likely to have a lower average income than male-headed households. Thus, Carroll and Jackson

(1983:180) contend that “the direct effect on predatory crime rates, which [Cohen and Felson] attribute to the household activity ratio, may in fact be indirect, operating through an unmeasured intervening variable—income inequality.” Consequently, their study found that inequality did operate as an intervening variable between the household activity ratio and rates of predatory crime.³²

Despite this potential problem with the conceptual basis of routine activities theory, others have critiqued not the theory itself, but rather how researchers have typically gone about testing the theory. In particular, most tests of routine activities theory virtually ignore the *motivated offender*³³ portion of Cohen and Felson’s framework. This may not be surprising, however, given the lack of substantive attention paid by Cohen and Felson themselves to those factors that may influence (or proxy, for that matter) the size of the pool of motivated offenders. For a tabular summary of these tests see Appendix 5. Even so, researchers have attempted to correct this potential shortcoming in the body of empirical tests of routine activities theory.

In particular, Bryant and Miller (1997) sought to extend Cohen and Felson’s theory by adding the notion of *labor market segmentation*, and specifying its relationship to the motivated offender construct in routine activities theory. Drawing largely on the work of Currie (1985), Bryant and Miller (1997:74) noted that a stratified labor market,

³² The mediating effects of inequality, however, were stronger for rates of property offenses (burglary) than for direct, person-to-person predatory crimes (homicide, rape, and aggravated assault).

³³ This omission may be due, in part, to the conceptual overlap of macro-level measures such as “unemployment” with the concepts of “motivated offenders” and “lack of capable guardianship” and the theoretical ambiguity associated with each. For example, Pratt and Lowenkamp (1999) noted that studies will often specify conflicting relationships between high rates of unemployment and crime—where some see it as producing more frustration among collectives and therefore more motivated offenders (and more crime), others see high unemployment as increasing guardianship and therefore reducing crime. This fact has led some (see, e.g., Copes, 1999:128; Mustaine and Tewksbury, 1997) to advocate disaggregating tests of the routine activities model to the prediction of specific rates of crime under the assumption that property crimes and violent crimes may have different “opportunity structures.”

where there is a wide gap between the high and low ends of “quality” jobs, is likely to produce a sizeable “secondary labor market.” Currie (1985:113) argues that “It is the condition of being locked into . . . the ‘secondary labor market’—low-level, poorly paying, unstable jobs that cannot support a family and that offer little opportunity for advancement—that is most likely to breed crime.”

To test the effects of labor market segmentation on crime rates at the city level, Bryant and Miller constructed a modified version of the *location quotient*.³⁴ This measure indicates whether a particular city’s labor market is over- or under-represented in a specific area of the labor market (in the present case, the secondary labor market) relative to the other cities in the sample. Controlling for other structural characteristics in cities such as racial and economic distributions, the prevalence of commercial establishments (to proxy the availability of “targets”), police presence, population density, and the household activity ratio, their measure of labor market segmentation was somewhat inconsistently related to crime rates in two time periods (1980 and 1990). In particular, the location quotient was not a significant predictor of either violent or property crime rates in 1980, and neither was the household activity ratio. Identical equations predicting 1990 crime rates, however, revealed more support for the labor market segmentation hypothesis; its effect on violent crime rates was comparable to that of the household activity ratio (beta weights of .17 and .28, respectively), and it was significantly related to property crime rates (where the household activity ratio was not).

³⁴ This measure is generally used to determine whether a regional economy is either over- or under-represented in a particular industry relative to the national distribution of employment (see Brantingham and Brantingham’s 1995 analysis of “hot spots”).

Summary of Routine Activities Theory

Much like the way researchers have approached the absolute deprivation/conflict and the relative deprivation/inequality models, routine activities theory has been extensively tested empirically at the macro-level. Despite considerable similarities in research design approaches across studies (e.g., the consistent use of the household activity ratio to proxy the effects of attenuated guardianship), questions still remain as to whether (and to what extent) the effects of variables from routine activities theory are mediated by other macro-structural factors. In particular, the degree to which the effect of measures such as the household activity ratio on crime rates is sensitive to statistical controls for other variables such as income inequality, or more precise measures of “motivated offenders.”

It is also still uncertain at this time as to whether variables from routine activities theory fair better when predicting rates of property versus violent crime. Indeed, both types of offenses can, under certain conditions, be considered “predatory” (the focal concern of Cohen and Felson). Nevertheless, certain variables that are typically used in tests of routine activities theory—such as unemployment rates—may predict conflicting effects (i.e., both inverse and positive relationships) on crime rates. Thus, it may be safe to conclude at this time that despite the plethora of empirical tests of routine activities theory, significant questions still linger about the conditions under which it may be a relatively powerful or more modest explanation of crime rates.

DETERRENCE THEORY

Key Propositions and Substantive Content

In the late 1970s, the implicit notion of the rational criminal contained in routine activities theory was not unique to Cohen and Felson's work (e.g., choosing crime targets; avoiding capable guardians). Research by rational-choice economists such as Gary Becker (1968), coupled with an emerging concern over the potential impact that growing incarceration levels may have on crime rates, resulted in a renewed interest in the deterrence-rational choice theory of crime. Much of the deterrence research from the 1980s focused on "perceptual deterrence" at the individual level using self-report data (Paternoster, 1987). Nevertheless, its popularity during a period ruled by "get-tough" crime policy rhetoric resulted in a lengthy roster of macro-level tests of deterrence theory (Blumstein, Cohen, and Nagin, 1978; Wilson, 1983).

Dating back to the original formulations of Bentham and Beccaria, deterrence theory is perhaps the most straightforward in its explanation of crime relative to the other macro-level theories. Simply put, deterrence theory assumes that offenders exercise rational judgement and are reasonably aware of the potential costs and benefits associated with criminal acts. This assumption translates generally into the proposition that aggregate crime rates in an ecological region can be curbed by the crime-control activities of the criminal justice system (i.e., by increasing the potential "costs" and probable "risks" for criminal behavior). Such activities may come in the form of more rigorous police practices (e.g., crackdowns, increasing clearance rates), prosecuting offenders more efficiently, and/or legislatively increasing the severity of certain criminal

sanctions (Greenberg, Kessler, and Logan, 1979; Logan, 1975; Wilson and Boland, 1978). The central empirical claim of deterrence theory, therefore, is that these types of public efforts “matter,” and that they should, independent of other macrosocial processes (e.g., those specified by the other theories discussed in this chapter), have an appreciable effect on rates of crime.

Variable Specification and Measurement

Despite the apparent simplicity of the theoretical framework offered by deterrence theory, determining its empirical validity has not been so easy. This difficulty is not due to a lack of existing tests of propositions from deterrence theory. Indeed, it is one of the more extensively tested macro-level theories of crime (for a summary of these studies, see Appendix 6). Rather, a number of methodological problems have plagued the deterrence literature since the late 1970s and 1980s that have muddied the waters concerning whether, and under what conditions, variables from deterrence theory will significantly predict rates of crime.

Compounding this problem is the diversity in approaches to testing deterrence theory; cross-sectional and longitudinal analyses, and studies based on variables related to the police, judicial decision-making, and the potential deterrent and incapacitation effect of prisons all coexist in the deterrence literature. Nevertheless, three broad groups of macro-level deterrence studies can be identified, all of which are discussed below: (1) interrupted time series analyses, (2) ecological studies of the impact of police practices, and (3) ecological studies of the deterrent and incapacitative qualities of prisons.

Interrupted Time Series Analyses. Interrupted time series analyses are generally intended to assess the impact of specific deterrence-based policy interventions on crime rates. Many of these studies have examined the potential deterrent effect of policies targeting drunk driving (Ross, 1982), police crackdowns on drug markets (Reuter, Haaga, Murphy, and Praskac, 1988; see also Mazerolle, Kadleck, and Roehl, 1998), and gun control laws (Loftin and McDowell, 1984; McDowell, Loftin, and Wiersema, 1992). Reviews of this literature consistently reach the conclusion that the effects of such policies on crime rates are “generally only transitory: the initial deterrent effect typically begins decaying even while the intervention is still in effect” (Nagin, 1998a:9; see also Nagin, 1998b). While the decaying effect may not always be complete (see Sherman, 1990), the general trend in these analyses is that, on balance, they reveal little empirical support for deterrence theory.

A more sizeable body of empirical literature concerns the potential deterrent effect of the death penalty on crime rates. Many early studies on the subject contained simple comparisons of the homicide rates of states that had the death penalty “on the books” versus those that did not. While these studies did not indicate support for deterrence theory (see, e.g., Sellin, 1959, 1967), problems associated with temporal ordering and the lack of statistical controls for other aggregate-level predictors negated any firm conclusions based on their results.

In response to these limitations, researchers began to study the potential deterrent effect of the death penalty with more sophisticated multivariate and time-series research designs (Bailey, 1980, 1983, 1990; Black and Orsagh, 1978; Bowers and Pierce, 1980; Decker and Kohfeld, 1990; Peterson and Bailey, 1988, 1991). Consistent with their less

methodologically rigorous predecessors, these studies also yielded little or no support for the deterrence perspective (cf. Stack, 1987, 1990).³⁵ Perhaps the most methodologically rigorous study of the impact of the death penalty was conducted by Cochran, Chamlin, and Seth (1994).³⁶ In a time series analysis of the “naturally occurring experiment” of Oklahoma’s return to the death penalty, Cochran et al.’s series of ARIMA models found no evidence of a deterrent effect even after disaggregating the homicide time series into felony murder and stranger homicides. In fact, their analyses revealed a mild but statistically significant “brutalization effect,” where the public execution in Oklahoma actually produced a corresponding increase in stranger homicides.

Deterrence and the Police. Ecological studies of the possible deterrent effect of the police on crime rates are fairly ubiquitous (Sampson and Cohen, 1988; Wilson and Boland, 1978). Yet, Nagin (1998a, 1998b) notes that most of these studies suffer from a common problem of “endogeneity” (see also Nagin, 1978). The endogeneity problem arises in cross-sectional ecological studies, using cities as the unit of analysis for example, where there is an assumption of one-way causation between a deterrence theory variable such as “police force size” (generally standardized to police per capita) and crime rates. In this instance, it may be reasonable to contend that cities experiencing high

³⁵ Some of the works that have revealed a significant deterrent effect for the death penalty—for example those conducted by Ehrlich (1975a), Phillips (1980), and Stack (1987)—have been refuted as other scholars have taken the original authors’ data and reanalyzed it using more appropriate statistical techniques and found no evidence of a deterrent effect (see, e.g. Bailey and Peterson, 1989; Bowers, 1988; Bowers and Pierce, 1975).

³⁶ Cochran et al.’s (1994) study may be considered the most methodologically rigorous analysis of the effect of the death penalty on homicide rates for three reasons. First, the Oklahoma execution was geographically isolated (i.e., no surrounding states conducted a similar execution at the same time to produce a “history effect”). Second, the researchers were able to gather weekly estimates of state-level homicides from the FBI’s Supplemental Homicide Reports (SHRs) to avoid temporal aggregation bias. Finally, the SHRs allowed for separate analyses of felony murder and stranger homicides to avoid potential offense aggregation bias.

levels of crime are likely to increase the size of their police departments to combat the perceived threat of crime. Thus, the failure to adequately deal with the problem of endogeneity (or, “feedback” causation) may result in the inflation of the estimated deterrent effect of the independent variable. This problem is also present for deterrence measures such as the “arrest ratio” or “number of arrests” on crime rates in cross-sectional studies (see, e.g., Brown, 1978; Geerken and Gove, 1977; Tittle and Rowe, 1974). Further complicating this matter in both cross-sectional and longitudinal analyses of arrests on crime involves having to deal with the issue of a “tipping effect” (cf. Chamlin, 1988, 1991; Greenberg and Kessler, 1982; Greenberg et al., 1979; Logan, 1975).

Researchers have typically dealt with the endogeneity problem in one of two ways. First, studies have used lagged statistical models (e.g., estimating the effect of police resources at “time 1” on crime rates at “time 2” and vice versa) to test for possible reciprocal effects (Marvell and Moody, 1996). Others have attempted to specify an “identification restriction” in the analysis by controlling for an additional factor that may be related to the independent variable but not the dependent variable. For example, Levitt’s (1997) study of police force size and crime rates in U.S. cities controlled for the timing of mayoral elections, which is assumed to affect police force sizes, but should be unrelated to crime rates. Both of these approaches have revealed a certain measure of empirical support for deterrence theory, but it is also important to note that they are the exception; most studies fail to account for the endogeneity problem and therefore may be overestimating the effect of police activity on crime rates.

Deterrence, Incapacitation, and Prisons. The endogeneity problem is also present in the research evaluating the potential deterrent effect of prisons. In particular, cross-sectional analyses of the effect of prison populations on crime rates that fail to simultaneously estimate the effect of crime rates on prison populations may artificially increase the level of support for deterrence theory (Nagin, 1998a). In an effort to avoid this problem, similar to his work on police resources, Levitt (1996) employed an identification restriction—court orders to reduce prison crowding (which was assumed to affect prison populations but not crime rates)—that plausibly dealt with the issue of endogeneity in the effect of prison populations on crime rates. While his analysis did indicate a degree of support for deterrence theory, this type of methodological approach is still in the minority; most studies fail to include an identification restriction, and may therefore be overestimating the potential deterrent effect of prison populations on crime rates.

The dubious status of the deterrent effect of prisons is exacerbated by the difficulty in attempting to disentangle the deterrent from the incapacitative effects of prisons. For example, some studies contend that up to fifteen index crimes can be avoided per year by incarcerating one offender (Levitt, 1996; see also the similar estimates derived by Spelman, 1994). Other studies with less liberal estimates of the stability of offending over the life-course (i.e., studies that control for the decline in criminal behavior as offenders age), however, reach the conclusion that the “incapacitation effect” is negligible (Zimring and Hawkins, 1995; see also Visser, 1987). Thus, the literature on the independent effect of incapacitation on crime rates is just as equivocal as the evidence concerning the deterrent effect of prisons.

Summary of Deterrence Theory

Whether macro-level deterrence research comes in the form of interrupted time series analyses, ecological studies of the potential deterrent effect of the police, or examinations of the crime control capacity of prisons, a similar theme runs through each set of studies: the empirical support for deterrence theory is, at best, mixed. Some studies appear to demonstrate a certain measure of support for the theory, yet the overall trend in the literature seems to be that the ability of deterrence variables to significantly predict crime rates is weakest among the studies that are strongest methodologically.

Certain reviewers of the deterrence literature have had no trouble ignoring this fact, however, and have continued extol the virtues of the theory. For example, one early review by Blumstein, Cohen, and Nagin (1978:7) concluded that: “The evidence certainly favors a proposition supporting deterrence more than it favors one asserting that deterrence is absent.” Others are less cautious in their claims that “the criminal justice system, ineffective as it may seem in many areas, has an overall crime deterrent effect of *great magnitude*” (Cook, 1980:213, emphasis added). Even more recent reviews of deterrence research have reached the more “emphatic conclusion” that “the collective actions of the criminal justice system exert a *very substantial deterrent effect*” (Nagin, 1998a:3, emphasis added).

Given the above discussion of the three groups of macro-level deterrence studies, a more tentative conclusion regarding the empirical status of deterrence theory may be warranted. In all fairness, it would be overly pessimistic to assume that rigorous police practices would have *no* effect on the crime rate. By the same token, it would be

counterintuitive to argue that the incapacitation of offenders in prison would result in *no* reduction in crime. Even so, perhaps the question of the empirical status of deterrence theory could be asked in a more substantively meaningful way than simply determining “whether” deterrence variables are related to crime rates. In particular, at this point it is still unclear as to what the effect on the crime rate of deterrence variables is *relative* to other variables specified by competing macro-level theories of crime. In addition, existing reviews have yet to provide any concrete information regarding how the effect of deterrence variables are conditioned by methodological variations across all macro-level studies (e.g., across models that do, or do not, estimate reciprocal effects). Perhaps the meta-analytic approach taken in the present study will provide more definitive answers to these questions.

SOCIAL SUPPORT/ALTRUISM THEORY

As discussed above, macro-level theories of crime generally fall into one of two broad categories: “motivational” theories (e.g., social disorganization, anomie/strain, absolute deprivation/conflict, and relative deprivation/inequality theories), and “opportunity” theories (routine activities theory and, to a certain extent, deterrence theory). The emerging social support/altruism theory in macro-criminology signifies, however, a bit of a departure from the motivation-opportunity dichotomy. While this recent theoretical development pays a considerable amount of homage to its theoretical predecessors, its central focus is qualitatively different. In particular, instead of looking at the ways in which collectives of individuals may or may not be *controlled* by social-

structural conditions, social support/altruism theory has been developed to explain how certain characteristics of social aggregates, through social support and/or altruistic actions, may *insulate* them from experiencing high rates of crime.

Key Propositions and Substantive Content

Social support/altruism theory is rooted primarily in the works of Braithwaite's (1989) theory of reintegrative shaming, Cullen's (1994) development of social support as an organizing concept for criminology, Messner and Rosenfeld's (1994, 1997a) institutional anomie theory, and Chamlin and Cochran's (1997) discussion and empirical test of social altruism theory. Each of these theoretical statements are conceptually distinct when viewed in their entirety, yet they all draw on a common proposition that social aggregates—from communities to nations—vary in their degree of cohesiveness, support, shared values, and willingness to come to the aid of those in need. Accordingly, such variations are assumed to be related to crime rates.

These insulating properties of social aggregates have been labeled both as “social support” (Cullen, 1994:527) and as “social altruism” (Chamlin and Cochran, 1997:203). Cullen (1994) borrows Lin's (1986:18) definition of social support as “the perceived or actual instrumental and/or expressive provisions supplied by the community, social networks, and confiding partners.” Cullen (1994:527) goes on to contend that “whether social support is delivered through governmental social programs, communities, social networks, families, interpersonal relations, or agents of the criminal justice system, it reduces criminal involvement.” In contrast, Chamlin and Cochran (1997:204) define social altruism, which is also predicted to vary inversely with crime rates, as “the

willingness of communities to commit scarce resources to the aid and comfort of their members, *distinct from the beneficence of the state*” (emphasis added).

Thus, despite considerable overlap in their core theoretical propositions, it seems that the central disagreement between the two perspectives lies in two areas. The first difference concerns whether the state is capable of being supportive and/or altruistic. Where Cullen (1994) answers this question in the affirmative (e.g., rehabilitation programs), Chamlin and Cochran (1997:209) dismiss the relevance of state-sponsored programs (e.g., financial assistance programs) to their notion of social altruism because they “may not reflect the humanitarianism of localities.” Relatedly, the second difference concerns whether state-based efforts at support/altruism will have any effect on crime rates above those resulting from the non-coerced charitable actions of groups that may be more in the “spirit of local volunteerism” (Chamlin and Cochran, 1997:210).

Variable Specification and Measurement

The empirical questions posed by social support/altruism theory have only recently begun to be addressed by researchers (e.g., whether levels of social support or altruism, and in what form, are related to crime rates). As such, any absence of firm conclusions regarding the theory’s main propositions should not be attributed to diverse or inconsistent research results (as is the case with many other macro-level theories of crime), but rather to the fact that the body of studies testing the theory is still in its infancy. Indeed, social support/altruism theory contains multiple hypotheses that are readily

amenable to empirical investigation, yet its recent arrival on the macro-level scene has resulted in only one *explicit* test of the theory.³⁷

Chamlin and Cochran's (1997) test of social altruism theory³⁸ used United Way contributions at the city-level as a measure of social altruism. Their analysis was conducted on a sample of 279 U.S. cities that reported collecting at least one million dollars in contributions to United Way campaigns. To isolate the effect of the altruism measure on rates of both personal and property crime,³⁹ they included statistical controls in their analysis for economic deprivation variables (both percent below the poverty level and the Gini index of economic concentration), variables related to urbanism (population size, age structure, and racial and ethnic heterogeneity), household opportunity structures (the percentage of single-person households), regional location (the south), and for the social disorganization-type variables of residential mobility (percent of persons five years or older living in different locations in 1990 and 1985) and family disruption (percent divorced).

³⁷ That there is currently only one "explicit" test of social support/altruism theory does not mean that other studies—possibly those purposely testing some other macro-level criminological theory—do not indicate partial support for the social support/altruism perspective. For example, the "decommodification index" used in Messner and Rosenfeld's (1997b) test of institutional anomie theory, which contained national-level estimates of welfare and income assistance benefits to families which were related to crime rates, may be viewed as *de facto* support for social support/altruism. Further, studies addressing the effects of economic assistance programs on crime rates here in the U.S. (see, e.g., DeFronzo, 1983) also reveal a certain measure of implicit empirical support for the present theory (see also Muller, 1985).

³⁸ Consistent with their version of social altruism theory, Chamlin and Cochran (1997:209-210) chose to exclude certain governmental activities, such as transfer payments, from their measure of social altruism because such programs "tend to be determined by a multiplicity of interests and decisions that are made at federal, state, and local levels."

³⁹ The property crime rate was measured as the total number of burglaries, larcenies, and motor vehicle thefts per 100,000 residents, and the violent crime rate was measured as the total number of homicides, robberies, aggravated assaults, and forcible rapes per 100,000 residents.

In the final analyses, the measure of United Way contributions was a significant inverse predictor of rates of both property and personal⁴⁰ crimes net of the statistical controls. It should be noted, however, that the effects of other variables were stronger than those of the social altruism measure. In particular, the racial heterogeneity and family disruption measures were among the more robust and stable predictors of both dependent variables, with standardized regression coefficient values (beta weights) consistently between .20 and .30. Even so, the magnitudes of the relationships between property and personal crime rates and Chamlin and Cochran's (1997) proxy for social altruism were quite respectable (with beta weights of -.12 and -.10, respectively), and stable when subjected to different regression model specifications. Overall, their study indicates that social support/altruism theory, despite its recent birth into the criminological family, may prove in the future to be among the more popular macro-level theories of crime (see Appendix 7 for a tabular summary of these studies).

Summary of Social Support/Altruism Theory

Unlike the previous macro-level theories of crime discussed thus far, social support/altruism theory has not experienced a high level of empirical controversy regarding the typical methodological debates. To be sure, discussions have yet to seriously emerge in the published literature concerning how key variables should be measured, what the appropriate research design may be, or even whether the theory is generally supported across multiple empirical tests. The bulk of the debate has instead

⁴⁰ The natural log of the personal (i.e., violent) crime rate was used in the analysis to correct for heteroscedasticity in the un-logged distribution of error terms around the fixed values of the independent variables.

been devoted to clarifying the central theoretical components of the social support/altruism paradigm. This endeavor is critically important to help guide what will inevitably be a quickly growing body of empirical tests of the theory in the future.

SUBCULTURAL THEORIES

Key Propositions and Substantive Content

Social support/social altruism theory attempts to outline the social and cultural conditions that may insulate individuals from engaging in crime. A similar theoretical tradition exists which follows this logic in that, if cultural influences can reduce the amount of crime in a social aggregate, opposing influences must also be capable of increasing the amount of crime in a given area. The notion that certain cultural conventions can be criminogenic serves as the basis of subculture of violence theories. While there is no universal macro-level subcultural theory of crime, most of the diverse perspectives within this school of thought tend to locate the source(s) of deviant and/or violent subcultures in one of two areas: the South and/or in urban areas.

The Southern Subculture of Violence Thesis. A sizeable body of empirical literature demonstrates that throughout the twentieth century the southern region of the United States has consistently led all others in violent crime rates (Huff-Corzine, Corzine, and Moore, 1986). To explain such a pervasive pattern of regional violence, some researchers have argued that certain cultural norms contained in the South may predispose individuals to not only engage in violent behavior, but also to approve of such actions on the part of others (see, e.g., Gastil, 1971; Hackney, 1969).

Considerable confusion exists, however, concerning which cultural norms in the South are responsible for generating high levels of violence, and also why they tend to be located only in this region of the country. Indeed, researchers have implicated factors such as an historical tradition of chivalry (Hackney, 1969), heightened notions of defensiveness (Erlanger, 1975), and even an exaggerated willingness to resort to violence in cases where the “good name” of a woman may be threatened (Brearley, 1932). Furthermore, explanations as to why these values are so entrenched in the South have ranged from residual bitterness over having lost the Civil War (Hackney, 1969), to more traditional criminological perspectives on family and community socialization (Gastil, 1971), to a dominant religious perspective that carries the view of a vengeful God hell-bent on retaliation (Ellison, 1991a). Still others have posited that a combination of these factors in conjunction with disproportionately high rates of firearm ownership have contributed to the high levels of crime in the South (cf. Dixon and Lizotte, 1987; Ellison, 1991b; Lizotte and Bordua, 1980; O’Connor and Lizotte, 1978). Despite such differences of opinion among researchers, they tend to agree, at minimum, on the proposition that certain elements of southern culture create conditions that consistently breed high levels of violence.

The Urbanism and Crime Thesis. The other major branch of macro-level subculture of violence theories shifts the focus of the influence of cultural values on crime away from just the South to all urban areas. Much like the consistently observed pattern of high crime rates in the South, researchers have also noted the regular appearance of a positive association between the size of the population in a locale and the rate of crime and deviance (see the discussion by Archer and Gartner, 1984). While this

relationship has been discussed in the context of other theoretical traditions discussed in this chapter, such as routine activities theory (Cohen and Felson, 1979; Cohen et al., 1981), it has been explained by others from a subcultural perspective.

In particular, Fisher (1975) developed a theory of urban crime that depicts large population concentrations as the facilitators of unconventional subcultural values—some of which may increase the motivation toward engaging in crime and deviance (see also the discussion by Tittle, 1989). According to Fisher, large urban populations will produce deviant subcultures through a three-step process. First, as an urban population grows larger, the probability that individuals with unconventional interests and/or lifestyles will come into contact with each other increases as well. Second, urban environments provide the context for such scattered unconventional people to form more complete social relationships with one another and to form subcultural groupings. Third, since urban environments tend to foster the development of multiple subcultures, each must compete for survival in what is still a finite amount of geographic and social space. This final element is assumed to lead to the intensification of core subcultural values and therefore greater within-group cohesion among members adhering to the subculture's major tenets. The result of this process is that high rates of unconventional behavior (crime and deviance) will be found in areas with larger populations; and, since subcultural values tend to follow a process of “diffusion” from one generation to the next, the positive association between population size and rates of crime and deviance will tend to persist over time.

Variable Specification and Measurement

Southern Subculture of Violence Thesis. Although a number of empirical explorations into the southern subculture of violence thesis have been conducted at the individual level (see, e.g., Corzine and Huff-Corzine, 1989; Dixon and Lizotte, 1987; Ellison, 1991; Ellison and McCall, 1989; Erlanger, 1975; O'Connor and Lizotte, 1978; Reed, 1972), considerable work has also been done at the macro level. The first macro-level articles to explicitly examine the southern subculture of violence perspective were conducted by Hackney (1969) and Gastil (1971). Using partial correlation analysis at the state-level, both researchers found that, after controlling for urbanization and sociodemographic characteristics of states, variables indicating southern region⁴¹ were among the strongest predictors of homicide rates⁴² in their statistical models.

Both Hackney (1969) and Gastil (1971) interpreted their results as supporting the notion that southern cultural (or subcultural) traditions tend to prescribe and reinforce violence. A replication and extension of these works conducted by Loftin and Hill (1974), however, was less optimistic about the validity of the southern subculture of violence perspective. Loftin and Hill's (1974:715) skepticism of the subcultural thesis stemmed from the fact that neither of the previous studies contained a measure of "culture" other than the regional variable itself. Indeed, Hackney and Gastil's collective decision "to interpret the relationship between region and homicide rates as a cultural effect clearly forces them to assume that all of the non-cultural determinants of homicide

⁴¹ Hackney (1969) used a dummy variable indicating whether or not the state was a former Confederate state. Gastil (1971), on the other hand, employed a "southernness index"; the individual variables comprising this measure were never reported, however, making the interpretation of the results difficult.

⁴² Hackney's (1969) analysis was limited to white homicide rates only (for 1940), whereas Gastil's (1971) study used overall homicide rates as the dependent variable (for 1960).

that are correlated with region have been measured completely and accurately” (Loftin and Hill, 1974:716).

Thus, the primary limitation to the existing macro-level subculture of violence literature, according to Loftin and Hill, was model misspecification error. To correct this problem, Loftin and Hill (1974) replicated the earlier studies but extended them by including what they viewed as more substantively important structural control variables. In particular, they included state-level measures of inequality (the Gini index) and poverty (a structural poverty index) in their multiple regression analyses. The result was that, after controlling for these structural predictors, neither of the variables indicating southern region (the regional dummy variable or the southernness index) were statistically significant predictors of homicide rates. Loftin and Hill therefore concluded that the high levels of homicide in the South were due to high levels of poverty and inequality, and that the simple relationship between “region” and homicide was spurious.

Further complicating the inconsistencies in the southern subculture of violence literature is Blau and Golden’s (1986) analysis at the SMSA level. In extending the theoretical framework originally set forth by Blau and Blau (1982), Blau and Golden’s (1986) study found that, even after controlling for racial inequality and other socioeconomic factors, their measure of southernness (the percent southern-born) still maintained a statistically significant direct effect on rates of murder, assault, and overall violent crime. Furthermore, a consistent interaction effect between the percent southern-born and rates of family disruption (percent divorced or separated) was observed in predicting rates of rape, robbery, assault, and overall violent crime. What is left, therefore, is a fair amount of confusion surrounding what methodological conditions will

make it more or less likely that measures of “the South”—as proxies for “culture” (or subculture)—will significantly predict aggregate rates of crime.

The Urbanism and Crime Thesis. As stated above, the primary independent variable specified by Fisher’s (1975) urbanism and crime thesis is the size of the population in a geographic region. Much like the empirical literature on the southern subculture of violence, the relationship between the size of population and crime has been characterized as “complicated and variant” (Tittle, 1989:273). Some studies find that the relationship is not strictly linear, but that a J-shaped curve exists, where extremely rural areas will often exhibit higher crime rates than small towns (Archer and Gartner, 1984). Other studies do not even find a relationship between size-of-place and crime rates, or may find that the relationship is conditioned by the unit of analysis (cf. Archer and Gartner, 1984; Berman, 1973; Krohn, Lanza-Kaduce, and Akers, 1984; Lodhi and Tilly, 1973). Still others find an association between population size (as a statistical control variable) and crime while testing a different macro-level theoretical perspective (see, e.g., Blau and Blau, 1982; Blau and Golden, 1986; Wilson, 1985).

Summary of Subcultural Theories

Much like the empirical status of other macro-level theories of crime, the body of tests of subculture of violence theories reveals equivocal results. Tests have emerged that (1) support a particular version of subcultural theory (either the southern or urban variation of the theory), (2) find no support for the perspective, (3) find evidence of a subcultural effect in an unexpected way, while still others (4) indicate partial support for the perspective under certain methodological conditions (for a tabular summary of these

studies see Appendix 8). The meta-analysis of these studies should uncover the degree to which the methodological differences across subculture of violence studies “matters” to the overall relationships between the key theoretical variables. This information should, in turn, aid in our understanding of the validity of the subcultural perspective relative to the other macro-level theories of crime.

CONCLUSIONS

The seven major macro-level theories of crime discussed in this chapter are all conceptually unique in their own way. Some focus on the social conditions that tend to breed high levels of criminal “motivation,” while others specify how criminal “opportunities” may emerge due to certain social-structural factors. Some theories emphasize the role that variations in the formal or informal “control” over collectives of individuals condition aggregate rates of crime, while others see the “support” of such groups as being more important. Still others attempt to integrate these constructs under the same theoretical framework.

How researchers have gone about testing these theories seems equally as diverse. Studies using cross-sectional and time series designs exist between multiple tests of the same theory. Studies also vary in what types of crime rates are being predicted (e.g., total violent versus property crimes; or rates of specific offenses). Further, studies of nations, states, counties, cities, SMSAs, blocks, census tracts, and neighborhoods have all been used in ecological studies of crime. Finally, while estimates of reciprocal causation have appeared in a few tests of many of the macro-level theories of crime, researchers have yet

to consistently embrace the method—even when the existing research (and theory) indicates that it may be necessary.

The plurality of research findings from the overall body of ecological studies may be viewed as at least partially responsible for the current lack of consensus concerning which macro-level theories are, and are not, well-supported empirically. Further, existing reviews of ecological studies of any particular theory offer few firm insights as to the relative predictive power of the major macro-level theories of crime. Indeed, such “narrative” reviews fail to provide any precise estimates of relationships between key theoretical variables across studies. The present study being proposed is intended to remedy this problem by systematically determining whether, to what degree, and under what methodological conditions certain macro-level variables are related to crime rates through the use of meta-analytic methodology. Perhaps this approach will signify a sizeable first step toward a better understanding of how the major macro-level theories of crime “stack up” against each other.

CHAPTER 4

RESEARCH METHODS

As stated previously in Chapter Two, in contrast to narrative reviews of empirical literature, meta-analyses have four potential advantages. First, they can provide a more precise estimate of the relationship, across all tests, of theoretical variables to crime. Second, they can allow for multivariate analyses in which researchers can explore whether the effect size of theoretical variables differs significantly under certain methodological conditions (e.g., in longitudinal versus cross-sectional studies; when variables are operationalized differently). Third, because coding decisions are “public,” meta-analyses can be replicated by other scholars. Fourth, the database is not static but dynamic: as additional studies are published, they can be added to the sample of studies and relationships reassessed.

To date, scholars have generally not subjected *criminological theories* to meta-analyses. Meta-analyses in the criminal justice-criminology literature have been used to address system dynamics in criminal justice, such as racial disparities in sentencing (Pratt, 1998), the relative cost-effectiveness of correctional alternatives (Pratt and Maahs, 1999), and the effectiveness of correctional treatment interventions (Andrews, Zinger, Hoge, Bonta, Gendreau, and Cullen, 1990; Lipsey and Wilson, 1998). Works have also emerged that have used the technique to assess the relative influence of various predictors of crime/delinquency (Lipsey and Derzon, 1998; Loeber and Stouthamer-Loeber, 1986), of recidivism (Gendreau and Goggin, 1996), and of crime rates (Hsieh and Pugh, 1993). Nonetheless, with few exceptions (see Pratt and Cullen, 2000), our

understanding of the empirical status of the major *theories* of crime relies virtually exclusively on narrative literature reviews (see, e.g., Akers, 1997; Burton and Cullen, 1992; Kempf, 1993).

THE CURRENT STUDY

The central purpose of the current study, therefore, is to move beyond the narrative-review approach and to subject the empirical tests of macro-level theories and predictors of crime to a meta-analysis. In doing so, three issues will be addressed. First, the meta-analytic technique is used to assess the “effect size” between all macro-level variables and crime rates used in empirical studies. In particular, the focus will be on whether certain theoretical constructs should be considered as important predictors of macro-level crime rates. If strong and stable ecological predictors of crime are revealed, future studies not incorporating such measures will potentially be misspecified.

Second, the analysis examines whether the effect size between key macro-level theoretical variables and crime rates across studies are influenced by methodological factors. These factors could include differences in the measures used to operationalize certain concepts, variations in multivariate model specifications (e.g., whether including variables from competing theories conditions other macro-level relationships), the use of different research designs (e.g., cross-sectional versus longitudinal research designs), and sample characteristics/level of aggregation. Finally, and perhaps most importantly, the analysis seeks to uncover the relative predictive power of the major macro-level theories of crime. Specifically, by noting the strength and stability of the mean effect size

estimates for the variables specified by each macro-level theory, insights could be gained as to their degree of theoretical validity.

It should also be noted that meta-analyses are not beyond criticism. Wolf (1986) notes that critics of the method tend to focus on two potential problems. First, academic journals (and their editors) may be biased in favor of statistically significant findings, and therefore literature reviews may not uncover every study of a particular hypothesis that has been conducted. Rosenthal (1979) refers to this as the “file drawer” problem due to the tendency of studies failing to reject the null hypothesis to be buried away in file drawers. This omission of null-model studies may limit the utility of meta-analyses that are conducted on published research only. Although this issue cannot be dismissed fully, the potential for bias in the current study may be reduced because a statistical estimate is provided that indicates the number of unmeasured studies that would have to contain a “null finding” to drive the mean effect size estimate down to zero. In other words, it is possible to calculate the number of studies failing to reject the null hypothesis—referred to as the “fail-safe N”⁴³—that would be needed to reverse a conclusion that a relationship exists. This approach is taken in the current study.

The second potential problem, which tends to be the more serious of the two, is that well-done studies may be included with studies using less rigorous methodological designs (i.e., what is referred to as the “apples and oranges” problem), which may bias the overall effect size estimates of the analysis (Cohen, 1977). The primary mechanism for minimizing this problem is to code each empirical study for methodological variations that could influence the effect size estimate(s) (e.g., see Glass, McGaw, and Smith, 1981;

⁴³ This statistic, the “fail-safe N” (Rosenthal, 1979; see also Wolf, 1986), is calculated using a .05 significance level by the formula: $N_{.05} = (\sum z\text{-scores} / 1.645)^2 - N$

Pratt, 1998). Doing so is especially important when integrating the results of studies using correlational research designs (which is often the case in macro-level crime research). Statistical control, as opposed to experimental control, is typically used to assess key theoretical relationships in correlational designs (and in most tests of criminological theories). In turn, estimates of effect size from correlational designs may be contingent, at least in part, on which variables are used as statistical controls, on the composition of the sample, and on how theoretical variables are measured. Thus, controlling statistically such methodological variations across empirical studies becomes necessary for calculating valid and reliable mean effect size estimates. This approach is adopted in the present analysis. Indeed, a central objective of this dissertation is to assess the impact of certain methodological techniques on the effect size of macro-level relationships.

Relatedly, a more general caution should be added that the quality of a meta-analysis is contingent on the quality of research on the topic being investigated. One advantage of using published research—as is used in this study—is that the research studies being analyzed have been vetted for quality by the review process. Still, this does not obviate the fact that measurement error marks virtually all social science research endeavors. To some extent, meta-analysis assumes that error that is idiosyncratic to individual studies “washes out” or is minimized as empirical relationships are examined across a sample of studies. Of more concern, however, are methodological biases that are patterned. To address this possibility, one approach—just mentioned above—is to examine within the meta-analysis whether methodological factors (e.g., measuring a variable one way rather than another) condition the estimates of the effect size. For those

who remain skeptical of the results of the meta-analysis—claiming, for example, that the quality of the sample of studies assessed is suspect—another option remains: probe the meta-analysis for potential shortcomings and then undertake a new, rigorous individual study that attempts to falsify or otherwise revise the conclusions supported by the meta-analysis. In this way, a meta-analysis may prove useful not only in organizing existing knowledge but also in prompting or otherwise guiding future empirical research.

SAMPLE

This dissertation contains a comprehensive review of the existing criminological research conducted on social aggregates. The time frame for the collection of studies for the sample included those conducted between 1960 and 1999.⁴⁴ For studies falling within this time period, the data collection process proceeded in three phases. First, a number of criminal justice/criminology, sociology, and economics journals⁴⁵ were examined, issue by issue, for macro-level studies of crime. Second, a literature search through electronic databases was conducted, including National Criminal Justice Reference Service [NCJRS], Articlefirst, Criminal Justice Abstracts, PsycINFO), for published macro-level analyses predicting aggregate rates of crime. Finally, consistent with the guidelines set forth by Petrosino (1995) prior narrative reviews of criminological literature for tests of

⁴⁴ Although macro-level studies did not begin to emerge in any quantity until the mid-1970s, the lower time boundary of 1960 was established as a buffer to make sure that even the early—yet more sophisticated (e.g., multivariate)—macro-level analyses of crime would be included in the sample.

⁴⁵ The criminal justice/criminology journals included the *American Journal of Criminal Justice*, *Criminology*, the *Journal of Crime and Justice*, the *Journal of Criminal Justice*, the *Journal of Criminal Law and Criminology*, the *Journal of Research in Crime and Delinquency*, *Justice Quarterly*, *Law and Society Review*, and the *Journal of Quantitative Criminology*. The sociology journals included the *American Journal of Sociology*, *American Sociological Review*, *Social Forces*, and *Social Problems*. Finally, the economics journals included *American Economic Review*.

theories were also examined. Studies that did not explicitly test particular macro-level theories, but included aggregate measures predicting crime rates in their statistical models, were also included in the sample. Overall, the sample includes 214 empirical studies, which contained 509 statistical models, which produced a total of 1,984 effect size estimates.⁴⁶

EFFECT SIZE ESTIMATE

The effect size estimate used—the meta-analytic equivalent of the dependent variable—is a standardized correlation coefficient r . This estimate, drawn from each empirical study, was chosen because of its ease of interpretation, and because formulae are available for converting other test statistics (e.g., F , t , chi-square)⁴⁷ into an r (see Rosenthal, 1978, 1984). Using Fisher's r to z transformation (see Wolf, 1986), the

⁴⁶ A number of macro-level studies of crime contained only univariate descriptive statistics or charts of crime rate statistics over time (or prewhitened univariate time series) with no inferential statistics that could be used for the present analysis (see, e.g., Barnett and Schwartz, 1989; Barnett et al., 1980; Boritch and Hagan, 1990; Bursik and Grasmick, 1993b; Cheatwood, 1988; Chilton, 1982, 1986, 1987; Clarke, 1998; Cobb, 1973; Cook and Laub, 1986; Cork, 1999; Deane, 1987; Ebbe, 1989; Fisher, 1980; Gould, 1975; Harries, 1989; Hirschi and Gottfredson, 1983; Holzman, 1982; Loftin and McDowall, 1982; Marvell and Moody, 1991; McPheters, 1976; Neapolitan, 1999; O'Brien, 1996; Parker et al., 1991; Pettaway, 1982, 1985; Phillips, 1980; Rahav, 1981; Sampson, 1983; Sherman et al., 1989; Steffensmeier et al., 1989; Wikstrom, 1990; Wikstrom and Dolmen, 1990; Winsberg, 1993). Other studies provided maximum likelihood parameter estimates (see, e.g., Britt, 1992; Greenberg et al., 1981; McPheters and Strange, 1976; Taylor and Covington, 1988; Vila and Cohen, 1993; Wallace, 1991) or imprecise designations of "significant/non-significant" or "coefficient twice its standard error" (O'Brien, 1987) that could not be converted to a common metric. Still others contained contextual or multi-level analyses of aggregate characteristics with the dependent variable measured at the individual level (see, e.g., Brownfield, 1986; Bursik, 1999; Cohen et al., 1981; Junger and Polder, 1992; Kennedy and Forde, 1990; Krohn et al., 1980; Laub, 1983; Loeber and Snyder, 1990; Lynch and Cantor, 1992; Makki and Braithwaite, 1991, 1994; Maxfield, 1987; McNulty, 1999; Messner and Tardiff, 1985; Moriarty and Williams, 1996; Sampson, 1987; Sampson and Wooldredge, 1987; Stark et al., 1982; Taylor, 1997). Thus, the results of these studies did not contribute to the sample.

⁴⁷ The following equation was used for converting t -values into an r : $r = \sqrt{[(t^2) / (t^2 + df)]}$ where "df" is the degrees of freedom for the effect size estimate ($N-3$).

standardized regression coefficients (zero-order correlation coefficients and/or beta weights) corresponding to each variable gathered from each empirical study were converted to a $z(r)$ score. The standardized regression coefficients were converted to z -values because the sampling distribution of $z(r)$ -scores is assumed to approach normality, whereas the sampling distribution for r is skewed for all values other than zero (Blalock, 1972).⁴⁸ Normally distributed effect size estimates are necessary (1) for the accurate determination of central tendency for fixed values of the control variables, and (2) to allow for unbiased tests of statistical significance (Hanushek and Jackson, 1977; Rosenthal, 1984).

Each $z(r)$ was then weighted for sample size, according to the method recommended by Rosenthal (1984), by taking the product of the $z(r)$ value and the appropriate degrees of freedom (sample size-3) from each study. Weighting the studies on the basis of their sample sizes was done to place a greater emphasis on those studies yielding outcomes from larger samples, which are assumed to be more representative of the population of interest (Rosenthal, 1984; see also Blalock, 1972; Hanushek and Jackson, 1977).

Beta Weights Versus Bivariate Correlations as Effect Size Estimates

Two possible options exist when combining the results of non-experimental, or “correlational,” studies in a meta-analysis. The first option is to use zero-order correlation coefficients (Hedges and Olkin, 1985), which are typically drawn from each

⁴⁸ The equation for the transformation of r values to $z(r)$ values (see Blalock, 1972), which converts the sampling distribution of r to one that approaches normality is: $z(r) = 1.151 \log[1+r/1-r]$

empirical study's correlation matrix (assuming one is provided).⁴⁹ The most significant problem with using such estimates, of course, is that of failing to account for partial spuriousness. In particular, since the potential influences of other predictors of a dependent variable have not been removed, the bivariate correlation between two variables is at a substantial risk of being inflated.

This fact has certainly not been lost on criminologists, who typically deal with correlational research designs. Indeed, the norm in tests of criminological theories is to statistically control for potentially confounding variables so as to isolate the effects of the independent variables specified by the theory and to avoid model misspecification error. The implication of this approach for the quantitative synthesis of such studies is that an alternative effect size estimate may be used: standardized regression coefficients, or beta weights, from multivariate statistical models (see Pratt, 1998; Pratt, forthcoming; Pratt and Cullen, 2000). Beta weights share certain properties with bivariate correlation coefficients (e.g., boundaries of zero and one/negative one, assumptions about the skewness of the sampling distribution), which are largely due to their similarity in mathematical construction. Both are produced by the following equation:

$$\beta \text{ and } r = b [Si/Sd]$$

⁴⁹ Others in this tradition have used similar bivariate effect size estimates such as the Cohen's *d*, which is the difference between two group means divided by the pooled within-group standard deviation (Cohen, 1977; see also Loeber and Stouthamer-Loeber, 1986). Still other researchers have used the RIOC statistic (relative improvement over chance), which scales down certain descriptive statistics into a two-by-two table of whether or not a predictor variable is present and whether or not an individual engaged in delinquency (Loeber and Dishion, 1983). Both of these statistics, however, assume the individual level of analysis and, at minimum, a quasi-experimental research design, and are therefore not applicable to synthesizing macro-level correlational research.

In this equation, both the beta weight (β) and the correlation coefficient (r) are calculated as a linear slope estimate (b) standardized by the ratio of the standard deviations of the independent and dependent variables (Blalock, 1972; Hanushek and Jackson, 1977). The central difference between the beta weight and zero-order correlation coefficient values, then, is that the magnitude of the slope estimate generally decreases from the bivariate to the multivariate model because the variation in the dependent variable explained by other factors has been removed (the ratio of standard deviations stays the same from the r to the β estimates). Accordingly, using beta weights as an effect size estimate (at least for the meta-analysis of criminological research) may produce more valid mean effect size estimates than the inflated coefficients calculated from bivariate correlations because the issue of spuriousness has already been dealt with accordingly.

Nevertheless, the zero-order correlation effect size estimate may be defended by acknowledging that the bivariate estimates are inflated. In particular, this may not be a cause for concern because even though the mean effect size estimates themselves may be inflated, the *relative rank-order* of the magnitudes of the effect sizes of various predictors should be the same. Thus, in the context of macro-level criminological research, taking this position would require making the assumption that model misspecification error plagues *all predictors of crime rates in the same way*. In other words, failing to control for a significant predictor of a dependent variable is assumed to have the effects of random error across all predictors from all studies and therefore leaves the mean effect size estimates uncontaminated and therefore identical in their relative aggregated magnitude.

There is little reason to believe, however, that this assumption will ever be met. Indeed, least-squares and maximum likelihood multivariate estimation techniques all begin with the basic understanding that model misspecification error is, by definition, systematic error that cannot be treated as random fluctuations across studies that affect all predictors in exactly the same way. Supporters of using beta weights as effect size estimates when synthesizing criminological research are cognizant of this fact (see, e.g., Pratt, forthcoming). Accordingly, they tend to view zero-order relationships as, at best, limited in their substantive meaning and, at worst, as dangerously misleading. This logic may even be extended to the suspicion that meta-analysts who choose to use bivariate correlation coefficients as effect size estimates when the beta weight alternative is present are either unaware of—or unconcerned with—how their results are polluted by systematic error.

The bivariate correlation versus the beta weight effect size debate is likely to extend well into the future. Although it is not the purpose of the present analysis to make a declaration of methodological Truth for either position, as stated previously in this chapter the beta weights from each macro-level empirical study will comprise the effect size estimates to be used in the current meta-analysis. Beta weights are used because, consistent with the above discussion, they more closely meet the basic statistical assumptions surrounding random and systematic error. Even so, the bivariate correlations, when provided by each empirical study, will be coded as a method of possible corroboration of the results using the beta weights.

Of course, it would be ideal to compare the unstandardized slope estimates (metric coefficients) for each specified relationship across studies. Such estimates are not

sample specific (as are both zero-order correlation coefficients and beta weights due to potential differences in standard deviations across studies), and are assumed to be the same across random samples. A comparison of the metric coefficients must, however, assume identical measurement techniques on both the independent and dependent variables across studies. Since this is often not the case in macro-level research (even less so in individual-level criminological research), beta weights may represent the most accurate alternative to the unstandardized metric coefficients since they can adjust for different scales of measurement across studies. Nonetheless, similar to coding the bivariate correlations as effect size estimates, the metric coefficients from each study are coded as an added dimension for comparative purposes.

Independence of Effect Size Estimates

As noted, the analysis is conducted on 1,984 effect size estimates drawn from 214 empirical studies. This approach means that most studies contained more than one effect size estimate for a particular empirical relationship (e.g., for different ways of measuring key theoretical variables), and that the number of effect sizes drawn from individual studies varied in number. The rationale for including multiple effect size estimates from certain individual studies is twofold. First and most important, selecting only one effect size estimate from each study that reported multiple estimates would severely limit the possibility of examining how methodological variations in the studies potentially affect the effect size estimates (e.g., does the effect size of a particular relationship differ according to how it is measured?).

Second, it would be difficult to develop a “rule” that would guide which effect size estimate should be selected from any one empirical study. Single data sets are used in multiple published studies, each of which potentially includes a unique set of variables in the multivariate analyses that are presented in the articles’ tables. Selecting only one effect size estimate from these different analyses could introduce, wittingly or unwittingly, a “researcher” bias (see the discussion by Pratt and Cullen, 2000). To correct for the potential biases associated with a lack of statistical independence across effect size estimates, the “fixed effects” correction method discussed in Chapter Two will be employed.

PREDICTOR DOMAINS

The effect size estimates for the key theoretical variables from each major macro-level criminological theory are coded and grouped into “predictor domains,” which generally refers to aggregated mean effect size values for a set of similar predictors (Rosenthal, 1984; see also Gendreau, Goggin, and Gray, 1998). This is not to say that all variables from a test of a theory are lumped into one mean effect size estimate (i.e., this does not entail combining the effect sizes from fundamentally different variables such as age distributions and unemployment measures into one predictor domain for routine activities theory). Rather, only similar measures of the same construct will contribute to a single mean effect size estimate (e.g., effect size estimates from unemployment measures are only combined with those from other unemployment measures).

To minimize the potential biases associated with combining effect size estimates generated from slightly different operational measures, a coefficient of “heterogeneity” in effect size estimates was calculated across studies within each predictor domain (see Glass, McGaw, and Smith, 1981; Wolf, 1986). This measure determines whether the aggregated estimates of central tendency (e.g., mean effect size estimates) are significantly biased due to extreme values that may have been produced by studies too dissimilar to be combined.⁵⁰ As an added precaution, any substantively significant within-predictor domain differences in measurement are noted, and categorical variables reflecting those differences were constructed accordingly (see the following section of the present chapter on methodological controls).

The variables for which effect size estimates were gathered are outlined below. It is important to note, however, that the placement of a given variable under a particular theoretical heading can be somewhat arbitrary.⁵¹ Indeed, as stated previously in this chapter and in Chapter Three, aggregate measures of “unemployment” are claimed by multiple macro-level theories of crime, which may specify and predict their effects in different ways. The purpose of a meta-analysis is not to settle the theoretical portion of this debate (e.g., which theory “should” be able to claim a particular predictor as its own).

⁵⁰ The test statistic for heterogeneity (often referred to as Q-statistics) are based on the following equation:

$$\chi^2 = \sum(zr - Mz)^2$$

This statistic is distributed as a chi-square with k-1 degrees of freedom where k is the number of effect size estimates (zr) in the predictor domain, and Mz is the mean effect size estimate for the predictor domain.

⁵¹ As an added caveat, it should also be noted that, like most other techniques involving statistical control (as opposed to experimental control) meta-analysis is not intended to necessarily settle issues of causality. For example, if a strong mean effect size is found between variables such as *residential mobility* and *crime*, unless reciprocal effects have been well estimated, the meta-analysis cannot determine whether changes in the independent variable actually cause changes in the dependent variable. Rather, what the meta-analysis can uncover is the extent to which the two factors covary.

Rather, the meta-analysis will reveal the strength and stability of such predictors across studies—regardless from what theoretical tradition they may be framed.

Social Disorganization Theory.

The effect size estimates from variables coded into predictor domains from social disorganization theory originally specified by Shaw and McKay included measures of *racial heterogeneity* (including a measurement control variable, where 0 = percent non-white, 1 = percent black, and 2 = a heterogeneity index); *socioeconomic status* (with a measurement control variable with values of 0 = median household income, 1 = mean household income, and 2 = SES index); and *residential mobility*. Furthermore, following the recent theoretical advances set forth by Sampson and colleagues, effect size estimates from measures of *family structure/disruption* were coded as well (including a measurement control variable where 0 = percent divorced or separated, 1 = single-headed households, and 2 = female-headed households).⁵² Effect size estimates were also coded for the variables specified as intervening mechanisms for the traditional social disorganization theory variables, including measures of *collective efficacy* and *unsupervised local peer groups*.⁵³

⁵² The major methodological issues in social disorganization theory, such as the level of analysis, will be addressed in the section of this chapter dealing specifically with the impact of methodological variations on the effect size of key theoretical relationships.

⁵³ Other less common variables used in certain tests of criminological theories (e.g., social disorganization theory), that may be quite similar to existing measures, will be combined under a common predictor domain. For example, a variable such as “neighborhood organizational participation” (see Sampson and Groves, 1989) may be viewed as a forerunner to “collective efficacy” (i.e., the willingness of citizens to get involved in the community and to come to the aid of one another), and therefore the two measures may be combined under the “collective efficacy” predictor domain. If, however, a test for heterogeneity reveals that the measures are too dissimilar to be combined, they will be analyzed separately.

Anomie/Strain Theory.

As discussed in Chapter Three, few direct tests of anomie/strain theory exist at the macro level. Indeed, only those conducted by Chamlin and Cochran (1995) and by Messner and Rosenfeld (1997) can be treated as true tests of the theory. Thus, the small number of empirical studies examining anomie/strain theory is likely to preclude any substantive analysis of the effect of methodological variations on outcomes across studies. Nevertheless, mean effect size estimates may still be calculated for anomie/strain theory variables, which may then be used for comparative purposes between macro-level theories. Accordingly, effect size estimates were gathered for measures of the *strength of noneconomic institutions*.⁵⁴

Absolute Deprivation/Conflict Theory.

As stated in Chapter Three, empirical tests of absolute deprivation/conflict theory are generally uniform in their use of poverty rates to proxy the degree of absolute deprivation experienced by a social aggregate in predicting crime rates. Effect size estimates for measures of *poverty* were therefore coded from empirical studies.⁵⁵ Measurement control variables were also included reflecting whether poverty was measured as the percent below the *poverty line* (code = 0) or another *poverty index* (code

⁵⁴ As a diagnostic procedure, tests for heterogeneity will be conducted on the effect size estimates from the measures used in the studies by Chamlin and Cochran (1995) and Messner and Rosenfeld (1997) to determine whether they may be combined under the predictor domain of the strength of noneconomic institutions.

⁵⁵ As stated in Chapter Three, certain tests of conflict theory (e.g., Chamlin, 1989) specifically examined the “threat hypothesis.” The variables used in these studies, however, will be included under different theoretical headings. In particular, one threat hypothesis variable, percent non-white, is included under the “sociodemographic and other predictor variables” section. Others, such as inequality, are included under the “relative deprivation/inequality” section.

= 1), for whether the poverty measure was *racially heterogeneous* or *homogeneous* (coded as 0 and 1, respectively), and for whether the poverty rates were broken down by *gender* (0 = male poverty rates, 1 = female poverty rates).

Relative Deprivation/Inequality Theory.

Relative deprivation/inequality theory is also fairly straightforward in that its key theoretical variable—income inequality—has been consistently measured across empirical studies as some form of the Gini index of family income or as group mean differences in socioeconomic status. Thus, effect size estimates for *inequality* were coded from empirical studies (including a measurement control variable for whether inequality was measured as the Gini index, code = 0, or by another inequality index, code = 1). Consistent with the discussion of relative deprivation/inequality theory in Chapter Three, a control variable is included for whether the inequality measure used in a particular study was *racially homogeneous* or *heterogeneous* (coded as 0 and 1, respectively).

Routine Activities Theory.

As discussed in Chapter Three, empirical tests of routine activities theory generally assess the effects of two sets of variables on crime rates: the absence of capable guardianship and the presence of motivated offenders. The *household activity ratio* is generally used as a proxy for attenuated guardianship in tests of routine activities theory, and the *unemployment rate* is often treated as a measure of the presence of motivated

offenders.⁵⁶ Thus, effect size estimates for both sets of variables were coded from applicable studies.

A number of categorical variables were constructed to test whether the effect size of unemployment rates on crime rates is conditioned by how the unemployment rate was measured. The first measurement control variable taps into the unemployment variable was measured as the *overall rate* (0 = no, 1 = yes). If the measure was not the overall rate, dummy variables were constructed to reflect a *gender dimension* (0 = male unemployment rate, 1 = female unemployment rate), whether the measure was *age restricted* (0 = no, 1 = yes), whether the unemployment rate was *racially heterogeneous* or *homogeneous* (coded as 0 and 1, respectively), and whether the *length of unemployment* was considered in the measure (coded as 0 = no, 1 = yes).⁵⁷

Deterrence/Rational Choice Theory.

Three sets of variables have been used in macro-level tests of deterrence theory, and corresponding effect size estimates were coded accordingly. First, effect size estimates were gathered from studies testing the effect of *incarceration* on crime rates. Second, effect size estimates from measures of the effect of *police activities* on crime rates were coded. A categorical variable to control for measurement differences was also

⁵⁶ Assuming a non-significant chi-square test for heterogeneity, measures of labor market segmentation (see Bryant and Miller, 1997), which are few in number, will be combined under the “motivated offender” predictor domain for certain portions of the following analyses.

⁵⁷ It should be noted that the “unemployment rate” has been used as a central explanatory variable and as a statistical control variable in multiple macro-level studies that did not focus on routine activities theory. Nevertheless, for the purposes of the present meta-analysis, the concern is not to rectify this theoretical ambiguity surrounding which theory is the rightful owner of “unemployment rates,” but rather to determine (1) its mean effect size on crime rates, and (2) the degree to which it is conditioned by methodological variations.

constructed for the police activities predictor domain (coded as 0 = police force size, 1 = arrest ratio, 2 = police per capita, 3 = police expenditures).

Furthermore, consistent with the discussion in Chapter Three, this group of studies is characterized by the frequent use of cross-sectional research designs. Thus, another dummy variable was included in the analysis to reflect whether the researcher(s) included an *identification restriction* (where 0 = no and 1 = yes). The third set of variables used in tests of deterrence theory involves the effect of *get tough policies* on crime rates. A measurement control variable was also included reflecting what type of get-tough policy was specified (0 = police practices, such as a “crackdown,” 1 = sentencing policy or initiative, 2 = firearms policy, and 3 = the death penalty). If the policy targeted firearms, a dummy variable was also coded for whether the purpose of the policy was for gun control (coded as 0) or to allow for citizens to carry concealed weapons (coded as 1).

Social Support/Social Altruism Theory.

Much like the body of empirical tests of anomie/strain theory, few formal tests of social support/social altruism theory exist. Consequently, the small number of potential effect size estimates available will negate the possibility of conducting any higher-order analyses of the effect of methodological variations on outcomes across studies. Even so, as with anomie/strain theory, mean effect size estimates may still be calculated, which may then be used for comparative purposes between macro-level theories. Therefore, effect size estimates were gathered for measures of *social support/social altruism*. Should chi-square tests for heterogeneity reveal that the effect size estimates being

combing exhibit statistically significant heterogeneity, a dummy variable will be used to determine the mean effect size across variables for *public* and *private* social support (coded as 0 = public; 1 = private).

Subcultural Theory.

Two sets of predictor domains were constructed for variables indicating a subculture of crime effect. First, variables tapping into an *urban subculture* were coded from empirical studies. A dummy variable was also included indicating whether the variable was measured as population size, or some other urban measure (coded as 0 and 1, respectively). Second, variables that proxy a *southern subculture* effect were coded from relevant studies. A categorical variable for this predictor domain was included to reflect whether the “southernness” variable from each study was a regional dummy variable (code = 0), some form of a southern index (code = 1), or by the percent southern-born (code = 2).

Also, consistent with the southern subculture of violence thesis, effect size estimates indicating the effect of *religion* were coded from relevant studies. A measurement control variable was included for how the religion variables were operationalized (coded as 0 = religious participation, 1 = percent Protestant, 2 = other religion measure). Finally, although there is considerable overlap for the variable of *firearms ownership* between subcultural and deterrence theories, indicators of the effect of this predictor were coded from studies as well. In addition, a dummy variable was included to control for whether the measure addressed overall firearms ownership (coded as 0) or handgun ownership only (coded as 1).

Sociodemographic and Other Predictor Variables.

A number of macro-level variables have been used in empirical studies simply for the purpose of statistical control. Predictor domains were also constructed for these variables. Accordingly, effect size estimates were gathered for measures of *age effects* (including a dummy variable for 0 = percent in theoretically-relevant age category, 1 = age cohort effect), *structural density* (including measures often labeled “population density”), *educational variables* (including a categorical control variable for 0 = percent high school graduates, 1 = mean or median teachers’ salaries, or 2 = other education measure), and the *sex ratio—male* (calculated from either measures of percent male or percent female). A tabular summary of studies that were included in the meta-analysis because they included these types of measures (but may have been atheoretical in nature) is provided in Appendix 9.

IMPACT OF METHODOLOGICAL VARIATIONS

Each empirical study was coded for a number of characteristics related to methodological variations. This was done in order to determine their impact on the effect size estimate for the relationships between macro-level variables and crime rates. In addition to the measurement-oriented variables included within each predictor domain, four main sets of methodological control variables will be assessed: each study’s level of

aggregation, model specification and research design, measurement of the dependent variable, and overall predictive power.⁵⁸

Level of Aggregation

Understanding the impact of the level of aggregation on the effect size of certain macro-level relationships is important for two reasons. First, certain theories were developed with specific units of analysis in mind (e.g., social disorganization theory was developed as a neighborhood-level theory). Therefore, accurately assessing the degree of support for such theories across empirical studies may require comparative analyses between those studies examining the proper unit of analysis from those that do not. Second, other macro-level theories do *not* specify a particular level of aggregation. In such instances, it would be possible to determine the mean effect size of certain key theoretical variables when measured at different units of analysis (e.g., whether routine activities theory is best supported at higher or lower levels of aggregation; see Farnworth, McDermott, and Zimmerman, 1988; Firebaugh, 1978). The codes used to determine the impact of the *level of aggregation* on the effect size estimates across studies include: 0 = neighborhood/block, 1 = census tract, 2 = city, 3 = county, 4 = SMSA, 5 = state, 6 = country, 7 = multi-level model. Studies were also coded as to the *geographic origin* of their sample, with the values being 0 = the United States only, 1 = analysis confined to single Western nation other than the U.S., 2 = analysis confined to a single non-Western nation, and 3 = cross-national comparison.

⁵⁸ As discussed previously, due to their multiple methodological advantages beta-weight values (as effect size estimates) will be used in the analyses examining the potential impacts of methodological variations on the effect size of the empirical relationships.

Model Specification and Research Design

As discussed in Chapter Three, much of the theoretical debate in macro criminology may be traced back to the different methodological approaches taken by researchers. In particular, differences in model specification and research designs may be partially responsible for the lack of theoretical clarity that currently guides ecological research. To evaluate this contention, each study was coded according to its basic methodological approach. These measures included whether variables from *competing criminological theories* (such as those reviewed in Chapter Three) were included in the model (0 = no, 1 = yes for each criminological theory), the type of *statistical method* used in the analysis (0 = OLS regression, 1 = WLS regression, 2 = LISREL estimation or path analysis, 3 = ARIMA or other time series design, 4 = nonlinear model, 5 = stepwise regression); and whether *reciprocal effects* were estimated (0 = yes, 1 = no). In addition, studies were coded according to the *time dimension* of whether a cross-sectional or longitudinal design was employed⁵⁹ (dummy coded to values of 0 and 1, respectively), and whether the full statistical model was *race specific* (0 = not race specific, 1 = analysis of black crime rates only, 2 = analysis of non-white crime rates only).

If the study used a time series design, a control variable indicating the *time lag* specified in the analysis (coded as the number of months) is included. Finally, a control for the *number of independent variables* is coded from each study to assess the degree to

⁵⁹ Pooled cross-sectional time-series (CSTS) designs analyzed through OLS or WLS regression techniques were coded as “cross-sectional” even though they cover an extended time period. This was done since, in such designs, the statistical assumption is that the slopes of the specified relationships do not change from year to year (thus, allowing them to be pooled) (Beck and Katz, 1995). Therefore, unlike formal ARIMA time-series modeling techniques, CSTS models do not, statistically, specify “time” as a predictor of the dependent variable (i.e., no substantive adjustment is made for the stability of relationships across time periods).

which certain predictor variables are, and are not, at risk of being “washed out” by including a high number of controls in a statistical model.

Dependent Variable

Studies also varied according to what was being predicted. These differences may be the product of either theoretical logic and/or theoretical curiosity. For example, Blau and Blau’s (1982) discussion of metropolitan structure/urban inequality had an explicit focus on violent crime. Other theories, such as routine activities theory, specify a relationship between key theoretical independent variables and “predatory” crime rates—including both rates of predatory personal and property victimization. Accordingly, routine activities theory implicitly assumes that opportunity/environmental exposure variables will affect violent and property crime rates in much the same way (i.e., the effect size of routine activities variables should not differ significantly across the two types of dependent variables). To determine the relative effect size of the predictor domain variables on different types of crime rates, a categorical variable was constructed. The codes for the different *dependent variables* across studies included rates of: 1 = overall violent crime, 2 = overall property crime, 3 = robbery, 4 = burglary, 5 = homicide or murder, 6 = rape/forcible sexual assault, 7 = aggravated assault, 8 = all index offenses, 9 = violent delinquency rates, 10 = property delinquency rates, 11 = overall delinquency rates, and 12 = theft/larceny rates (adults only).

In addition, the distribution of crime rates across social aggregates is often skewed. Thus, it is not uncommon for researchers to change the scale of the outcome

variable by taking the log (usually the natural log⁶⁰) of the dependent variable to correct for statistical problems such as heteroscedasticity and autocorrelation⁶¹ (see Blalock, 1972; Hanushek and Jackson, 1977). To control for the potential impacts on the effect size estimates across studies of altering the distribution of the dependent variable, an additional dummy variable was also included as to whether the dependent variable was in its *logged or unlogged* form (0 = unlogged, 1 = logged). Finally, the standard deviation of the dependent variable was coded from each study, which may then be used as a weight in the calculation of the overall mean effect size estimates for each of the predictor domains.

Overall Predictive Power

In addition to these three sets of methodological controls, the r-square values from each study (i.e., the variation in crime rates explained by each full statistical model) were coded to determine their degree of sensitivity to the controls for methodological variations outlined above. In particular, doing so may allow for the discovery of (1) whether those studies that included variables from particular theories of crime (e.g., relative deprivation/inequality theory) could significantly explain more variation crime rates than those without such measures, (2) whether full statistical models are more or

⁶⁰ The natural log (log base e) is assumed to be more stable than the log base 10.

⁶¹ In macro-level research, heteroscedasticity (unequal error variances across fixed categories of the independent variables) often arises when the dependent variable is skewed because of unequal numbers of cases that may fall along combinations of the values of the independent variables. Autocorrelation (a statistical relationship between successive residuals) may occur in aggregate research because both outliers in a skewed distribution, and because of the prospect of non-linearity due to the boundaries often placed on the dependent variable (i.e., the values bound by zero and one).

less robust using a logged dependent variable, or (3) how well full statistical models predict property versus violent crime rates.⁶²

Statistical Analysis Procedures

A dummy variable will be constructed for each effect size estimate reflecting whether it was or was not a statistically significant predictor of crime rates at the $p < .05$ level in its respective statistical model. This is simply an initial step in the meta-analysis process, which is often labeled “vote counting” (see Wolf, 1986). Although statistically unsophisticated, it may serve two purposes. First, it may be a starting point for the higher-order analyses to follow (e.g., actual significance testing), where in a purely descriptive fashion a preliminary estimate could be obtained regarding how often a particular variable is a significant predictor of crime rates (e.g., in what percent of studies is “poverty” a significant predictor of crime rates?). Second, and perhaps more importantly, these descriptive estimates may serve as a basis of comparison for the potentially different interpretations that would be reached when comparing the *percent significant* (i.e., with no mention of magnitude) versus *effect size* of the predictor domains.⁶³

⁶² Of course, not all macro-level studies do, or are able to, report an r-square value. Time series analyses, for example, will contain unusually high r-square values (typically well above .90) simply due to the method of estimation. Accordingly, the analysis of r-square values across studies will be used only as a rough “reliability check” that may corroborate the findings of the more precise and rigorous meta-analysis techniques.

⁶³ As stated in Chapter Two, the supposed advantage of a meta-analysis over the vote-counting method is how meta-analysis takes into account the magnitude of relationships. Coding both the size of the coefficient and whether or not it is statistically significant will allow for an empirical test of this meta-analytic assumption.

Accordingly, difference of means tests will be used to assess the impact of the methodological variations on the effect size estimates for each of the predictor domains. While it would be ideal to conduct multiple regression analyses using the methodological control variables as independent variables, most of the assumptions of the technique would be violated given the small sample sizes within most of the predictor domains (see the discussion by Lipsey, 1992). For those predictor domains that do contain a sufficient number of contributing effect size estimates, however, multiple regression techniques may be appropriate for assessing the impact of methodological variations on the specified relationships. In such instances, OLS regression equations will be estimated.

CHAPTER 5

STRENGTH AND VARIABILITY OF THE EFFECTS OF MACRO-LEVEL PREDICTORS OF CRIME

As stated previously in Chapter One, the overall purpose of the present meta-analysis is to assess the relative strength (predictive power) and stability (sensitivity to varying methodological approaches) of the macro-level predictors of crime. This chapter contains the results of these two sets of analyses, which are presented in two stages. First, the mean effect size estimates, across all macro-level studies, are reported for each macro-level predictor of crime. These estimates are then ranked according to their relative magnitude similar to the method used by Andrews and Bonta (1994) and Loeber and Stouthamer-Loeber (1986).

Uncovering the overall relative effects of macro-level predictors of crime is important so that future studies, by controlling for the “known” predictors of crime rates, may reduce the risk of misspecification error. Nevertheless, it may also be necessary to examine separately the relative effects of macro-level predictors of crime under those methodological conditions assumed to have different causal structures. Accordingly, the second stage of analyses explicitly addresses the variability of effects for the predictor domains according to certain methodological differences. Specifically, these analyses reveal the relative strength and rank-order of the macro-level predictors of crime when specified by the dependent variable (violent versus property crimes), by the level of aggregation used in the study, by the temporal portion of the research design (cross-sectional versus longitudinal analyses), and by whether rates of juvenile delinquency or

adult crimes were being assessed. This chapter then concludes with a tabular summary of the strength and stability of each of the macro-level predictors.

STRENGTH OF EFFECTS

The predictor domains were developed based on certain concepts contained in the macro-level criminological research (discussed in Chapter Three). Each “domain,” however, could be assessed through different operational measures. When these measures had effect sizes that were not significantly different from one another, they were not differentiated in the analysis and tables. Instead, their effects were combined into a single estimate for that predictor domain. In three instances, however, the measures of the predictor domain’s concept did differ significantly (racial heterogeneity, unemployment, and the policing effect). In these cases, the effects of each variable are reported separately.

For example, the effect size of the *predictor domain* “racial heterogeneity” significantly differed according to whether a measure of percent non-white, percent black, or a racial heterogeneity index was used across empirical studies (see Table 5.2). Accordingly, separate mean effect size estimates for each of these three *macro-level predictors* were calculated and then placed in the ranking in Table 5.1 (similar disaggregated estimates were calculated for the unemployment and policing effect predictor domains). Other predictor domains, however, did not exhibit such sensitivity to measurement differences. For example, the predictor domain “socioeconomic status” contained measures based on the mean household/per capita income, the median household/per capita income, and SES index measures (see Chapter 4). Difference of

means tests revealed that these measures did not significantly differ from one another,⁶⁴ and a chi-square test did not reveal significant heterogeneity across the effect size estimates. Consistent with common practice in meta-analytic research (Bonta et al., 1998; Gendreau et al., 1998; Loeber and Stouthamer-Loeber, 1986; Pratt and Cullen, 2000), the effect size estimates generated by these three proxies for socioeconomic status were therefore grouped together and treated as a single predictor domain.

All of the chi-square tests for heterogeneity were conducted on metric coefficients first, since they are not assumed to vary across samples. Since beta weights can vary across samples, tests of heterogeneity may be biased to either inflate or deflate the chi-square value due to variations in standard deviations from study to study when the operational definitions of the same concept differ. Thus, conducting the heterogeneity tests on the metric coefficients maximizes the likelihood of accuracy in their interpretation. Assuming no significant heterogeneity exists across the metric coefficients, the same tests were conducted on the aggregated predictor domains using the beta weights.

To clarify what the chi-square test for heterogeneity does, and does not, tell the meta-analyst, a brief note on its history may be helpful. This test was developed by meta-analysts who typically dealt with small samples of studies for their quantitative syntheses (often fewer than twenty in fields such as psychology and medicine). Given the instability of mean estimates generated by small samples, the chi-square test for heterogeneity was developed as a method of testing for outliers that could bias a mean

⁶⁴ Depending on the number of categories representing the measurement differences within each of the predictor domains, either two-sample t-tests or one-way analysis of variance (ANOVA) were used to test for significant differences across measurement techniques.

effect size estimate (upward or downward) under such conditions. With the exception of only a few of the predictor domains in the present study, the potential threat associated with the presence of outliers is substantially reduced due to the unusually large sample of studies being synthesized. Only five of the twenty-three predictor domains contain a sample of fewer than thirty contributing effect size estimates, and many contain well over one hundred effect size estimates. Thus, the stability of these mean effect size estimates, and their corresponding non-significant heterogeneity tests noted throughout this chapter, is not surprising.

Again, along with racial heterogeneity, the only predictor domains that contained statistically significant measurement differences among its constituent predictors were unemployment and the policing effect. Thus, separate mean effect size estimates for the macro-level predictors within the unemployment and policing effect predictor domains were calculated and placed into the rankings presented in Table 1 and Table 2. Since none of the remaining twenty predictor domains contained statistically significant within-domain measurement differences, however, they were not disaggregated into their constituent macro-level predictors for the rank ordering. Now that the conceptual difference between macro-level predictors and predictor domains has been established, the results of meta-analysis contained in Table 5.1 and Table 5.2 can be discussed.

Rank Order of Macro-Level Predictors of Crime

Table 5.1 displays the rank order of the mean effect size estimates from the thirty-one different macro-level predictors of crime across all 214 empirical studies, 509 statistical models, and 1,984 effect size estimates. The rank ordering of these estimates is

based on the relative magnitude of the independence-adjusted mean effect size estimates contained in Table 5.2.

The top five predictors of crime are the strength of noneconomic institutions, unemployment (when the length of unemployment is considered), firearms ownership, the percent non-white, and the effect of incarceration. Each of these predictors has a mean effect size above .30, which according to the general consensus in meta-analytic research is substantial (Rosenthal, 1984; see also Andrews et al., 1990; Wolf, 1986). Thus, macro-level studies of crime that fail to control for the effects of these variables may be at risk for the biases associated with model misspecification error.

It should be noted that these rankings are based on the absolute value (magnitude) of the mean effect size estimates assuming they are specified in the proper direction. For example, the effects of predictors such as the strength of noneconomic institutions, incarceration, collective efficacy, and social support/altruism are all inverse, but are theoretically expected to be so. Certain policing effect predictors, however, are also expected to be inversely related to crime but have positive mean effect size estimates (police size and police per capita). Accordingly, these macro-level predictors were ranked downward because their mean effect size estimates, while potentially greater in absolute value (i.e., distance from zero) than certain other predictors, were in the opposite direction than theoretically predicted.

The other predictors ranked in the upper third of the distribution include collective efficacy⁶⁵ (which also has a mean effect size above .30), the percent black, the religion

⁶⁵ Neighborhood-level measures of “social interaction” (Bellair, 1997), “neighborhood integration” (Patterson, 1991), and “local social ties” (Warner and Roundtree, 1997) were included within the collective efficacy predictor domain. A chi-square test revealed that combining these effect size estimates did not result in statistically significant heterogeneity.

effect, family disruption, and poverty. Although not in the top third of the rank ordering, the remaining predictors with a mean effect size above .20 include unsupervised local peer groups, the household activity ratio, social support/altruism, and inequality.

The macro-level predictors ranked from number fifteen to number twenty-three may be interpreted as mid-range predictors of crime, with mean effect size estimates under .20 but above .10. Included within this set of predictors are the effects of a racial heterogeneity index, urbanism, residential mobility, unemployment (with no age restriction), age effects, the southern effect, unemployment (when the length of unemployment is not considered), socioeconomic status, and the arrest ratio. While certainly not as robust as the effects of those predictors ranked higher in the distribution, these mid-range macro-level predictors of crime are likely to make a significant contribution to the proportion of explained variation in a statistical model (i.e., their effects, while not large, may not be negligible).

The macro-level predictors ranked from twenty-four through thirty-one represent the relatively weak predictors of crime, with mean effect size estimates under .10. Mean effect size estimates this small are generally considered by meta-analysts as being substantively unimportant (e.g., see the discussion by Andrews and Bonta, 1994). Included within this list are the effects of unemployment (with no age restriction), the sex ratio, structural density, police expenditures, get tough policies, education effects, police per capita, and police size. Although it may be necessary to control for one or more of these macro-level predictors of crime in a particular empirical study for theoretical reasons (e.g., testing competing criminological theories), the omission of controls for

these variables in a statistical model is less likely to result in biases associated with model misspecification error.

*Effect Size Estimates from Sociological, Socio-Economic,
and Criminal Justice System-Related Sources*

Given the rank ordering of the mean effect size estimates from the macro-level predictors of crime contained in Table 5.1, it may be useful to “take a step back” and examine the broader sources of these predictors. In particular, are the sources of the relatively strong predictors of sociological, socio-economic,⁶⁶ or criminal justice system-related? The same question could be asked for the mid-range and weaker predictors of crime as well. In other words, does there appear to be a pattern regarding these various sources of macro-level predictors of crime as to where they fall in the rank-ordered distribution of effect sizes?

Top Tier Predictors. Among the fourteen macro-level predictors of crime considered to be in the top tier (having substantively meaningful effect sizes of .20 or above), most (ten) have sociological roots: the strength of noneconomic institutions, firearms ownership, percent non-white, collective efficacy, percent black, the religion effect, family disruption, unsupervised local peer groups, the household activity ratio, and social support/altruism. Another three of the top-tier predictors may be interpreted as more socio-economic in nature, including unemployment (when the length of

⁶⁶ This is not to say that certain socio-economic variables are not also sociological. For example, a predictor such as inequality—which contains an economic component—is certainly an important sociological variable. Despite this conceptual overlap, to the extent that a predictor has an economic base and is therefore hypothesized to affect social experiences, it is classified as being socio-economic in nature.

unemployment is considered), poverty, and inequality. The only criminal justice system-related predictor to find its way into the top tier for mean effect sizes was the incarceration effect.

Mid-Range Predictors. Sociological and socio-economic sources are also most common among the nine macro-level predictors that are mid-range in their mean effect sizes (between .20 and .10). The five sociological predictors include a racial heterogeneity index, urbanism, residential mobility, age effects, and the southern effect. The three predictors with socio-economic sources include unemployment (with an age restriction), unemployment (when the length of unemployment is not considered), and socioeconomic status. The only criminal justice system-related predictor in this tier is the arrest ratio.

Bottom Tier Predictors. Although macro-level predictors from sociological and socio-economic sources dominated both the top tier and mid-range categories of effect sizes, an even more discernable pattern emerges when the bottom tier predictors are examined (mean effect size below .10). Socio-economic predictors (unemployment with no age restriction) and sociological predictors (sex ratio, structural density, and education effects) still appear in this category. Nevertheless, the bottom tier of mean effect size estimates is dominated by predictors related to the criminal justice system. In particular, the macro-level predictors of police expenditures, get tough policies, police per capita, and police size are all located in this bottom tier of relative effect size. Thus, the ability of macro-level proxies of criminal justice system dynamics to predict crime rates—relative to other sociological and socio-economic factors—is fairly limited.

Statistical Diagnostics and the Predictor Domains

An understanding of the relative effects of the macro-level predictors of crime is important in its own right. Even so, it is also necessary to examine how each of the predictor domains outlined in Chapter Four is affected by the statistical adjustments (discussed in Chapter Two) to correct for potential biases in estimation. To explore these issues, Table 5.2 contains the overall mean effect size estimates for each predictor domain, the mean effect size estimates weighted for sample size, and the mean effect size estimates adjusted for the potential lack of statistical independence.

While this table contains a considerable amount of information, three issues need to be highlighted. First, all of the independence-adjusted mean effect size estimates (ADJz) fall within the original confidence intervals generated by the unadjusted effect size estimates (Mz).⁶⁷ Thus, while the correction for the lack of statistical independence is certainly theoretically appropriate given the discussion in Chapter Two, the overall mean effect size estimates were not significantly biased due to the potential lack of independence.

Second, the overwhelming majority of the mean effect size estimates, based on their corresponding confidence intervals, were significantly different from zero (i.e., statistically significant at $p < .05$). For these estimates, their magnitudes can and should be taken at face value (i.e., their non-zero mean effect sizes are not due to sampling error). Only the mean effect size estimates from four macro-level predictors were not

⁶⁷ The original confidence intervals were used for these tests because, if biases due to the lack of statistical independence were present, the likely effect would be to bias the standard errors for the predictor domains downward. If so, the confidence intervals would be more narrow, and therefore the test for differences

statistically significant, including police size, police per capita, police expenditures,⁶⁸ and education effects. For these macro-level predictors, their mean effect size estimates (while already relatively small in magnitude) should be viewed as non-zero random fluctuations from a null effect.

The emphasis placed on statistical significance in this discussion may, admittedly, be an unpopular position to take according to certain meta-analytic researchers (e.g., see the discussions by Rosenthal, 1984; Wolf, 1986; and the work of Andrews, Bonta, Gendreau, and colleagues). Specifically, since significance testing is largely contingent upon sample size (Blalock, 1972; Hanushek and Jackson, 1977), certain “effect sizes” might be substantial yet statistically insignificant if obtained from a small sample. Nevertheless, the present sample of 214 studies, 509 statistical models, and 1,984 effect size estimates is by no means small, and therefore it is reasonable to assume that significance testing, while perhaps not the norm in disciplines that typically deal in the realm of limited clinical samples, is warranted in this case. Even so, those who are not satisfied with the significance testing approach taken in these analyses may look to the mean effect size estimates weighted for sample size contained in Table 5.2, which account for the potentially differential effects across studies due to their different sample sizes.

Third, Fail-Safe N estimates were calculated for each predictor domain. As discussed in Chapter Two, the Fail-Safe N is a statistical estimate for the number of

between the overall mean effect size estimates and those adjusted for the lack of independence would be more conservative.

⁶⁸ While these macro-level predictors falling under the “policing effect” predictor domain were not statistically significant, the overall effect for the policing domain ($ADJz = -.054$) is statistically significant.

studies that would have to emerge to reduce a predictor's mean effect size so that is not significantly different from zero. Due to the large sample in the present analysis, most of the Fail-Safe N estimates were extremely large (consistently well over 100, even among some of the weaker predictors).⁶⁹ To make the presentation of these estimates more meaningful, Table 5.2 indicates which mean effect size estimates may actually be at risk for having their effects significantly diminished with the publication of a reasonable amount of studies finding “no effect.” To this end, a predictor domain—or predictor within a domain—was singled out if its Fail-Safe N was less than twenty. In so doing, it becomes apparent that a number of top tier macro-level predictors of crime—the ranking for which was based on relative magnitude only—are below the Fail-Safe N threshold of twenty. In particular, the macro-level predictors of the strength of noneconomic institutions, unemployment (when the length of unemployment is considered), firearms ownership, collective efficacy, the religion effect, and unsupervised local peer groups all have Fail-Safe Ns under twenty.

Thus, the mean effect size estimates for the top three macro-level predictors of crime, and five of the top ten predictors, must be viewed with caution. This is not to say that the low Fail-Safe Ns for these predictors renders them substantively unimportant. Rather, despite their relatively large effect sizes, their mean effect size estimates were generated by a small number of empirical studies and corresponding statistical models. The number of contributing effect size estimates for this group of predictors ranges between only four (the strength of noneconomic institutions and firearms ownership) and

⁶⁹ As with the chi-square test for heterogeneity in a meta-analysis, the Fail-Safe N was developed in a context where meta-analysts typically did not deal with samples as large as the one used in this study. It is therefore not surprising that the Fail-Safe N estimates are so high in the present case because of the high numbers of contributing effect size estimates per predictor domain (see Table 5.2).

thirteen (collective efficacy and unemployment when the length of unemployment is considered). In short, only after more empirical studies are conducted and published will it be determined how well the effects of these macro-level predictors “hold up” across multiple tests.

VARIABILITY OF EFFECTS

The second objective of the present meta-analysis was to assess the degree to which the mean effect size estimates for the macro-level predictors of crime are conditioned by certain methodological factors (a similar approach was taken in the examination of the variability of treatment effects by Lipsey, 1992; see also Pratt and Cullen, 2000). This objective is rooted in the assumption that a full understanding of the relative empirical validity of the macro-level predictors of crime requires not only a comparison of their relative strength, but also a comparison of how the magnitude of their effects may be either more or less robust under varying methodological conditions. To address this issue, the mean effect size estimates for each of the macro-level predictors were calculated and ranked when specified by the dependent variable (when predicting violent versus property crimes), by the level of aggregation (from the block/census tract level up to the national level of analysis), by the type of research design (cross-sectional versus longitudinal), and by whether rates of juvenile delinquency versus adult crimes were being examined. The results of these analyses are presented in Table 5.3 through Table 5.6.

Dependent Variable Specification

Table 5.3 contains the overall rank ordering of the mean effect size estimates for each of the macro-level predictors of crime—as a baseline for comparison (the mean ADJz gathered from Table 5.2)—and the rank ordering of the same effect size estimates when specified by the dependent variable (violent versus property crimes). The overall trend in this specification is one of consistency. The rank-order correlation is .822 ($p < .001$),⁷⁰ meaning that few predictors make a dramatic shift in ranking (i.e., from one “tier” of strength to another) when predicting violent versus property crimes. Only two predictors made a substantial change in ranking within this specification. First, the percent non-white dropped from a ranking of fourth to fifteenth when moving from a predictor of violent to property crime. Second, residential mobility increased its ranking from twenty-fourth for violent crimes to tenth for property crime.

The mean effect size estimates⁷¹ for the top five predictors of violent crimes are unemployment (when the length of unemployment is considered) at .687, the incarceration effect at -.421, the strength of noneconomic institutions at -.407, percent non-white at .374, and firearms ownership at .346.⁷² The mean effect size estimates for the top five predictors of property crimes are the strength of noneconomic institutions at

⁷⁰ The rank-order correlation represents the multiple correlation (multiple R) among the rank ordered values for each macro-level predictor.

⁷¹ Across each of the specifications discussed in this chapter, the mean effect size estimates refer to the ADJz estimates.

⁷² As with the overall mean effect size estimates, it is important to remember the potential instability of the mean effect size estimates calculated with small Fail-Safe Ns for certain macro-level predictors (many of which are in the top tier), which may be heightened when broken down by the specifications within this section of the chapter because the numbers of contributing effect size estimates may decrease accordingly.

-.386, the religion effect at -.365, the incarceration effect at -.326, unemployment (when the length of unemployment is considered) at .285, and collective efficacy at -.284.

Level of Aggregation Specification

Table 5.4 contains the rank ordering of the effect size estimates for the macro-level predictors of crime when specified by the level of aggregation. The categories for this specification move from the smaller to the larger units of analysis: block/census tracts, cities/SMSAs, states, and nations. As with the dependent variable specification, the overall trend in the rank ordering across the levels of aggregation is one of consistency. The rank-order correlation is .896 ($p < .01$).⁷³ Nevertheless, despite the apparent stability of the rank ordering within this specification, two factors warrant discussion.

First, a number of macro-level predictors of crime have not been tested across each of these levels of aggregation. This may be due to theoretical reasons, where certain predictors may have only appeared in tests of theories that explicitly target a particular unit of analysis. Another reason may be that certain macro-level predictors have only recently been included in empirical studies, and therefore have yet to be subjected to the same level of empirical scrutiny as the more well established predictors of crime. In any event, as can be seen in Table 5.4, many macro-level predictors of crime “drop out” of the ranking for one or more categories in this specification.

⁷³ Although the rank-order correlation for this specification is larger in magnitude than that for the previous specification, its probability level is not as low. This is because of certain mean effect size estimates drop out of particular categories within the specification. Since the overall rank-ordered correlation can only be calculated with listwise deletion (where cases with missing values are removed from the calculation), the degrees of freedom—and corresponding statistical power—are reduced.

Second, while most of the macro-level predictors do not make many dramatic changes in ranking across this specification, certain predictors do seem to fair either better or worse at particular levels of aggregation. For example, the mean effect size of unemployment (when the length of unemployment is considered) is much higher at the national level than at the city/SMSA level (mean $ADJ_z = .531$ and $.167$, ranked number one and ten, respectively). Also, two indicators of racial heterogeneity, the percent non-white and the percent black, drop from a respective ranking of first and second at the city/SMSA level to a ranking of fifteenth and fourteenth at the national level. In addition, the household activity ratio jumps from a ranking of fourteenth at the city/SMSA level to second at the national level. The effect of residential mobility also demonstrates considerable sensitivity in this specification, moving from a ranking of nineteenth at the block/census tract level to a ranking of first at the state level. The southern effect, however, responds to this specification in the opposite manner, going from the top rank at the block/census tract level to being ranked twenty-fourth at the national level.

Given both the stability and instability of the effect sizes within this specification, the mean effect size estimates for the top five macro-level predictors of crime at the block/census tract level include the southern effect at $.351$, poverty at $.347$, the percent non-white at $.316$, collective efficacy at $-.303$, and unemployment (when the length of unemployment is considered) at $.300$. The mean effect size estimates for the top five predictors at the city/SMSA level are the percent non-white at $.329$, the percent black at $.317$, family disruption at $.282$, the racial heterogeneity index at $.273$, and firearms ownership at $.249$. The mean effect size estimates for the top five predictors at the state level are residential mobility at $.477$, the religion effect at $-.421$, poverty at $.406$,

urbanism at .398, and the strength of noneconomic institutions at -.386. Finally, the mean effect size estimates for the top five predictors at the national level are unemployment (when the length of unemployment is considered) at .531, the household activity ratio at .487, firearms ownership at .443, inequality at .417, and the strength of noneconomic institutions at -.407.

Research Design Specification

Table 5.5 contains the rank ordering of the effect size estimates for the macro-level predictors of crime when specified by the research design. The categories for this specification include studies that employed cross-sectional versus longitudinal research designs. Unlike the two previous specifications, the rank-order correlation is not as high at -.417 (it is, however, statistically significant) and, as noted, it is *negative*. Thus, as the rank ordering of the mean effect size estimates moves from cross-sectional to longitudinal research designs, a number of predictors make dramatic shifts in ranking.

In particular, nine macro-level predictors make enough of a change across the categories in this specification to essentially “drive” the negative rank-order correlation. Moving from the cross-sectional to the longitudinal rankings, these predictors include: unemployment (when the length of unemployment is considered, which moves from a rank of twelfth to second), family disruption (dropping from seventh to fourteenth), poverty (which drops from ninth to eighteenth), the household activity ratio (which jumps from twenty-fourth to first), social support/altruism (which increases from eighteenth to seventh), residential mobility (moving from seventeenth to fifth), age effects (rising from

twenty-third to tenth), the sex ratio (from twenty-seventh to eleventh), and finally police expenditures (increasing from twenty-eighth to sixth).

Given the shifts in the rank ordering across the categories within this specification, the mean effect size estimates for the top five macro-level predictors of crime in studies with cross-sectional research designs are the strength of noneconomic institutions at $-.391$, firearms ownership at $.346$, the percent non-white at $.306$, collective efficacy at $-.303$, and the percent black at $.299$. The mean effect size estimates for the top five predictors of crime in studies with longitudinal research designs are the household activity ratio at $.629$, unemployment (when the length of unemployment is considered) at $.496$, the incarceration effect at $-.469$, the percent non-white at $.447$, and residential mobility at $.442$.

Juvenile Delinquency Versus Adult Crime Specification

Table 5.6 contains the rank ordering of the effect size estimates for the macro-level predictors of crime when specified by whether rates of juvenile delinquency versus adult crime were examined across studies. Unlike the previous specifications, the rank-order correlation ($.034$) is not statistically significant. This is most likely due to a problem shared with the level of aggregation specification (although amplified in this case): a number of macro-level predictors of crime have not been tested within the juvenile delinquency category. Also consistent with the level of aggregation specification, the absence of many macro-level predictors in this specification may be due to theoretical reasons, where certain predictors may have only appeared in tests of theories that explicitly target rates of juvenile offending (e.g., social disorganization

theory). In any event, as can be seen in Table 5.6, many macro-level predictors of crime “drop out” of the ranking in the juvenile delinquency category in this specification.

Nevertheless, the mean effect size estimates for the top five macro-level predictors of juvenile delinquency are poverty at .557, education effects at -.553, the percent non-white at .457, unemployment (when the length of unemployment is considered) at .341, and residential mobility at .299. The mean effect size estimates for the top five macro-level predictors of adult crime are the strength of noneconomic institutions at -.391, unemployment (when the length of unemployment is considered) at .387, firearms ownership at .346, the incarceration effect at -.322, and the percent non-white at .305.

STRENGTH AND STABILITY OF EFFECTS: A SUMMARY

Table 5.7 contains a tabular summary of the relative strength and stability of effects for each of the macro-level predictors of crime across the various methodological specifications. Categories representing high, moderate, and low were constructed for both the strength and stability summaries. The strength designations were based on the calculation of an overall pooled mean effect size estimate and pooled standard error for all macro-level predictors. A macro-level predictor was classified as “high” or “low” on strength when its mean effect size estimates were consistently two standard errors above or below the pooled mean across the various methodological specifications (with the “moderate” category in between).⁷⁴

⁷⁴ The pooled mean was .178 and the pooled standard error was .023.

The stability designations were based on two factors. First, an average change score was calculated for each macro-level predictor according to its rank order across the various specifications. These averages were then divided into thirds of the overall distribution, where the third with the lowest average change in ranking was given a designation of “high” stability, the next third receiving the “moderate” stability score, and the final third (i.e., with the highest average change scores) given the “low” stability label. Second, changes in stability ranking were made for the mean effect size estimates with a Fail-Safe N under twenty. Again, such small Fail-Safe N estimates indicate that a reasonable amount of studies indicating a “null effect” could reduce the mean effect size of these macro-level predictors to non-significance (i.e., non statistically different from zero). Thus, due to their small Fail-Safe Ns, the strength of noneconomic institutions, unemployment (when the length of unemployment is considered), firearms ownership, collective efficacy, the religion effect, and unsupervised local peer groups were all designated as “low” on stability.

Given these criteria and corresponding designations, Table 5.7 indicates that there are five macro-level predictors of crime that are found to be “high” on both strength and stability. These predictors include the percent non-white, the incarceration effect, the percent black, family disruption, and poverty. Therefore, researchers conducting macro-level studies of crime in the future may wish to consider, at minimum, controlling for the effects of these predictors since they tend to be generally robust across different methodological specifications. Studies that fail to consider the effects of at least one or more of these predictors in their statistical models run a substantial risk of being misspecified.

CHAPTER 6

THE EMPIRICAL STATUS OF THE MAJOR MACRO-LEVEL THEORIES OF CRIME

This meta-analysis is intended to serve two purposes. The first purpose, as discussed in Chapter five, is to establish the relative strength and stability of the macro-level predictors of crime. Despite the importance of these findings for future research concerning what we currently do, and do not, “know” about the relative effects of these variables, such knowledge is essentially “atheoretical.” Thus, the second purpose of the present meta-analysis is to provide as much insight as possible regarding *the empirical status of the major macro-level theories of crime*. This chapter deals specifically with this issue.

To accomplish this objective, the macro-level criminological theories discussed in Chapter Three are evaluated in this chapter in terms of the strength and stability of the key variables specified by each theory. Furthermore, assuming that these predictors have been adequately tested,⁷⁵ two additional multivariate analyses are conducted on the effect size estimates from the theory’s key variables (a similar meta-analytic approach was taken by Tittle, Villemez, and Smith, 1978; see also Pratt and Maahs, 1999). The first is a “general” regression model, where the effect size estimates from a particular predictor domain are regressed on a uniform set of six methodological characteristics (for a detailed discussion of these factors, see Chapter Four). The first of these characteristics

⁷⁵ Most of the predictor domains specified by the various macro-level criminological theories contained a substantial number of contributing effect size estimates (often more than 100). To ensure the stability of the parameter estimates for the regression models in this chapter, only those predictor domains with at least

is a seven-category ordinal scale representing the *level of aggregation* used in the study. Based on the codes contained in Chapter Four, the values move from the smallest unit (neighborhood/block) to the largest unit (nation) of analysis.⁷⁶

The second general methodological factor is a dummy variable for whether the sample was from the *U.S. in origin* only (0 = no, 1 = yes). This was included to test for whether the variables specified by a particular theory are relevant to crime in the United States only or to other geographic settings as well. The third general methodological characteristic is a dummy variable for whether *variables from competing theories* were controlled in the analysis (0 = no, 1 = yes).⁷⁷ Fourth, whether the research design was *cross-sectional versus longitudinal* (coded as 0 and 1, respectively) was included within the general regression models. To control for the potential instability in parameter estimates generated by small samples across empirical studies, the fifth general control variable is a *ratio of the sample size to the number of independent variables* in the statistical model. The final independent variable for the general regression models is whether the dependent variable was comprised of *violent crimes* (0 = no, 1 = yes). Three of these factors (the level of aggregation, the time dimension, and the violent crime dependent variable) overlap with those examined and discussed in Chapter Five, yet they

40 effect size estimates were subjected to these additional analyses (the only exception being the household activity ratio with 37).

⁷⁶ The assumption of linearity in least-squares regression estimation may be threatened with the inclusion of the ordinal variable representing the level of aggregation (see the discussion by Hanushek and Jackson, 1977). Nevertheless, the truncated number of categories for this variable (seven)—in the absence of extreme skewness (which is not a problem in the present case)—is likely to result in more conservative parameter estimates and corresponding significance tests due to its lack of variation (i.e., in smaller parameter estimates relative to their corresponding standard errors). Thus, if the lack of linearity does bias the parameter estimates in the general and theory-specific regression models, it is likely to bias them in a conservative direction.

⁷⁷ Dummy variables for each macro-level criminological theory were also constructed to determine the potentially mediating effects of particular criminological theories on certain effect size estimates.

are included here in a multivariate (as opposed to bivariate) context which may better isolate their influences on the mean effect size estimates.

Following the “general” regression models, a series of “theory-specific” multivariate models are estimated. In these models, certain methodological issues unique to each particular criminological theory are explored. Therefore, the conclusions regarding the empirical status of each of the major macro-level criminological theories contained in this chapter are presented in two stages. The first, as discussed previously, is a review of the strength and stability of the mean effect size estimates for the predictors specified by each theory (both the results of the analyses presented in Chapter Five and the additional analyses presented in this chapter). Based on these analyses, the second stage essentially presents a “bottom line” assessment of the theory (e.g., has the theory been adequately tested? If so, does it appear to be well supported? Are there any salient methodological conditions under which support for the theory is more or less likely to be revealed?). In the end, based on the analyses presented in Chapter Five and those contained in this chapter, the present meta-analysis should provide for a more firm understanding of the empirical status of the major macro-level criminological theories than has been revealed by any other review—narrative or quantitative—to date.

SOCIAL DISORGANIZATION THEORY

Key Theoretical Variables: Strength and Stability

Table 6.1 contains a summary of the effect size estimates for the macro-level predictors of crime specified by social disorganization theory. With the exception of the effects of socioeconomic status and residential mobility, the mean effect size estimates

for the other four social disorganization predictors are quite substantial (above .20) and are consistently in the top ten in ranking across the methodological specifications. Furthermore, three of the five macro-level predictors of crime noted in Chapter Five that scored “high” on both strength and stability (see Table 5.7) may be viewed as social disorganization predictors: the percent non-white, the percent black, and family disruption. Even so, the social disorganization predictor with the strongest mean effect size—collective efficacy (at -.315)—has a Fail-Safe N estimate that is less than twenty. Thus, while promising, the predictive stability of this variable across multiple empirical tests is still unknown since it has just recently appeared on the criminological scene.

The Impact of Methodology: The General Models. Table 6.2 and Table 6.3 contain the general regression models (Model 1 for each of the four⁷⁸ predictors) of the impact of certain methodological characteristics on the mean effect size estimates specified by social disorganization theory. Each of the general and theory-specific regression models presented in this chapter are estimated using weighted least-squares regression techniques (see Hanushek and Jackson, 1977) to accommodate the fixed-effects correction for independence. As can be seen in Table 6.1, the effect size estimates for racial heterogeneity are significantly stronger in studies conducted in the U.S. only ($b = .264$),⁷⁹ and in those predicting rates of violent crime ($b = .070$). The effect is

⁷⁸ Due to their small numbers of contributing effect size estimates, the collective efficacy and unsupervised local peer groups predictor domains were not subjected to these additional multivariate analyses.

⁷⁹ The b coefficients discussed in this chapter refer to the metric coefficients from the multiple regression models. Thus, the magnitude of these coefficients may be taken at face value. In other words, the metric coefficient for racial heterogeneity indicates that its effect size increases by .264 when moving from studies not conducted in the U.S. to those that were conducted in the U.S. only. Furthermore, all of the regression models presented in this chapter contain all of the necessary components for estimating various predicted values of each of the dependent variables. For example, using any of the regression models found in this chapter, the form of the equation is as follows:

$$\square \text{ (effect size estimate)} = b_0 + b_1 (X_1 + \dots + X_v) + U$$

significantly weaker, however, when variables from competing theories are controlled ($b = -.113$) and when the ratio of the sample size to the number of independent variables is high ($b = -.063a$).⁸⁰

The effects of socioeconomic status appear to be a bit more stable. Only the level of aggregation significantly conditioned the effect size of this predictor, where the inverse coefficient ($b = -.032$) indicates that the effect of socioeconomic status on crime is stronger at smaller units of analysis, which is actually more consistent with neighborhood-level focus of social disorganization theory. The opposite is true, however, for the effects of residential mobility. Table 6.3 indicates that the effect of residential mobility on crime is significantly higher at larger levels of aggregation ($b = .094$) and in longitudinal analyses ($b = .339$); and, unlike racial heterogeneity, the effect size of residential mobility is significantly weaker when predicting rates of violent crime ($b = -.204$). Finally, the effect of family disruption is significantly larger in U.S. samples ($b = .155$), and is significantly weaker ($b = -.081$) when the ratio of sample size to the number of independent variables is high.

The Impact of Methodology: The Theory-Specific Models. Table 6.2 and Table 6.3 also contain the results of the theory-specific regression models of the impact of certain methodological characteristics on the mean effect size estimates specified by

In this equation, b_0 is the estimated constant, and b_1 is the overall slope estimate for the set of independent variables X_1 through X_v (with U as the error term assumed to equal zero). By placing variable values into each regression equation, estimated values of the effect size of the dependent variable could be calculated. Should the reader wish to isolate the differences in estimated values across the categories of a single independent variable (i.e., when “all else is equal”), the mean values for the independent variables for each of the regression equations presented in this chapter can be found in Appendix 11.

⁸⁰ The effect of the ratio of sample size to the number of independent variables may be treated as a proxy for the degree to which the assumptions of multivariate statistical modeling have been met. When the value of this ratio is low, the likelihood that enough cases exist to provide for stable parameter estimation is low as well. Accordingly, if this ratio is high, the likelihood of stable parameter estimation is high.

social disorganization theory (Model 2 for each predictor). Based on the discussion of social disorganization theory contained in Chapter Three, two additional methodological factors were entered into the regression equations. First, a variable indicating whether the study's statistical model was race-specific (0 = no, 1 = black or non-white only) was included under the assumption that the experiences of certain racial/ethnic groups may differ to the extent that the variables related to race-specific crimes may differ from rates of white offending (e.g., see the discussion by Sampson and Wilson, 1995). The second theory-specific factor was a dummy variable for whether reciprocal effects were estimated (0 = estimated, 1 = not estimated), since high crime rates in an area may influence the structural characteristics of that area over time (Bursik, 1986a). As can be seen in Table 6.1 and in Table 6.2, neither of these additional methodological factors significantly influenced the overall effect size estimates for any of the predictors specified by social disorganization theory.

The Empirical Status of Social Disorganization Theory

Based on the results of the meta-analysis contained in Chapter Five and the additional analyses reported in this chapter, the level of empirical support for social disorganization theory is somewhat mixed. Although the theory has been subjected to numerous empirical tests (see Appendix 1), there is considerable variation in the variables used to proxy key concepts, and there is little consistency across studies in the control variables included to isolate their effects. For example, it may be reasonable to argue that other indicators of economic deprivation (e.g., poverty and/or inequality) are actually the precursors to social disorganization (as opposed to socioeconomic status

measures). If so, the level of support for social disorganization theory may differ once again.

This problem is not, however, applicable to social disorganization theory only. Indeed, as stated in Chapter Three, a similar argument can be made for a number of macro-level predictors of crime. For example, debates still continue concerning whether variables such as poverty and inequality should be considered conceptually distinct or as two proxies of general economic deprivation; or whether unemployment should be treated as a routine activities variable or as a conflict theory predictor. Nevertheless, this is, virtually by definition, a conceptual debate that the technique of meta-analysis cannot settle (i.e., which theory may claim ownership over a particular variable). What the present meta-analysis can do, however, is provide precise estimates of the effects of these various predictors so that the theoretical debate concerning which predictors fall under which theories may continue in a more informed manner. Thus, to help inform what is essentially a *theoretical* debate, the way the macro-level predictors of crime are grouped under particular theoretical traditions in this chapter is based on the *theoretical* discussion contained in Chapter Three.

Putting these theoretical debates aside, one macro-level predictor of crime that is unique to social disorganization theory is collective efficacy. Despite the low Fail-Safe N estimate for this predictor (due to its recent articulation and corresponding small number of contributing effect size estimates), its large mean effect size ($b = -.315$) indicates that its empirical future may be promising. Given the recent developments in, and reformulations of, social disorganization theory in recent years (e.g., see the work of Sampson and Groves, 1989; Sampson et al., 1997, 1999), uncovering the effects of

collective efficacy on crime may prove to solidify the empirical status of the social disorganization perspective in studies to come.

ANOMIE/STRAIN THEORY

Key Theoretical Variables: Strength and Stability

Despite being similar in “criminological age” to social disorganization theory, anomie/strain theory has not been rigorously tested empirically (see Appendix 2). Only after its re-conceptualization as “institutional anomie theory” (Messner and Rosenfeld, 1994, 1997b) did explicit macro-level tests of the theory begin to emerge. It is therefore not surprising that the theory’s key theoretical variable—the strength of noneconomic institutions—has yet to experience the same level of empirical scrutiny as most of the other macro-level predictors of crime in the present meta-analysis. Nevertheless, Table 6.4 indicates that the mean effect size of the strength of noneconomic institutions across these tests is $-.391$. Given the rather large magnitude of this predictor, it has an overall ranking of first and an average ranking of 2.43 across the methodological specifications noted in Chapter Five. A note of caution is warranted, however, since the stability of the mean effect size of this predictor is low (with a Fail-Safe N of less than twenty).

The Empirical Status of Anomie/Strain Theory

Consistent with the discussion in Chapter Three and relative to other macro-level criminological theories, anomie/strain theory has not been adequately tested to confirm its empirical status. The large mean effect size for its key theoretical variable indicates a certain measure of promise for the theory, yet empirical tests of the effects of this concept

on crime are sparse. Consequently, the degree to which the theory will be supported under different methodological conditions (e.g., different levels of aggregation and model specifications) is still unknown. Perhaps after additional empirical tests of anomie/strain theory are conducted and published, a re-assessment of the literature may reveal a more concrete picture of this theory's empirical status.

ABSOLUTE DEPRIVATION/CONFLICT THEORY

Key Theoretical Variables: Strength and Stability

Table 6.5 contains a summary of the effects for the key theoretical variable in absolute deprivation/conflict theory: poverty. As stated previously in Chapter Three, measures of absolute and relative deprivation are often viewed as conceptually similar in the macro-level criminological literature (see, e.g., Bailey, 1984, 1999; Crutchfield, 1989; Messner, 1982; Messner and South, 1986; Patterson, 1991; Peterson and Bailey, 1988; Williams and Flewelling, 1988). Indeed, measures of poverty and inequality both tap into a dimension of economic deprivation. Even so, consistent with the separate theoretical discussions devoted to these traditions in Chapter Three, they can also be viewed as conceptually distinct (see, e.g., Austin, 1987; Bennett, 1991; Blau and Blau, 1982; Blau and Golden, 1986; Farley, 1987; Gauthier and Bankston, 1997; Loftin and Parker, 1985; Rosenfeld, 1986; Smith and Bennet, 1985; Stack, 1984; Williams and Drake, 1980). Therefore, to remain consistent with the theoretical framework established thus far, the results of their analyses are discussed separately in this chapter as well.

The effect of absolute deprivation on crime has been well tested, with the 214 studies in the sample producing 153 contributing effect size estimates for the effect of

poverty on crime (see Table 6.6). The overall mean effect size of poverty is in the top tier of the distribution at .253, with an overall ranking of eighth and an average rank of 8.73 across each of the methodological specifications examined in Chapter Five. Table 5.7 from Chapter Five also indicates that the effect of poverty was one of the five macro-level predictors of crime to receive scores of “high” for both strength and stability of effects.

Table 6.6 contains the results of the general and theory-specific regression models, which further test for the conditioning influences of various methodological factors on the effect size estimates for poverty. The general model for poverty in Table 6.6 (Model 1) indicates that none of the methodological factors significantly influence the effect of poverty on crime. Furthermore, for the theory-specific regression model (Model 2 for poverty), a dummy variable was included indicating whether the effects of inequality were controlled in the statistical model. This variable was entered into the regression equation to determine how the effect size of poverty “holds up” when another indicator of economic deprivation is taken into account. The effect of this control variable was not statistically significant, which means that, across all existing macro-level studies of crime, the effect of poverty on crime does not appear to be significantly conditioned by the presence of statistical controls for the effects of inequality.

The Empirical Status of Absolute Deprivation/Conflict Theory

Based on the results of the meta-analysis contained in Chapter Five and the additional analyses reported in this chapter, the empirical status of absolute deprivation/conflict theory is that it appears to be well supported across all macro-level

studies of crime. The effects of poverty on crime are fairly robust (consistently above .20). Perhaps even more important, the relatively strong effect of poverty on crime remains stable across various methodological conditions.

RELATIVE DEPRIVATION/INEQUALITY THEORY

Key Theoretical Variables: Strength and Stability

Table 6.5 contains a summary of the effects for the key theoretical variable in relative deprivation/inequality theory: economic inequality. Similar to its absolute deprivation cousin, the relationship between relative deprivation/inequality and crime has also been well tested empirically, with the sample of studies producing 167 contributing effect size estimates for the effect of inequality on crime (see Table 6.6). The overall mean effect size estimate for inequality is .207, which places it in the top tier of predictive strength. Its overall rank in the distribution of effect sizes is twelfth, with an average ranking of 12.00 across the various methodological specifications examined in Chapter Five.

Table 6.6 contains the results of the general and theory-specific regression models, which further test for the conditioning influences of various methodological factors on the effect size estimates, for inequality. Unlike the effects of poverty, the effect size estimates for inequality on crime are much more sensitive to methodological variations. In particular, the general regression (Model 1) shows that the effect of inequality on crime is significantly higher at larger levels of aggregation ($b = .055$), yet its effects are significantly reduced when predicting violent crimes ($b = -.064$) and when

the ratio of the sample size to the number of independent variables is high ($b = -.003$). A dummy variable for whether the effect of poverty was included in the study was entered into the theory-specific regression model (Model 2) in Table 6.6. While the previous methodological conditioning effects from the general model remain, the presence of controls for poverty in a statistical model does not have a significant effect on the magnitude of the inequality-crime relationship across all empirical tests.

The Empirical Status of Relative Deprivation/Inequality Theory

Based on the results of the meta-analysis contained in Chapter Five and the additional analyses reported in this chapter, the empirical status of relative deprivation/inequality theory is that it too appears to be fairly well supported across all macro-level studies of crime. Similar to the mean effect size of poverty, the effects of inequality on crime are also fairly robust (generally above .20 across the different specifications calculated in Chapter Five). Nevertheless, as can be seen in Table 6.6, the effect size estimates for inequality are not as stable across various methodological conditions as those that proxy the effects of poverty. Even so, the overall effect size for inequality is in the top tier of predictive strength (see Table 5.1) and its relative stability is categorized as “high” (see Table 5.7). Thus, continued confidence in the validity of the relative deprivation/inequality paradigm is certainly warranted.

ROUTINE ACTIVITIES THEORY

Key Theoretical Variables: Strength and Stability

Table 6.7 contains a summary of the effects for the key variables specified by routine activities theory: the household activity ratio and unemployment. The mean effect size of the household activity ratio is in the top tier of predictive strength at .228. This places the household activity ratio at tenth in the overall distribution of effect sizes, with a mean rank of 10.63 across the methodological specifications examined in Chapter Five. The mean effect size of unemployment, however, is substantially weaker at .135. Thus, viewed as a mid-level predictor of crime, unemployment has an overall rank in the distribution of effect sizes of fifteenth and an average rank of 14.50 across the methodological specifications.

Table 6.8 contains the results of the general and theory-specific regression models examining the impact of methodological characteristics on the effect size estimates for the household activity ratio and unemployment. The general regression models (Model 1 for both predictors) indicate that the effect sizes of both routine activities predictors are significantly larger when variables from competing theories are controlled (household activity ratio $b = .157$, unemployment $b = .150$). Other methodological conditioning influences include the significantly higher mean effect size of the household activity ratio in longitudinal studies ($b = .618$), and the significantly weaker mean effect size of unemployment when predicting rates of violent crime ($b = -.082$).

The Conditioning Effects of Inequality. Jackson (1984) observed that the degree of empirical support for routine activities theory in general, and the effect of the household activity ratio on crime in particular, may be contingent upon the potentially mediating effects of inequality. To examine this assertion across all macro-level studies

of crime, the theory-specific regression models contained in Table 6.8 (Model 2 for both predictors) included a variable for whether controls for inequality were included in a study's statistical model. This additional methodological characteristic did not significantly predict the effect size estimates for either predictor specified by routine activities theory.

Temporal Aggregation and Unemployment. Another methodological issue that is directly related to routine activities theory is temporal aggregation—especially in the context of the impact of unemployment on crime. In particular, researchers have argued that, depending on how the time lag is specified in a particular study, unemployment may exhibit different—and often conflicting—relationships with crime (see, e.g., Kapuscinsky et al., 1999; Land et al., 1985). For example, a short time lag testing the effect of unemployment on crime (e.g., in cross-sectional research designs, or in time series models specifying short time lags or zero-order transfer functions) is likely to produce a *negative* relationship between the two variables. In the language of routine activities theory, this may be interpreted as a “guardianship effect” produced by the reduction of the dispersion of activities away from the home brought on by the loss of employment (Felson and Cohen, 1980).

As stated previously, however, the architects of a number of macro-level theories of crime may reasonably claim “unemployment,” in one form or another, as a key theoretical variable. Indeed, unemployment could be interpreted as a source of economic deprivation (relative or absolute), as a precursor to social disorganization, and/or even as a proxy for frustration-induced anger (anomie/strain). While these alternative viewpoints regarding how unemployment should affect crime hold different causal assumptions, they

each make the common prediction that the functional form of the unemployment-crime relationship should be *positive*. Accordingly, Land et al. (1985) contend that the positive relationship between unemployment and crime will most likely be revealed in empirical tests specifying a relatively long time lag. Specifically, estimating a longer time lag in a statistical model should tap into the criminogenic effects brought on by economic deprivation, frustration-induced anger, and so on, which may result in a positive association between unemployment and crime.

Table 6.8 contains the results of the regression model predicting the effect size of unemployment on crime (Model 2), with the inclusion of a statistical control for the time lag specified by each empirical study (measured in months).⁸¹ In this model, the time lag was not significantly related to the effect size of unemployment. It is possible, however, that the aggregated least-squares regression technique that produced the results presented in Table 6.8 may mask the effect of the time lag on the unemployment-crime relationship because, in essence, two separate causal structures may be at work (i.e., those predicting positive versus inverse effects). Thus, to model these separate causal structures, Table 6.9 contains the results of two logistic regression models predicting whether the effect of unemployment on crime was *statistically significant and positive* (0 = no, 1 = yes), and whether the effect of unemployment on crime was *statistically significant and negative* (0 = no, 1 = yes).⁸²

In the logistic regression model predicting a positive and significant relationship between unemployment and crime, the variable indicating the time lag was statistically

⁸¹ Cross-sectional studies were coded as “0” for the time lag variable.

⁸² Logistic regression techniques were used in these models because the dependent variable for each equation was dichotomous (Hanushek and Jackson, 1977).

significant and positive ($b = .154$).⁸³ This means that, as the time lag gets larger across empirical studies, the likelihood of uncovering a statistically significant positive effect of unemployment on crime increases as well. Accordingly, the logistic regression model predicting a negative and significant relationship between unemployment and crime reveals an inverse effect of the time lag variable ($b = -.269$). This indicates that, as the time lag grows shorter across empirical studies (i.e., approaching zero), the likelihood of uncovering a statistically significant negative effect increases.

The Empirical Status of Routine Activities Theory

Based on the results of the meta-analysis contained in Chapter Five and the additional analyses reported in this chapter, the empirical status of routine activities theory, in terms of its level of support across all macro-level studies of crime, is incomplete. Despite being well tested (the mean effect size of the household activity ratio and unemployment as based on 37 and 204 contributing effect size estimates, respectively), most studies focus only on the “guardianship” component of the theory (e.g., see the discussion by Bryant and Miller, 1999). While the effects of guardianship on crime—as typically proxied by the household activity ratio—are fairly robust at .228, few studies have examined the validity of the other propositions contained in the theory (i.e., what are the independent effects of “motivated offenders” or the presence of “suitable targets” on crime?).

⁸³ Unlike the unstandardized coefficients generated by the WLS regression models, the maximum-likelihood coefficients from the logistic regression models have no intuitive interpretation. Instead, they represent the change in the natural log of the odds ratio for the dependent variable across the categories of the independent variable (Hanushek and Jackson, 1977). For ease of interpretation, therefore, the significance tests contained in Table 6.9 may be most informative (i.e., magnitude aside, whether or not the methodological characteristic significantly predicts the dependent variable).

As such, two facets of routine activities theory warrant additional empirical scrutiny before greater confidence in its predictive validity is deserved. First, the neglected elements of the routine activities perspective should be the subject of future empirical tests. While this trend has already begun (see, e.g., Bryant and Miller, 1999), there is, to date, only a paltry sum of studies exploring either the motivated offender or suitable targets portions of routine activities theory as it was originally conceived. Second, future researchers may wish to follow the approach taken in early tests of the theory (e.g., Jackson, 1984) that examined how variables specified by routine activities theory may mediate, or be mediated by, other social-structural or socio-economic variables (e.g., poverty, inequality, family disruption, and/or racial heterogeneity). Perhaps after such additional tests emerge, a better understanding of the empirical status of routine activities theory—in its entirety—may be reached.

RATIONAL CHOICE/DETERRENCE THEORY

Key Theoretical Variables: Strength and Stability

Table 6.10 contains a summary of the effects for the key variables specified by macro-level rational choice/deterrence theory: the incarceration effect, the policing effect, and the effect of get tough policies on crime. Aside from the few tests of the effect of get tough policies on crime (with 37 contributing effect size estimates, 31 of which were related to the death penalty), macro-level tests of deterrence theory are numerous (see Appendix 6). The most common deterrence-related predictors of crime to appear in empirical tests are the incarceration effect (with 46 contributing effect size estimates) and the policing effect (with 114 contributing effect size estimates).

As can be seen in Table 6.10, the relative strength of the macro-level predictors of crime specified by deterrence theory range from those falling in the top to the bottom tiers of overall predictive strength. The mean effect size of the incarceration effect⁸⁴ is -.317, which places it at third in the overall distribution of effect sizes, with an average ranking of 5.77 across the methodological specifications examined in Chapter Five. The mean effect size estimates for both the policing effect and the effect of get tough policies, however, fall into the bottom tier of predictive strength at -.054, with rankings that consistently fall into the bottom third of the distribution of effect sizes (overall and across the methodological specifications).

Table 6.11 contains the results of the general and theory-specific regression models, which further test for the conditioning influences of various methodological factors on the effect size estimates, for the incarceration effect and the policing effect.⁸⁵ The general model for the incarceration effect (Model 1) indicates that its effect is somewhat sensitive to varying methodological conditions. The deterrent (or incapacitative) effect of incarceration is significantly stronger at higher levels of aggregation ($b = -.101$)⁸⁶ and when applied to samples limited to the U.S. ($b = -.457$). None of the general methodological characteristics, however, were significantly related to the effect size estimates for the policing effect.

⁸⁴ Although the technique of meta-analysis is not able to determine the construct validity of the various macro-level predictors of crime, it should be noted that the effect size of incarceration on crime may indicate either a deterrent or incapacitation effect (or some combination of the two).

⁸⁵ The effect of get tough policies was not subjected to the additional regression analyses in this chapter because of the potential instability of the regression coefficients that would be brought on by examining such a small sample ($n = 37$). Although the same number of contributing effect size estimates for the household activity ratio ($n = 37$ also) was included in the additional regression analyses, they all represented the *same variable*, and therefore greater stability could be theoretically expected than for the various measures comprising the get tough policy predictor domain.

Aside from these general methodological characteristics, the discussion of macro-level rational choice/deterrence theory contained in Chapter Three brought up two methodological issues directly relevant to this theoretical tradition. The first issue concerns how well deterrence variables—in particular, the incarceration effect—can predict crime rates when the effects of variables from competing criminological theories have been controlled. Thus, the theory-specific regression model for the incarceration effect presented in Table 6.11 (Model 2) indicates that when variables from routine activities theory (unemployment or the household activity ratio) are controlled, the incarceration effect is significantly weaker predictor of crime ($b = .358$). Thus, although the effect of incarceration on crime does not appear to be *generally* influenced by controls for other macro-level predictors, when variables from routine activities theory are entered in to the statistical model, the effect of incarceration on crime is substantially diminished.⁸⁷

The second macro-level rational choice/deterrence theory-specific methodological issue, which tends to be most often targeted toward studies examining the policing effect, is whether a control for simultaneous effects was estimated in the study. In particular, researchers have argued that the failure to control for the effects of crime on policing variables in a statistical model may artificially inflate the predictive capacity of policing variables (Nagin, 1980a; 1980b). Thus, the theory-specific regression model for the policing effect in Table 6.11 includes controls for two additional methodological factors: a dummy variable indicating whether reciprocal effects were estimated, and another

⁸⁶ Since deterrence theory variables predict inverse relationships with crime, a negative coefficient in the regression models presented in Table 6.11 indicate a more substantial negative (i.e., deterrent) effect.

⁸⁷ Separate regression models were estimated testing for the potential conditioning effects of variables from each of the other criminological theories in the incarceration effect as well. None of the dummy variables

indicating whether an “identification restriction” was used in the study’s statistical model. Both of these methods are assumed to account for the problem of simultaneous causation (or “endogeneity”) when assessing the effect of policing variables on crime. Table 6.11 indicates that, while the “reciprocal effects” variable was not statistically significant, Model 2 reveals that the deterrent effect of policing variables on crime is significantly stronger when an identification restriction is not used ($b = -.181$).

The Empirical Status of Rational Choice/Deterrence Theory

Based on the results of the meta-analysis contained in Chapter Five and the additional analyses reported in this chapter, the empirical status of macro-level rational choice/deterrence theory is that it is only marginally supported across all empirical tests. Despite the specification of the incarceration effect (which could, theoretically, be an incapacitation as opposed to a deterrent effect)—which is one of the five macro-level predictors to receive a rating of “high” for both strength and stability (see Table 5.7)—the other predictors specified by the theory are consistently ranked in the bottom tier of predictive strength. Furthermore, consistent with the discussion of rational choice/deterrence theory in Chapter Three, high levels of support for the theory are more likely to be found in empirical tests that are weakest methodologically, such as, those that do not control for variables from competing criminological theories and those that fail to account for the potentially confounding effects of simultaneous causation between crime rates and rational choice/deterrence theory predictors.

representing the other macro-level theories were significantly related to the incarceration effect size estimates; thus, only the results for the routine activities theory variables are reported in Table 6.11.

SOCIAL SUPPORT/ALTRUISM THEORY

Key Theoretical Variables: Strength and Stability

Table 6.12 contains a summary of the effects for the key theoretical variable specified by social support/altruism theory. As stated in Chapter Three, explicit tests of this theory have only recently begun to appear in academic journals (see Appendix 7). Even so, a number of studies testing other criminological theories included statistical controls for social support/altruism measures, which then contributed to the overall sample of social support/altruism effect size estimates ($n = 47$, see Table 6.13). The overall mean effect size estimate for social support/altruism measures is $-.216$, which places it in the top tier of predictive strength. Its overall rank in the distribution of effect sizes is eleventh, with an average ranking of 12.89 across all of the methodological specifications examined in Chapter Five.

Table 6.13 contains the results of the general and theory-specific regression models, which further test for the conditioning influences of various methodological factors on the effect size estimates, for social support/altruism. The general regression model (Model 1) indicates that none of six methodological factors significantly condition the effect of social support/altruism on crime. The theory-specific regression models in Table 6.13 were designed to uncover how the relationship between social support/altruism and crime may vary when controls for variables from specific competing criminological theories have been estimated. These results indicate that the effect size estimates for social support/altruism are significantly conditioned by variables from absolute deprivation theory (Model 2, $b = -.312$), by variables from relative deprivation theory (Model 3, $b = -.244$), and by variables from routine activities theory (Model 4, $b =$

-184).⁸⁸ Nevertheless, it should be noted that these statistically significant regression coefficients are all negative. Since measures of social support/altruism are theoretically expected to have an inverse effect on crime, these negative regression coefficients indicate that the relationship between social support/altruism and crime is actually *stronger* when variables from these competing criminological theories are controlled in a statistical model.

The Empirical Status of Social Support/Altruism Theory

Based on the results of the meta-analysis contained in Chapter Five and the additional analyses reported in this chapter, the empirical status of social support/altruism theory is that, to date, it is well supported across existing empirical tests. The mean effect size of social support/altruism measures on crime is generally robust (consistently greater in magnitude than .20 across the different specifications calculated in Chapter Five). Furthermore, these effects are not consistently sensitive to methodological variations, nor are they systematically diminished when variables from specific criminological theories are controlled across studies. Even so, relative to other macro-level criminological theories, social support/altruism theory is somewhat “new,” and therefore the validity of certain propositions made by the theory are still in question. Accordingly, future tests of the theory should focus on two facets of the theory that have yet to be extensively assessed by researchers thus far.

The first question concerns the relative predictive capacity of social support/altruism measures from public versus private sources. In short, which type of

⁸⁸ Similar to the results of the mediating effects for deterrence theory presented in Table 6.11, only the theory-specific dummy variables that were significantly related to the effect size estimates for social

social support/altruism is better able to exert an inverse effect on crime? Is it that which indicates levels of support/altruism above and beyond what the state can provide (e.g., see Chamlin and Cochran, 1997)? Or, is social support/altruism of any kind (e.g., AFDC or other public transfer payments) just as important (e.g., see the discussion by Cullen, 1994)? The second question concerns whether measures of social support/altruism are able to mediate the criminogenic effects of other social-structural or socio-economic predictors of crime. For example, are levels of social support/altruism able to effectively mediate the relationship between measures of economic deprivation (poverty and/or inequality) and crime? In essence, although social support/altruism theory is currently well supported by the existing empirical studies, there are still a number of testable research questions that scholars may wish to address in the future.

SUBCULTURAL THEORY

Key Theoretical Variables: Strength and Stability

Table 6.14 contains a summary of the effects for the key variables specified by subcultural theory: urbanism and the southern effect (as proxies for the effects of urban and southern subcultures of violence). Both variants of macro-level subcultural theory have been adequately tested, with the sample of studies producing 178 contributing effect size estimates for the relationship between urbanism on crime and 110 contributing effect size estimates for the southern effect (see Table 6.15). Table 6.14 indicates that the overall mean effect size estimates for urbanism and the southern effect are .162 and .125, respectively, which designates both predictors as mid-level in strength. The effect of

support/altruism were reported in Table 6.13.

urbanism on crime has an overall ranking of thirteenth and an average ranking of 14.73 across the methodological specifications examined in Chapter Five. The southern effect has an overall ranking of seventeenth and an average ranking of 16.27 across the methodological specifications.

Table 6.15 contains the results of the general and theory-specific regression models that test for the conditioning influences of methodological characteristics on the effect size estimates for the urban and southern effects on crime.⁸⁹ As can be seen in the general regression model for the effect of urbanism on crime (Urbanism Model 1), the magnitude of this relationship is significantly conditioned by all but one of the general methodological factors. The effect size of the relationship between urbanism and crime is significantly higher at larger levels of aggregation ($b = .039$), when tested in samples restricted to the U.S. ($b = .129$), and when the ratio of the sample size to the number of independent variables is high ($b = .001$). The urbanism effect is significantly diminished, however, when variables from competing theories are controlled in a statistical model ($b = -.189$) and when predicting rates of violent crime ($b = -.071$).

The southern effect on crime is also sensitive to methodological variations. The general regression model (Southern Model 1) indicates that the effect size of southern region on crime is significantly reduced when variables from competing theories are controlled in the statistical model ($b = -.625$). In extending this issue, based on the discussion in Chapter Three, the theory-specific regression models in Table 6.15 (Model 2 and Model 3) include dummy variables for whether the effects of economic deprivation

⁸⁹ Although certain theory-specific controversies have been articulated with regard to the southern subculture of violence perspective (cf. Gastil, 1969; Hackney, 1971; Loftin and Hill, 1974), these issues have not been raised in reference to the urban subculture of crime tradition. Thus, the theory-specific

were controlled in the statistical model (e.g., see the discussion by Loftin and Hill, 1974). While the presence of variables from absolute deprivation theory does not significantly influence the southern effect on crime, controlling for variables from relative deprivation theory (e.g., economic inequality) does significantly reduce the effect of southern region on crime ($b = -.108$).

The Empirical Status of Subcultural Theory

Based on the results of the meta-analysis contained in Chapter Five and the additional analyses reported in this chapter, the empirical status of macro-level subcultural theory is that neither variation of the theory is consistently or substantially supported by the existing empirical research. The mean effect size estimates for both urbanism and southern region on crime are, at best, moderate. Furthermore, these moderate effects for both macro-level subcultural predictors on crime are significantly reduced when controls for variables from competing criminological theories are introduced into statistical models. Thus, the empirical merit of subcultural theory, relative to the other theoretical perspectives reviewed in this chapter, may be low enough to warrant abandoning it as a legitimate framework for explaining variations in crime at the aggregate level.

Nevertheless, should continued interest in the validity of the macro-level subcultural perspective remain, researchers should focus on two aspects of the theory. First, scholars may wish to determine how the theory may be revised to potentially accommodate (or integrate) the effects of other, more robust, predictors of crime (e.g.,

regression models presented in Table 6.15 are restricted to the effect size estimates for the southern effect only.

racial heterogeneity, poverty). Second, and perhaps just as important, researchers should attempt to uncover whether *direct* measures of culture can be found (i.e., as opposed to proxy measures such as regional dummy variables) and subsequently used for empirical analysis.

CHAPTER 7

CONCLUSIONS

During the 1960s and 1970s, social-psychological—or individual-level—theories of criminal and/or deviant behavior dominated the criminological landscape. During the late 1970s and 1980s, however, a number of empirical studies and theoretical discussions addressing the ecological correlates of crime from various theoretical perspectives began to emerge. Among the more important works in this tradition during this period were the development of routine activities theory by Cohen and Felson (1979), the seminal work of Blau and Blau (1982) on inequality and crime, the revitalization of social disorganization theory by scholars such as Bursik (1986, 1988), Sampson and Groves (1989), and Wilson (1990, 1996), and the renewed interest in rational choice/deterrence theory at the ecological level (Becker, 1968; see also Becker, 1978). When taken together, these contributions prompted a shift in the focus of criminological theory and research away from the individual toward the macro-level (see the discussion by Bursik and Grasmick, 1993).

This is not to say that macro-level theories have replaced individual-level explanations of criminal and deviant behavior among criminologists. To be sure, recent statements of criminological theories that have focused on the micro-level are still ubiquitous, including those from control (Gottfredson and Hirschi, 1990), social learning (Akers, 1998), general strain (Agnew, 1992), and life-course perspectives (Sampson and Laub, 1993). Nevertheless, given the resurgence of interest in the ecology of crime in

recent years, it may be safe to conclude that macro-level criminological theory and research currently share, at minimum, “equal time” on the criminological stage with their micro-level brethren. Indeed, over the last twenty years, in the major sociology, criminology, criminal justice, and economics journals, more than 200 empirical studies have been conducted which have addressed the predictors of aggregate crime rates. In the process, new theoretical perspectives have been developed, including institutional anomie theory (Messner and Rosenfeld, 1994a), social support/altruism theory (Chamlin and Cochran, 1997; Cullen, 1994), and even a macro-level version of general strain theory (Agnew, 1999).

Despite the theoretical and empirical advances made by these works, however, there has been only a limited effort on the part of researchers to “take a step back” and to “make sense” of what the body of macro-level empirical studies tells us about crime. Literature reviews that have attempted to establish the empirical status of certain macro-level criminological relationships are in short supply, and those that have been conducted to date are limited in two respects. First, these reviews tend only to focus on a limited set of empirical relationships, such as unemployment and crime (see, e.g., Chiricos, 1987; Piehl, 1998) or economic deprivation and crime (Hsieh and Puch, 1993). While such reviews are certainly not without value, they have not been able to establish the empirical validity of the relationships being reviewed *relative to other macro-level predictors of crime*. Second, these reviews have not provided precise estimates of the degree to which methodological variations condition the significance and strength of certain macro-level relationships. Given these limitations, perhaps the most glaring problem associated with

the few reviews conducted thus far is that they tell us little about the relative validity of the major macro-level theories of crime.

The purpose of this dissertation, therefore, was to undertake a *systematic* review of the existing body of macro-level criminological scholarship in the form of a “meta-analysis.” In response to the limitations of the existing “narrative” literature reviews noted above, the method used in the present study provided for the fulfillment of three major objectives. First, the relative strength—or “effect size”—of the empirical relationship to crime rates of the variables, or “predictors,” included in macro-level studies was established. Second, the analysis was able to precisely uncover how certain methodological variations may condition the effect size of particular macro-level relationships. Thus, a more firm understanding of the relative “strength” and “stability” of the macro-level predictors of crime has now been reached. Finally, the third objective of this dissertation was to use the meta-analysis as a source of information regarding the empirical status of the major macro-level theories of crime.

The remainder of this chapter contains a more detailed examination of what the present meta-analysis revealed about these objectives. The structure of the discussion is as follows: First, the results of the analyses contained in Chapter Five are revisited according to what conclusions can be drawn regarding the relative strength and stability of the macro-level predictors of crime that were examined. In short, what kinds of characteristics do social aggregates and/or geographical areas that tend to experience high crime rates share in common? Second, a similar analysis of the results presented in Chapter Six is provided in terms of what we may now say about the empirical status of the major macro-level theories of crime. Third, the implications of this study for future

research are discussed. Fourth, the potential limitations to the study are noted and discussed. Finally, the implications of this research for criminal justice and social policy development are outlined.

MACRO-LEVEL PREDICTORS OF CRIME: EMPIRICAL IMPORTANCE AND SUBSTANTIVE IRRELEVANCE

The analyses contained in Chapter Five were intended to establish the overall and relative mean effect size estimates for thirty-one different macro-level predictors of crime. These analyses also provided insight as to the degree to which the mean effect sizes of certain macro-level predictors of crime were sensitive to varying methodological conditions. Based these analyses, conclusions can be reached regarding two broad issues: (1) what are the consistently robust predictors of crime (i.e., those that are empirically important), and (2) what are the consistently weak predictors of crime (i.e., those that are substantively irrelevant)?

The Relatively Strong and Stable Predictors of Crime

Five macro-level predictors of crime were found to have consistently scored high in the various rankings of relative strength—overall and across the methodological specifications examined. Among these predictors were two indicators of racial composition (the *percent non-white* and the *percent black*), the traditional sociological predictor of *family disruption*, an indicator of economic deprivation (*poverty*), and one criminal justice system-related predictor (*incarceration*). This does not imply that none of the other macro-level predictors of crime are substantively unimportant. Rather, the

present meta-analysis revealed that these five macro-level predictors of crime tend to be the most generally robust across different methodological specifications. As stated in Chapter Five, therefore, future studies that fail to estimate (or control for) the effects of at least one or more of these predictors in their statistical models run a substantial risk of being misspecified.

Despite their strong independent effects, when taken together, these top tier predictors of crime paint a fairly clear picture as to what types of factors tend to characterize those social aggregates that persistently experience high levels of criminal victimization. In particular, the meta-analysis shows that high levels of racial heterogeneity, the presence of economic deprivation, and high rates of family disruption are among the strongest and most stable predictors of crime. Although the present analysis was not designed to create an index of these factors, when viewed in conjunction with each other they begin to resemble the concept of “concentrated disadvantage” that has been previously discussed by scholars from the social disorganization and relative deprivation/inequality perspectives (see, e.g., Currie, 1985; Wilson, 1987; see also the discussion by Anderson, 1999).

Even so, it may be tempting to assert that the effects of concentrated disadvantage may be mediated by incarceration—the remaining strong and stable *direct* predictor of crime. Nevertheless, a number of scholars have noted that high rates of incarceration may *indirectly increase* crime rates through its effect on both family disruption and the potential drop into economic deprivation brought on by the loss of an additional (or perhaps the only) income (see, e.g., Tonry 1995; Wilson, 1987, 1996). Furthermore, this potential threat is heightened given the disproportionately high incarceration rates among

members of racial/ethnic minority groups (Donziger, 1996). Thus, when taken in its entirety, the present meta-analysis lends considerable support to the concentrated disadvantage thesis—a finding that, at minimum, should be either explained or accommodated by the major macro-level theories of crime if they wish to remain empirically viable.

The Relatively Weak Predictors of Crime

The present meta-analysis also demonstrated that the predictors of crime with the most consistently weak mean effect size estimates (i.e., those falling into the “bottom tier” of predictive strength) are those related to the criminal justice system. Although the effect of incarceration is an exception, the rest of the criminal justice system-related predictors of crime—including *get tough policies*, *police expenditures*, *police per capita*, and *police size*—failed to exert a substantively important effect on crime. Indeed, their mean effect size estimates were typically below .10.

Thus, the meta-analysis conducted in this dissertation provides a certain level of insight as to relative ability of formal versus informal mechanisms of social control to affect crime rates. Indeed, state-based efforts at reducing crime through the manipulation of formal social control arrangements—such as through increasing sentence lengths for certain offenses, by increasing police officer strength, and so on—have had no appreciable effect on crime rates. This finding has been previously articulated by reviews of the micro-level criminological literature addressing the limited capacity of formal sanctions to affect individuals’ likelihood of engaging in criminal behavior (see

Paternoster, 1987). This study essentially echoes this apparent empirical regularity found in individual-level studies to the macro-level of analysis.

This does not immediately imply that all public policy proposals aimed at reducing crime are doomed to failure. Indeed, the meta-analysis also indicated that public levels of social support consistently maintain respectable inverse relationships with crime. Nevertheless, the demonstrated inadequacy of the methods typically employed by the state for exerting formal social control over its citizenry—particularly those used in the U.S. in recent years—represents a fairly clear and convincing failure of the crime control capacity of traditional institutions of formal social control.

IMPLICATIONS FOR THE MAJOR MACRO-LEVEL THEORIES OF CRIME

By establishing the relative effect size of the various macro-level predictors of crime, the meta-analysis presented in this dissertation, in turn, provides insight into an issue that is perhaps even more important: the relative empirical status of the major macro-level theories of crime. Most often, researchers attempting to establish the relative validity of two or more criminological theories will pit them against one another in the same statistical model to see which theories' variables "hold up" across alternative specifications. This technique is useful in its own right, and the present analysis should not be interpreted as a sweeping attempt at a methodological *coup* bent on undermining the validity of studies rooted in this tradition. Rather, the suggestion being made here is that the meta-analysis of a body of empirical literature represents a different, yet equally legitimate, method of discerning the relative validity of criminological theories.

In other words, this does not mean that applying meta-analytic techniques to a body of literature is the *only* way to gain insight into which criminological theories have garnered more or less empirical support. Instead, the argument being made here is that, by establishing the relative predictive capacity of the variables specified by certain criminological theories, meta-analysis contains a “built-in” structure for evaluating the relative strength of those theories in an objective manner. While this trend toward the meta-analysis of the empirical tests of certain individual-level criminological theories has already begun (Pratt and Cullen, 2000; see also Sellers, Pratt, Winfree, and Cullen, 2000), the research contained in this dissertation is the first to apply the method to the empirical examinations of each of the major macro-level criminological theories. Based on these analyses (those presented in Chapter Five and in Chapter Six), this section discusses which theories were well supported, moderately supported, and weakly supported by existing empirical studies. In doing so, the influences of theory-specific methodological issues will be noted where appropriate. Finally, this section also considers the implications of “variable ownership”—or, how multiple theories may lay claim to the same variable (e.g., unemployment)—on the relative empirical status of the macro-level theories of crime.

Theories with Strong Empirical Support

Table 7.1 contains a tabular summary of the relative empirical status of the major macro-level theories of crime based on the results of the analyses presented in Chapter Five and in Chapter Six. Overall, two of the macro-level criminological theories should be viewed as having received strong empirical support across the body of studies

included in the sample. First, social disorganization theory was designated as having strong empirical support, primarily due to its specification of three of the five macro-level predictors that were found to have scored “high” on both the strength and stability criteria in Chapter Five: two measures of racial heterogeneity (when measured as the *percent non-white* and the *percent black*) and *family disruption*. These predictors were among the most robust and reliable of all of the effect size estimates contained in the meta-analysis, with magnitudes consistently above .20. Although other social disorganization theory indicators such as low SES⁹⁰ and residential mobility were not in the top tier of predictive strength, measures of “collective efficacy”—despite only appearing in a limited number of empirical tests conducted thus far—show considerable promise in predicting levels of neighborhood crime ($ADJ_z = -.315$). Furthermore, as indicated by the analyses in Chapter Six and in Table 7.1, the level of support for social disorganization theory across empirical tests does not appear to dwindle under different methodological conditions.

The same could be said of the empirical status of absolute deprivation/conflict theory. Its key theoretical variable—*poverty*—was also one of the five “strong and stable” predictors of crime noted in Chapter Five. To be sure, the analyses in Chapter Six (and as indicated in Table 7.1) show that in addition to having a substantially robust mean effect size ($ADJ_z = .253$),⁹¹ the effect of poverty on crime is not significantly conditioned

⁹⁰ As will be discussed more fully below, the relatively weaker effect of low SES and crime—a potentially damaging finding for social disorganization theory—may be somewhat misleading. In particular, it may be reasonable to assume that low SES was specified by Shaw and McKay (and even more so by contemporary social disorganization theorists) as an indicator of economic hardship—a construct that may be better proxied by measures such as poverty.

⁹¹ Consistent with the standards established in Chapter Five, mean effect size estimates greater than .20 are generally considered to be substantial, or, substantively important (see the discussion by Andrews and Bonta, 1994). Mean effect size estimates between .20 and .10 are therefore considered “moderate” in strength, and those that fall below .10 are generally considered to be of little substantive importance.

by methodological factors (e.g., measurement differences, model specification variations, and so on). Given the relative strength and stability of the relationship between poverty and crime, therefore, absolute deprivation/conflict theory was designated as having received strong empirical support across studies.

Table 7.1. Tabular summary of the relative empirical status of the major macro-level theories of crime.

Theory	Adequately Tested	Conditioned by Methodology	Overall Empirical Support
Social Disorganization	Yes	No	High
Absolute Deprivation/Conflict	Yes	No	High
Anomie/Strain	No	N/A	Moderate
Social Support/Altruism	No	No	Moderate
Relative Deprivation/Inequality	Yes	No	Moderate
Routine Activities	Yes	Yes	Moderate
Rational Choice/Deterrence	Yes	Yes	Weak
Subcultural	Yes	Yes	Weak

Theories with Moderate Empirical Support

Four macro-level theories of crime have received a moderate level of empirical support across studies. Two of these four theories were placed in this category primarily because they have not been adequately tested at this point in time. First, anomie/strain theory—including institutional anomie theory—has yet to be subjected to a large number of rigorous empirical tests. Even so, consistent with the discussion contained in Chapter Six, the few tests of the theory that have been conducted thus far have yielded fairly

strong support for certain propositions made by the theory (e.g., the inverse effect of the *strength of noneconomic institutions* on crime, $ADJz = -.391$). Thus, support for anomie/strain theory has been revealed in the existing tests, and therefore a designation of “moderate” empirical support is deserved.

The empirical status of social support/altruism theory is similar to that of anomie/strain theory. Since the perspective is so new (at least relative to many of the other criminological theories), few explicit tests of the theory have emerged to date. Nevertheless, the sample of studies contained in the meta-analysis did reveal a substantial amount of empirical support for the theory. Indeed, an inverse relationship between measures of *social support/altruism* and crime was consistently observed across empirical studies ($ADJz = -.216$). Furthermore, based on the analyses contained in Chapter Six, the effect size of social support/altruism measures is not particularly sensitive to methodological variations. Thus, support for social support/altruism theory has been revealed by the existing tests, and therefore—like anomie/strain theory—a designation of “moderate” empirical support is also deserved.

Two other macro-level criminological theories were placed in the moderate empirical support category not because they have yet to be adequately tested, but rather because of the existence of consistently luke-warm empirical support across empirical tests. First, relative deprivation/inequality theory was designated as having received a moderate degree of empirical support for two reasons. First, compared to the effects of poverty (absolute deprivation), the mean effect size of *inequality* (e.g., the Gini coefficient) was consistently weaker (although still fairly strong at $ADJz = .207$). Second, relative to the effects of poverty, indicators of economic inequality were much

more sensitive to certain methodological variations (such as the level of aggregation, the ratio of sample size to independent variables, and when violent crime is the dependent variable). Thus, the results of the meta-analysis contained in Chapter Five indicated that although inequality did have a substantial mean effect size with crime, its level of empirical support across studies is not as high as its absolute deprivation/conflict theory counterpart—hence the label of moderate empirical support.

This is not to say that relative deprivation/inequality theory is therefore of little value to criminologists since, as previously discussed in Chapter Three, many scholars view the constructs of absolute and relative deprivation as more conceptually similar than as theoretically distinct (see Bailey, 1984, 1999; Crutchfield, 1989; Messner, 1982, Messner and South, 1986; Peterson and Bailey, 1988; Williams and Flewelling, 1988). Indeed, to the extent that measures such as poverty and inequality both tap into the common domain of “economic deprivation,” their separation by criminologists may artificially impose on inequality the potential label of a second class measure of the degree to which social collectives may be experiencing economic hardship. Nevertheless, in keeping with the framework of separating the two theories of economic deprivation—which has been the approach taken throughout this dissertation—relative deprivation/inequality theory has not enjoyed as much support across empirical studies as absolute deprivation/conflict theory.

The last macro-level theory of crime to have received a moderate level of empirical support is routine activities theory. Like relative deprivation/inequality theory, routine activities theory has been adequately tested by researchers at this point in time. The level of support afforded to the theory across existing empirical tests, however, is

extremely inconsistent. In particular, the mean effect size of the *household activity ratio* on crime is quite robust ($ADJz = .228$), yet its effect varies considerably according to certain methodological characteristics (e.g., whether variables from competing theories are included in the model, and whether the research design was cross-sectional versus longitudinal). Another key variable specified by routine activities theory—*unemployment*—is also quite sensitive to methodological variations (e.g., whether variables from competing theories are included in the model, whether the dependent variable is violent versus property crimes, and the time lag specified in the study), and its overall mean effect size is only moderate at best ($ADJz = .135$). When taken together, the mid-range mean effect size estimates and high levels of sensitivity to methodological conditions of the variables specified by routine activities theory leaves the perspective with a moderate level of empirical support across existing studies.

Theories with Weak Empirical Support

The remaining two macro-level theories of crime—rational choice/deterrence theory and subcultural theory—have received weak empirical support across existing studies. Both perspectives have been adequately tested by researchers to date, yet neither has enjoyed consistent empirical support. First, with the exception of the *incarceration effect*—which, again, could potentially be an incapacitation, as opposed to a deterrent, effect—the variables specified by macro-level rational choice/deterrence theory are among the most consistently weak predictors of crime revealed in the analyses presented in Chapter Five. Indeed, the mean effect size estimates for the *policing effect* and the *get tough policy effect* (both at $ADJz = -.054$) were substantively unimportant. Furthermore,

both the incarceration effect and the policing effect were significantly conditioned by the presence of variables from competing theories across statistical models. Perhaps even more damaging is the fact that the inclusion of controls for reciprocal effects consistently dampened the level of support for many of the propositions made by macro-level rational choice/deterrence theory (i.e., support for the theory is most likely to be revealed in studies that are the least rigorous methodologically). Thus, the combination of weak mean effect size estimates and considerable sensitivity to theoretically relevant methodological factors of the variables specified by rational choice/deterrence theory leaves it with weak overall empirical support.

Finally, macro-level subcultural theory has also received weak empirical support across existing studies. First, the mean effect size of *urbanism* on crime is moderate at best ($ADJz = .162$). What is more important is that this moderate mean effect size is significantly conditioned by nearly every methodological characteristic examined in Chapter Five and in Chapter Six. The *southern effect* on crime shares a similar fate: its mean effect size is only slightly greater than .10 (at $ADJz = .125$), and its effect on crime consistently “washes out” when variables from competing criminological theories are controlled across statistical models. When taken together, neither of the variables specified by macro-level subcultural theory were able to consistently predict aggregate levels of crime with any substantial magnitude across studies. In short, the mid-range to weak mean effect size estimates and extremely high levels of sensitivity to methodological conditions of the variables specified by subcultural theory leaves the perspective with a weak level of empirical support across existing studies.

Variables that Cut Across Theories

Meta-analysis is capable of establishing the relative predictive strength of a particular set of variables—in this case, the macro-level predictors of crime. As discussed in previous chapters of this dissertation, however, meta-analysis is not designed to settle the debates surrounding which theories may claim “ownership” over particular variables. Accordingly, many of the macro-level predictors of crime assessed in this study “cut across” multiple theories. As such, the implications of this issue for our understanding of the relative empirical status of the macro-level theories of crime warrant additional discussion.

Perhaps the most visible example of a variable that is shared by multiple criminological theories is *unemployment*. Consistent with the discussion contained in Chapter Three, researchers have used measures of unemployment as proxies of “guardianship” (routine activities theory) and economic hardship (absolute deprivation/conflict theory), as an indicator of a breakdown in the viability of community control/socialization (social disorganization theory), and even as a precursor to frustration-induced anger (anomie/strain theory). Adding to the potential confusion of the unemployment-crime (U-C) relationship is the fact that these theories often specify conflicting relationships between the two variables (i.e., positive and inverse relationships may be explained theoretically; see the discussion by Land et al., 1995). To help clarify this debate, the analyses contained in Chapter Six revealed that the U-C relationship is largely contingent on the time lag specified in a statistical model. In short, the U-C relationship is complex enough to accommodate the propositions made by multiple criminological theories. Longer time lags tend to support those theories

specifying a positive U-C relationship (e.g., absolute deprivation/conflict, social disorganization, and anomie/strain theories), whereas shorter time lags tend to support those theories specifying an inverse U-C relationship (e.g., routine activities theory).

The second set of macro-level predictors of crime that are shared by a number of criminological theories are those intended to proxy the effects of *economic deprivation* on crime. Aside from absolute deprivation/conflict and relative deprivation/inequality theories, both macro-level anomie/strain and social disorganization theories contend that economic deprivation—in some form—is criminogenic. Indeed, as social collectives experience economic hardships (such as poverty and/or economic inequality), the potential for higher levels of frustration to arise over such conditions may increase and therefore result in higher levels of crime as well (i.e., an anomie/strain explanation; see the discussion by Agnew, 1999). Assuming that this hypothesis is reasonable, and that measures of poverty accurately capture this phenomenon, the relatively strong and stable effect of poverty on crime may mean that macro-level anomie/strain theory has earned a considerable amount of empirical support across studies. Similarly, the social disorganization perspective explicitly specifies how economically deprived communities may lose the ability to effectively control and socialize their members which, in turn, may result in higher rates of crime and delinquency (Sampson and Wilson, 1995; Wilson, 1987). Thus, to the extent that a variable such as “poverty” is better able to proxy the effects of economic deprivation relative to traditional low SES measures, substituting the effects of poverty under the umbrella of social disorganization theory would afford the theory an even higher level of empirical support across studies.

Finally, in addition to the implications of “variable sharing” for the well established macro-level theories of crime, the issue is also relevant to certain theories that have only recently emerged. For example, Messner and Rosenfeld’s (1997b) test of their newly articulated institutional anomie theory (see Messner and Rosenfeld, 1994, 1997a) used what was referred to as a “decommodification index” that was made up of variables related to income-replacement public/governmental transfer payments to citizens of various types (e.g., social security expenditures, unemployment benefits, and family allowances). Although Messner and Rosenfeld (1997b:1397) treated this composite measure as an indicator of the level of “economic dominance” in particular institutional relationships, each of the constituent variables in the index could also be viewed as indicators of public efforts at *social support/altruism*. Assuming this interpretation is reasonable (i.e., that social security expenditures, unemployment benefits, and the like indicate “support”), Messner and Rosenfeld’s (1997b) study may have indirectly offered a degree of empirical validation for social support/altruism theory.

In any event, what is important to note is that it is not necessarily unreasonable and/or conceptually incorrect for multiple macro-level theories of crime to specify a common set of independent variables as predictors of crime. It should also be reiterated that the technique of meta-analysis cannot be the ultimate referee as theorists wrestle over the proper theoretical placement of variables such as “unemployment” or “economic deprivation.” What a meta-analysis can do, however, is provide precise estimates of the predictive strength of these relationships to crime. Armed with this knowledge, as criminological theorists proceed with their questions of “who gets what” with regard to

particular variables, perhaps the present meta-analysis will, at minimum, reveal to these theoretical stakeholders the “net worth” of what they are arguing over.

IMPLICATIONS FOR FUTURE RESEARCH

The analyses presented in Chapter Five and in Chapter Six of this dissertation have a number of implications for future empirical research. The first, and perhaps the most obvious, implication involves the specification of those macro-level predictors of crime that are consistently the most robust. In other words, future tests of macro-level criminological theories that fail to control for the five “strong and stable” predictors revealed in Chapter Five—or perhaps a composite of such factors (e.g., concentrated disadvantage)—would be vulnerable to the potential problem of model misspecification error.

In addition to revealing what *has* been done in terms of empirical tests of particular criminological theories, meta-analysis is therefore able to highlight what has *not* been done across such tests. Thus, another implication of the present study for future research is that “gaps” in the body of empirical literature have been exposed so that they may be addressed in future studies. Accordingly, researchers have yet to repeatedly examine the impact that estimating reciprocal effects may have on the outcome of tests of social disorganization theory (for an exception, see Bursik, 1986). Further, while Jackson (1983) raised the issue of the potential mediating effects of inequality between variables from routine activities theory and crime, researchers have neglected to follow her lead. Indeed, researchers may also wish to explore the mediating effects of variables from

some of the more recently developed theoretical perspectives. For example, are the effects of poverty and/or economic inequality on crime mediated by variables indicating social support or altruism? If so, which types of support are best able to do so (e.g., those from private versus public sources)? These types of questions have yet to be answered by empirical research.

The meta-analysis also revealed that certain macro-level theories of crime have been subjected to only a very few empirical tests. To be sure, macro-level anomie/strain theory has only recently been the focus of empirical studies following the revisions to the theory made by Messner and Rosenfeld (1994, 1997a). Furthermore, there are currently no published studies of the recent formulation of macro-level “general strain theory” (Agnew, 1999). In addition, certain portions of social disorganization theory—such as the dynamics of collective efficacy—have only been tested in limited contexts (e.g., only in a few American cities), and tests of social support/altruism theory have just begun to consistently emerge. This means that, although more than 200 macro-level studies of crime have been conducted and published to date, there is still a considerable amount of work that can and should be done in this area. For example, what effect do varying levels of aggregation and/or the inclusion of variables from competing criminological theories have on the level of support for institutional anomie theory, for macro-level general strain theory, or for social support/altruism theory? Also, can measures of collective efficacy consistently mediate the effects of economic deprivation, or of concentrated disadvantage? These types of questions have yet to be answered by empirical research to date.

Finally, it may also be useful to discuss what the future of the technique of meta-analysis may be in the area of criminological theory in general and in macro-level criminological theory in particular. There are essentially three components to this issue. First, researchers should continue to use the method to systematically organize empirical tests of criminological theories. This can be done, in one sense, by replicating existing meta-analyses over time to reassess particular relationships between variables. Perhaps even more importantly, researchers can also undertake other independent meta-analyses, using alternative methodological approaches⁹² (e.g., synthesizing different effect size estimates, examining the impact of a different set of methodological characteristics, and so on). Doing so may help to determine an overall level of consistency and consensus regarding what we do, and do not, know about the empirical status of the major micro-level and macro-level criminological theories.

Second, meta-analysts in the future may wish to focus more explicitly on how methodological variations can influence the level of support for certain empirical relationships. The meta-analytic technique is particularly well suited for this endeavor (i.e., examining the conditioning effects of methodological characteristics across studies). A focus on the conditioning effects of methodological variations may give researchers a better understanding of which types of measurement and/or methodological techniques may be most appropriate for future studies. Further, a clearer picture may emerge regarding whether certain methodological approaches are more or less likely to produce a particular empirical outcome. A more consistent focus on the conditioning effects of methodological factors may also help meta-analysts in their quest to shed the lingering

⁹² An example of how multiple independent meta-analyses can help to inform one another can be seen in the meta-analyses (and re-analyses) by Whitehead and Lab (1989) and by Andrews et al. (1990).

sentiment among certain critics that their chosen approach is methodologically “soft” (e.g., see the discussion by Pratt and Holsinger, 1999).

Finally, consistent with the themes discussed in this section, meta-analysts in the future should make every effort possible to frame their analyses in a way that helps to promote and to guide future primary research (i.e., additional independent empirical studies). In other words, a meta-analysis should not be viewed as the “final word” on a particular research hypothesis. Such reviews should instead illuminate what we know about the relationship under investigation and, just as importantly, highlight what we do not know about the relationship (i.e., what still needs to be done?).

LIMITATIONS OF THE RESEARCH

A full discussion of the more general problems that may be associated with the meta-analytic technique was provided in Chapter Two. Nevertheless, it is necessary to note that there are two additional limitations to the present study. First, as outlined above, the technique of meta-analysis cannot settle debates concerning which variables, or predictors of crime, should fall under which theoretical heading (e.g., unemployment, poverty). As stated previously, this is, by definition, a theoretical debate for which meta-analytic methodology—as a “tool”—cannot be the ultimate judge. Again, what meta-analysis can do, however, is provide researchers with accurate estimates of the effect size of the relationships under question.

The second limitation to the meta-analysis conducted in this dissertation (as also with any meta-analysis) is that it cannot make any definitive conclusions regarding the construct validity of the relationships being investigated. In other words, the meta-analytic technique has nothing to say about whether certain variables are in fact

measuring what the researcher says they are measuring. For example, do the measures comprising the household activity ratio (female labor force participation and “primary” households divided by the total number of households) really indicate levels of attenuated guardianship? Does a “southern region” dummy variable accurately proxy southern culture? Does the “decommodification index” indicate the strength of noneconomic institutions, social support/altruism, or neither? Does the incarceration effect indicate a deterrent or an incapacitation dynamic? Similar to the issue of the theoretical “ownership” of certain macro-level predictors of crime, the debate surrounding the construct validity of such measures is also inherently theoretical. As such, a meta-analysis cannot necessarily cast the deciding vote.

Accordingly, there is a certain risk when conducting a meta-analysis that a “garbage-in-garbage-out” phenomenon may occur. Indeed, how certain can we be of what the meta-analysis tells us about a particular criminological theory’s empirical status if the measures used by researchers to proxy the theory’s key concepts across studies are consistently inappropriate? Fortunately, the discussion contained in Chapter Four and the analyses presented in Chapter Five explicitly indicate which variables have been used by researchers to measure key theoretical constructs. The present meta-analysis is therefore able to reveal to researchers the specific variables that have been employed across tests of particular macro-level criminological theories and what their relative effects on crime are. Given this information, researchers may question the conclusions drawn in this chapter regarding the relative empirical status of the major macro-level theories of crime (e.g., is the lack of empirical support for the southern subculture of violence thesis due to conceptual flaws or to invalid measures of southern “culture”?). If so, such skeptics can

then assess for themselves whether a particular theory is, or is not, well supported across existing tests with the information contained in Chapter Five.

POLICY IMPLICATIONS OF THE RESEARCH

A number of criminologists have commented on the apparent ideological gulf that exists between academic researchers and public policymakers (see, e.g., Cressey, 1978; Currie, 1998; Gottfredson, 1982). Policy analysts have traditionally explained this chasm of communication in terms of the indifference of academic researchers to the political constituencies to which policymakers are devoted (Allison, 1971; see also Weimer and Vining, 1992). While this explanation may be somewhat over-simplified (Beckett, 1997), it seems to contain a nugget of truth. To be sure, criminologists have consistently criticized the utility of the “get tough” movement in criminal justice since the late 1960s and early 1970s (Clear, 1994; Gilsanin, 1991)—with all of its punitive policy trappings—only to have their warnings dismissed by policymakers as being politically unrealistic given the pervasive conservative culture of the American citizenry (Gordon, 1990). The constant threat of having one’s policy proposals ridiculed, thwarted, or otherwise ignored by policymakers has even caused some of the more influential criminologists to simply withdraw from the public policy battlefield altogether, and instead advocate a retreat back into the safety of the ivory tower (Cressey, 1978).

Even so, it is important to note that academic research is not *incapable* of influencing the direction of criminal justice policy. Indeed, since the early 1970s, political pundits have often heeded the advice of certain “criminologists” that have

advocated the adoption of “get tough policies” while at the same time condemning the more progressive crime control policy agenda traditionally championed by social scientists (for examples of such questionable, yet influential, “criminology,” see DiIulio, 1994; Murray and Cox, 1979; Murray, 1984; Wilson, 1983). Given the level of influence afforded to these “researchers” in recent years, it therefore appears as though the inability of criminologists as a whole to re-route the punitive direction that criminal justice policy has taken over the last few decades may not simply be attributable to policymakers’ unwillingness to listen to the message being touted by academics. Perhaps a more troubling contribution to the “knowledge gap” in this context has to do with an inability on the part of the majority of criminologists to “speak truth to power” (Wildavsky, 1979:15) in a language that policymakers can both understand and not feel as though their public life would be threatened should they choose to follow the advice of the researcher/policy analyst (Cullen, Wright, and Chamlin, 1999; see also Bobrow and Dryzek, 1987; Kingdon, 1995; Stone, 1988).

With this task in mind, this section provides an overview of the major policy implications of the research contained in this dissertation. Although this research was not necessarily intended to serve as a policy guide, the prospect that it may inadvertently be used as one by policymakers in the future compels a discussion of the broad types of policy agendas that are, and are not, likely to reduce aggregate levels of crime. To make this discussion as accessible as possible to a non-academic audience, hints and references to complex methodological intricacies across empirical studies will be left aside in favor of more general conclusions regarding what the present research tells us about our prospects for reducing crime through social/public policies.

Get Tough Policies: An Expensive Exercise in Futility

The most obvious implication of the present research for crime control policy is that lawmakers should understand the relative futility of continued efforts to reduce crime through focusing on criminal justice system dynamics alone. To be sure, with the possible exception of the effect of incarceration, variables related to the criminal justice system—including those measuring the crime control capacity of the police and of “get tough” policies—were consistently among the weakest macro-level predictors of crime. Should policymakers choose to ignore this empirical reality and therefore fail to deviate from the repressive crime control policy agenda that has characterized the political landscape in the U.S. in recent years, the attention of lawmakers (and the public) may unfortunately be diverted away from other policy arenas that actually show promise for reducing crime (e.g., social support/altruism policies, as will be discussed below).

Furthermore, as stated previously in this chapter, even the relatively large effect size of incarceration on crime—a finding over which a number of scholars are likely to be inordinately (yet prematurely) thrilled—may be potentially misleading. Indeed, a number of researchers have noted that high levels of incarceration may indirectly increase crime rates by increasing instances of family disruption (i.e., the removal of a family member from the home) and by imposing upon families a greater degree of economic deprivation brought on by the loss of an additional (or perhaps the only) income (see, e.g., Tonry 1995; Wilson, 1987, 1996). Again, as stated above, this potential threat is heightened given the disproportionately high incarceration rates among members of racial/ethnic minority groups (Donziger, 1996).

In addition to being a fine example of an impotent set of crime control policies, the “get tough” policy experiment in the U.S. has also been terribly expensive. As an illustration of the fiscal lunacy of the get tough crime control policy agenda, Currie (1998) notes that California currently devotes a greater portion of its state budget to the department of corrections than it does to its colleges and universities. Accordingly, Currie (1998:21) goes on to note that: “Short of major wars, mass incarceration has been the most thoroughly implemented government social program of our time.” Should policymakers in the future decide, however, that allowing *empirical data* to filter into their decisions regarding “what to do” about the “crime problem” is permissible, perhaps the present research will help to point them in a more reasonable direction than has been taken in recent years.

Tackling Concentrated Disadvantage: A Progressive Agenda

What, then, does the meta-analysis presented in this dissertation have to say about effective crime control policy? In short, to the extent that a construct such as “concentrated disadvantage” is a major key for our understanding of crime at the macro-level, those policies that are aimed at ameliorating the effects of economic deprivation and family disruption—especially in ecological contexts with large populations of racial minorities—are likely to have a significant impact on crime reduction. These policies could come in the form of social support/altruism efforts on the part of public or private entities to help families stay stable in the face of economically disadvantaged communities, such as early intervention, welfare, health care, and educational programs (Cullen et al., 1999; Currie, 1985). Some scholars have also suggested that directed efforts at job creation, systematic efforts to upgrade working wages, and greater support

for labor organization in macro-social units characterized by concentrated disadvantage may help to reduce crime (Currie, 1996; Wilson, 1987, 1996). Nevertheless, underlying each of these policy proposals is the recognition that levels of economic deprivation in general among social collectives need to be reduced as a precursor to the more specific, community-level policy initiatives (Harrington, 1984).

Perhaps the most fundamental policy implication of the present research has to do with the recognition that effective crime control policies must extend beyond the walls (literally and figuratively) of the criminal justice system. It is easy for both the public and policymakers to assume that each policy arena—from welfare to education, and from health care to economics—is an island unto itself that does not affect, and is not affected by, the others (e.g., see the discussion by Galbraith, 1996). The results of the research contained in this dissertation indicate otherwise. Indeed, reasonable and effective crime control policies are more likely to be articulated once public policymakers embrace the notion that the decisions they make in one policy domain have repercussions for the others. Accordingly, adopting a more progressive crime control policy agenda—one that specifically targets the multiplicity of negative effects associated with “concentrated disadvantage”—is much more likely to result in a substantial reduction in crime relative to the policies flowing from the empirically bankrupt get-tough agenda that has dominated criminal justice policy over the last few decades.

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Table 5.1. Rank ordered mean effect size estimates of macro-level predictors of crime.

Rank	Macro-Level Predictor	Rank	Macro-Level Predictor
1	Strength of noneconomic institutions	17	Residential mobility
2	Unemployment (length considered)	18	Unemployment (with age restriction)
3	Firearms ownership	19	Age effects
4	Percent non-white	20	Southern effect
5	Incarceration effect	21	Unemployment (no length considered)
6	Collective efficacy	22	Socioeconomic status
7	Percent black	23	Arrest ratio
8	Religion effect	24	Unemployment (no age restriction)
9	Family disruption	25	Sex ratio
10	Poverty	26	Structural density
11	Unsupervised local peer groups	27	Police expenditures
12	Household activity ratio	28	Get tough policy
13	Social support/altruism	29	Education effects
14	Inequality	30	Police per capita
15	Racial heterogeneity index	31	Police size
16	Urbanism		

Rank ordering is based on the independence-adjusted mean effect size estimates (values are presented in Table 2).

Table 5.2. Overall, sample size-weighted, and independence-adjusted mean effect size estimates for macro-level predictors rank-ordered by predictor domains.

Predictor Domain	Mz	% Sig.	WMz	ADJz	95% CI
1. Strength of Noneconomic Institutions (4)	-.391*	100.00	-.391	-.391	-.439-(-.343)
2. Firearms Ownership (4)	.350*	100.00	.332	.350	.159-.533
3. Incarceration Effect (46)	-.332	58.70	-.227	-.317	-.444-(-.201)
4. Collective Efficacy (13)	-.303*	100.00	-.277	-.315	-.351-(-.255)
5. Racial Heterogeneity (265)	.293†	72.83	.218†	.288†	.278-.308
Percent Non-white (72)	.328	76.38	.235	.330	.261-.395
Percent Black (162)	.295	72.22	.209	.294	.259-.332
Heterogeneity Index (31)	.198	67.74	.236	.187	.131-.323
6. Religion Effect (11)	-.278*	72.73	-.260	-.274	-.397-(-.160)
7. Family Disruption (137)	.261†	71.53	.152†	.262	.240-.282
Percent Divorced (98)	.229	70.40	.216		.189-.269
Single-Headed Households (15)	.448	80.00	.323		.258-.637
Female-Headed Households (22)	.275	68.18	.088		.107-.434
8. Poverty (153)	.250†	58.82	.234†	.253	.230-.270
Percent Poverty (101)	.213	60.39	.177		.166-.261
Poverty Index (52)	.320	55.76	.419		.249-.391
9. Unsupervised Local Peer Groups (7)	.251*	100.00	.251	.251	.216-.268
10. Household Activity Ratio (37)	.242	59.46	.149	.228	.148-.336
11. Social Support/Altruism (47)	-.219†	74.47	-.156†	-.216	-.291-(-.148)
Private Source (12)	-.350*	83.33	-.232		-.451-(-.249)
Public Source (35)	-.174*	71.41	-.150		-.261-(-.087)
12. Inequality (167)	.212†	55.09	.170†	.207	.176-.249
No Race Component (124)	.235	54.83	.190		.190-.282
Race Component (43)	.145	55.81	.122		.102-.249

Rank ordering is based on independence-adjusted mean effect size estimates.

Number of contributing effect size estimates reported in parentheses.

Mz = Mean z-score for effect size estimates.

% Sig. = Percent of effect size estimates statistically significant in original model ($p < .05$).

WMz = Mean effect size estimate weighted by sample size.

ADJz = Mean effect size estimate weighted for independence correction.

* = Fail-Safe N estimate is less than 20.

† = Mean effect size estimate is significantly conditioned by measurement technique; separate mean effect size estimates reported.

Table 5.2. Continued.

Predictor Domain	Mz	% Sig.	WMz	ADJz	95% CI
13. Urbanism (178)	.163	53.37	.243	.162	.137-.189
14. Residential Mobility (52)	.171	55.77	.102	.150	.136-.206
15. Unemployment (204)	.138†	44.12	.103†	.135†	.090-.186
No Age Restriction (158)	.010*	41.14	.077	.096	.043-.156
Age Restriction (46)	.283	55.81	.247	.279	.197-.368
No Length Considered (191)	.124	41.88	.094	.121	.075-.174
Length Considered (13)	.412*	90.00	.308	.387	.166-.657
16. Age Effects (138)	.135	48.81	.081	.131	.098-.172
17. Southern Effect (110)	.118	35.45	.072	.125	.077-.160
18. Socioeconomic Status (101)	-.127	41.58	-.097	-.118	-.150(-.104)
19. Sex Ratio (34)	.095*	35.29	.078	.094	.028-.162
20. Structural Density (94)	.083	42.55	.079	.087	.039-.127
21. Get Tough Policy (37)	-.053*	40.54	-.031	-.054	-.101(-.057)
22. Policing Effect (114)	-.056†	42.11	-.093†	-.054†	-.100(-.013)
Police Size (3)	.200*	66.67	.279	.199	-.251-.650
Arrest Ratio (77)	-.107	37.66	-.054	-.106	-.151(-.063)
Police Per Capita (20)	.113*	65.00	.107	.098	-.033-.260
Police Expenditures (13)	.099*	23.08	-.066	-.082	-.209-.035
23. Education Effects (31)	.035*	51.61	-.029	.025	-.048-.118

Number of contributing effect size estimates reported in parentheses.

Mz = Mean z-score for effect size estimates.

% Sig. = Percent of effect size estimates statistically significant in original model ($p < .05$).

WMz = Mean effect size estimate weighted by sample size.

ADJz = Mean effect size estimate weighted for independence correction.

* = Fail-Safe N estimate is less than 20.

† = Mean effect size estimate is significantly conditioned by measurement technique; separate mean effect size estimates reported.

Table 5.3. Rank ordering of independence-adjusted mean effect size estimates by dependent variable.

Macro-Level Predictor	Overall ADJz	Rank	Violent Crime	Rank	Property Crime	Rank
Strength of noneconomic institutions	-.391	1	-.407	3	-.386	1
Unemployment (length considered)	.387	2	.687	1	.285	4
Firearms ownership	.350	3	.346	5	--	--
Percent non-white	.330	4	.374	4	.215	15
Incarceration effect	-.317	5	-.421	2	-.326	3
Collective efficacy	-.315	6	-.333	6	-.284	5
Percent black	.294	7	.321	7	.259	9
Religion effect	-.274	8	-.127	19	-.365	2
Family disruption	.262	9	.257	9	.279	6
Poverty	.253	10	.255	10	.217	14
Unsupervised local peer groups	.251	11	.182	15	.236	12
Household activity ratio	.228	12	.206	12	.278	7
Social support/altruism	-.216	13	-.223	11	-.223	13
Inequality	.207	14	.198	13	.243	11
Racial heterogeneity index	.187	15	.186	14	.211	16
Urbanism	.162	16	.144	16	.172	17

Table 5.3. Continued.

Macro-Level Predictor	Overall ADJz	Rank	Violent Crime	Rank	Property Crime	Rank
Residential mobility	.150	17	.076	24	.254	10
Unemployment (with age restriction)	.279	18	.292	8	.265	8
Age effects	.131	19	.140	17	.132	19
Southern effect	.125	20	.136	18	.065	24
Unemployment (no length considered)	.121	21	.087	23	.149	18
Socioeconomic status	-.118	22	-.115	20	-.104	21
Arrest ratio	-.106	23	-.112	22	-.085	23
Unemployment (no age restriction)	.096	24	.064	27	.128	20
Sex ratio	.094	25	.113	21	.064	25
Structural density	.087	26	.072	25	.096	22
Police expenditures	-.082	27	-.026	29	.099	28
Get tough policy	-.054	28	-.055	28	.034	27
Education effects	.025	29	.072	26	-.037	26
Police per capita	.098	30	.052	30	.112	29
Police size	.199	31	.354	31	.248	30
Rank-Order Correlation = .822 (P<.001)						

Table 5.4. Rank ordering of independence-adjusted mean effect size estimates by level of aggregation.

Macro-Level Predictor	Block-Census Tract	Rank	City-SMSA	Rank	State	Rank	Nation	Rank
Strength of noneconomic institutions	--	--	--	--	-.386	5	-.407	5
Unemployment (length considered)	.300	5	.167	10	--	--	.531	1
Firearms ownership	--	--	.249	5	--	--	.443	3
Percent non-white	.316	3	.329	1	.344	8	.211	15
Incarceration effect	--	--	-.105	16	-.354	7	-.392	6
Collective efficacy	-.303	4	--	--	--	--	--	--
Percent black	.249	7	.317	2	.188	13	.236	14
Religion effect	--	--	-.197	7	-.421	2	--	--
Family disruption	.242	9	.282	3	.356	6	.167	18
Poverty	.347	2	.187	8	.406	3	.354	9
Unsupervised local peer groups	.251	6	--	--	--	--	--	--
Household activity ratio	--	--	.109	14	--	--	.487	2
Social support/altruism	--	--	-.147	13	-.057	19	-.367	8
Inequality	.120	17	.180	9	.161	15	.417	4
Racial heterogeneity index	.186	10	.273	4	--	--	.171	17
Urbanism	.125	15	.163	11	.398	4	.099	21

Table 5.4. Continued.

Macro-Level Predictor	Block-Census Tract	Rank	City-SMSA	Rank	State	Rank	Nation	Rank
Residential mobility	.096	19	.232	6	.477	1	--	--
Unemployment (with age restriction)	.245	8	.159	12	.279	10	.330	10
Age effects	.181	11	.036	26	.167	14	.252	12
Southern effect	.351	1	.106	15	.226	12	.064	24
Unemployment (no length considered)	.096	20	.101	17	--	--	.144	19
Socioeconomic status	-.078	21	-.092	21	-.289	9	-.247	13
Arrest ratio	-.159	14	-.093	20	.045	22	-.207	16
Unemployment (no age restriction)	.072	23	.097	19	.133	16	.125	20
Sex ratio	.111	18	.099	18	-.079	18	.094	22
Structural density	.073	22	.084	22	.249	11	.023	26
Police expenditures	--	--	-.064	23	--	--	-.369	7
Get tough policy	--	--	-.056	24	-.101	17	-.025	25
Education effects	.125	16	-.056	25	-.022	21	.093	23
Police per capita	-.172	13	.183	27	-.044	20	-.265	11
Police size	--	--	.197	28	--	--	--	--

Rank-Order Correlation = .896 (p<.01)

Table 5.5. Rank ordering of independence-adjusted mean effect size estimates by time dimension.

Macro-Level Predictor	Cross Section		Longitudinal	
		Rank		Rank
Strength of noneconomic institutions	-.391	1	--	--
Unemployment (length considered)	.242	12	.496	2
Firearms ownership	.346	2	--	--
Percent non-white	.306	3	.447	4
Incarceration effect	-.282	6	-.469	3
Collective efficacy	-.303	4	--	--
Percent black	.299	5	.217	12
Religion effect	-.279	8	--	--
Family disruption	.279	7	.153	14
Poverty	.251	9	.114	18
Unsupervised local peer groups	.251	10	--	--
Household activity ratio	.099	24	.629	1
Social support/altruism	-.160	18	-.337	7
Inequality	.207	13	.280	9
Racial heterogeneity index	.195	14	.127	16
Urbanism	.169	15	.094	20

Table 5.5. Continued.

Macro-Level Predictor	Cross Section		Longitudinal	
		Rank		Rank
Residential mobility	.166	17	.442	5
Unemployment (with age restriction)	.248	11	.329	8
Age effects	.101	23	.259	10
Southern effect	.118	20	.123	17
Unemployment (no length considered)	.116	21	.133	15
Socioeconomic status	-.127	19	-.175	13
Arrest ratio	-.167	16	-.061	22
Unemployment (no age restriction)	.090	25	.111	19
Sex ratio	.064	27	.238	11
Structural density	.083	26	--	--
Police expenditures	-.062	28	-.369	6
Get tough policy	-.108	22	-.023	24
Education effects	.046	29	-.064	21
Police per capita	.162	30	-.057	23
Police size	.200	31	--	--

Rank-Order Correlation =
-.417 (p<.05)

Table 5.6. Rank ordering of independence-adjusted mean effect size estimates by juvenile versus adult offenses.

Macro-Level Predictor	Juvenile	Rank	Adult	Rank
Strength of noneconomic institutions	--	--	-.391	1
Unemployment (length considered)	--	--	.387	2
Firearms ownership	--	--	.346	3
Percent non-white	.457	3	.305	5
Incarceration effect	--	--	-.322	4
Collective efficacy	--	--	-.303	6
Percent black	.032	12	.296	7
Religion effect	--	--	-.279	8
Family disruption	.239	6	.261	10
Poverty	.557	1	.243	12
Unsupervised local peer groups	--	--	.251	11
Household activity ratio	--	--	.241	13
Social support/altruism	--	--	-.219	14
Inequality	--	--	.212	15
Racial heterogeneity index	.040	11	.193	16
Urbanism	.141	9	.163	18

Table 5.6. Continued.

Macro-Level Predictor	Juvenile	Rank	Adult	Rank
Residential mobility	.299	5	.163	17
Unemployment (with age restriction)	.341	4	.262	9
Age effects	--	--	.135	19
Southern effect	.237	7	.117	21
Unemployment (no length considered)	--	--	.109	22
Socioeconomic status	-.072	10	-.131	20
Arrest ratio	--	--	-.106	23
Unemployment (no age restriction)	.149	8	.095	24
Sex ratio	--	--	.095	25
Structural density	--	--	.083	26
Police expenditures	--	--	-.082	27
Get tough policy	--	--	-.053	29
Education effects	-.553	2	.055	28
Police per capita	--	--	.098	30
Police size	--	--	.200	31

Rank-Order Correlation = .034 (p>.05)

Table 5.7. Summary of strength and consistency of independence-adjusted mean effect size estimates across various specifications.

Macro-Level Predictor	Strength			Stability		
	High	Moderate	Low	High	Moderate	Low
Strength of noneconomic institutions	X					X
Unemployment (length considered)	X					X
Firearms ownership	X					X
Percent non-white	X			X		
Incarceration effect	X			X		
Collective efficacy	X					X
Percent black	X			X		
Religion effect	X					X
Family disruption	X			X		
Poverty	X			X		
Unsupervised local peer groups	X					X
Household activity ratio		X			X	
Social support/altruism		X			X	
Inequality		X			X	
Racial heterogeneity index		X			X	
Urbanism		X			X	

Table 5.7. Continued.

Macro-Level Predictor	Strength			Stability		
	High	Moderate	Low	High	Moderate	Low
Residential mobility		X			X	
Unemployment (with age restriction)		X				X
Age effects			X		X	
Southern effect		X			X	
Unemployment (no length considered)			X			X
Socioeconomic status			X			X
Arrest ratio		X				X
Unemployment (no age restriction)			X		X	
Sex ratio			X			X
Structural density			X			X
Police expenditures			X			X
Get tough policy			X			X
Education effects			X			X
Police per capita			X			X
Police size			X			X

Table 6.1. Summary of effect size estimates for variables specified by social disorganization theory.

Predictor Domain	Mean ADJz	Overall Rank	Mean Rank Across Specifications
Racial Heterogeneity	.288	5	9.17
Socioeconomic Status	-.118	18	17.18
Residential Mobility	.150	14	12.10
Family Disruption	.262	7	8.82
Collective Efficacy	-.315	4	5.17
Unsupervised Local Peer Groups	.251	9	10.83

Table 6.2. General and theory-specific WLS regression models of the influences of methodological variations on the mean effect size estimates of variables specified by social disorganization theory.

Methodological Characteristic	Racial	Racial	SES	SES
	Heterogeneity	Heterogeneity	Model 1	Model 2
	y			
	Model 1			
Level of aggregation	-.053a (-.058)	-.023a (-.025)	-.032* (-2.486)	-.032* (-2.599)
U.S. origin	.264** (3.788)	.268** (3.821)	.075 (1.027)	.083 (1.127)
Variables from competing theories controlled	-.113* (-2.264)	-.112* (-2.242)	-.014 (-.203)	-.022 (-.321)
Time dimension (1 = longitudinal analysis)	.015 (.297)	.013 (.264)	-.041 (-.356)	-.044 (-.371)
Ratio of sample size to independent variables	-.063a* (-2.189)	-.063a* (-2.194)	-.030a (-.632)	-.031a (-.646)
Violent crime dependent variable	.070* (2.234)	.068* (2.151)	.036 (.772)	.037 (.796)
Race-specific model (1 = yes)	--	-.016 (-.194)	--	.116 (1.251)
Reciprocal effects estimated (1 = not estimated)	--	.045 (.803)	--	.056 (.846)
Constant	.116	.072	-.101	-.152
Model R ²	.087**	.089**	.067	.094
Sample size	265	265	101	101

a = coefficient multiplied by 1000 for ease of presentation.

Regression coefficients are metric estimates (weighted according to the fixed-effects correction for independence), and t-values are reported in parenthesis.

* = p<.05

** = p<.01

Table 6.3. General and theory-specific WLS regression models of the influences of methodological variations on the mean effect size estimates of variables specified by social disorganization theory.

Methodological Characteristic	Residential	Residential	Family	Family
	Mobility	Mobility	Disruption	Disruption
	Model 1			
	Model 1	Model 2	Model 1	Model 2
Level of aggregation	.094** (4.271)	.092** (4.126)	.001 (.112)	.003 (.218)
U.S. origin	.021 (.203)	.018 (.173)	.155* (2.016)	.156* (2.034)
Variables from competing theories controlled	-.015 (-.155)	-.006 (-.056)	-.030 (-.416)	-.028 (.700)
Time dimension (1 = longitudinal analysis)	.339† (1.848)	.335† (1.812)	-.113 (-1.500)	-.118 (-1.555)
Ratio of sample size to independent variables	-.004a (-.042)	-.003a (-.030)	-.081a* (-2.383)	-.088a* (-2.552)
Violent crime dependent variable	-.204** (-3.366)	-.196** (-3.161)	-.008 (-.197)	-.006 (-.859)
Race-specific model (1 = yes)	--	--	--	-.097 (-1.215)
Reciprocal effects estimated (1 = not estimated)	--	.155 (.497)	--	-.126 (-.859)
Constant	.097	-.061	.195**	.320*
Model R ²	.453**	.459**	.110*	.126*
Sample size	52	52	137	137

a = coefficient multiplied by 1000 for ease of presentation.

Regression coefficients are metric estimates (weighted according to the fixed-effects correction for independence), and t-values are reported in parenthesis.

* = p<.05

** = p<.01

† = p<.10

Table 6.4. Summary of effect size estimates for variables specified by anomie/strain theory.

Predictor Domain	Mean ADJz	Overall Rank	Mean Rank Across Specifications
Strength of noneconomic institutions	-.391	1	2.43

Table 6.5. Summary of effect size estimates for variables specified by absolute deprivation/conflict and relative deprivation/inequality theories.

Predictor Domain	Mean ADJz	Overall Rank	Mean Rank Across Specifications
Poverty	.253	8	8.73
Inequality	.207	12	12.00

Table 6.6. General and theory-specific WLS regression models of the influences of methodological variations on the mean effect size estimates of variables specified by absolute deprivation/conflict and relative deprivation/inequality theories.

Methodological Characteristic	Poverty Model 1	Poverty Model 2	Inequality Model 1	Inequality Model 2
Level of aggregation	-.010 (-.744)	-.009 (-.745)	.055** (4.596)	.056** (4.702)
U.S. origin	.107 (1.024)	.104 (.988)	.047a (.007)	-.013 (-.205)
Variables from competing theories controlled	-.101 (-1.299)	-.098 (-1.252)	.035 (.448)	.056 (.698)
Time dimension (1 = longitudinal analysis)	-.135 (-.694)	-.137 (-.700)	.085 (1.002)	.064 (.747)
Ratio of sample size to independent variables	.013a (.144)	.010a (.108)	-.003* (-2.578)	-.003* (-2.578)
Violent crime dependent variable	.073a (.017)	.002 (.056)	-.064† (-1.787)	-.061† (-1.695)
Controls for inequality included	--	-.016 (-.287)	--	--
Controls for poverty included	--	--	--	-.050 (-1.303)
Constant	.270*	.273*	.084	.086
Model R ²	.034	.035	.168**	.177**
Sample size	153	153	167	167

a = coefficient multiplied by 1000 for ease of presentation.

Regression coefficients are metric estimates (weighted according to the fixed-effects correction for independence), and t-values are reported in parenthesis.

* = p<.05

** = p<.01

† = p<.10

Table 6.7. Summary of effect size estimates for variables specified by routine activities theory.

Predictor Domain	Mean ADJz	Overall Rank	Mean Rank Across Specifications
Household activity ratio	.228	10	10.63
Unemployment	.135	15	14.50

Table 6.8. General and theory-specific WLS regression models of the influences of methodological variations on the mean effect size estimates of variables specified by routine activities theory (household activity ratio—HHR—and unemployment).

Methodological Characteristic	HHR Model 1	HHR Model 2	Unemp. Model 1	Unemp. Model 2
Level of aggregation	.008 (.234)	-.018 (-.465)	.007 (.488)	.009 (.603)
U.S. origin	--	--	-.045 (-.565)	-.047 (-.589)
Variables from competing theories controlled	.157† (1.873)	.227* (2.260)	.150* (2.360)	.146* (2.216)
Time dimension (1 = longitudinal analysis)	.618** (4.785)	.680** (4.945)	.093 (1.523)	.058 (.770)
Ratio of sample size to independent variables	.001 (.546)	.001 (.520)	-.017a (-.291)	-.011a (-.184)
Violent crime dependent variable	-.032 (-.586)	-.027 (-.500)	-.082† (-1.733)	-.080† (-1.694)
Controls for inequality included	--	-.104 (-1.241)	--	-.016 (-.233)
Time lag (in months)	--	--	--	.002 (.791)
Constant	-.066	.008	.054	.054
Model R ²	.708**	.722**	.045	.049
Sample size	37	37	204	204

a = coefficient multiplied by 1000 for ease of presentation.

Regression coefficients are metric estimates (weighted according to the fixed-effects correction for independence), and t-values are reported in parenthesis.

* = p<.05

** = p<.01

† = p<.10

Table 6.9. Logistic regression models predicting significant-positive and significant-negative unemployment effects.

Methodological Characteristic	Unemployment (significant-positive)		Unemployment (significant-negative)	
	Coeff.	SE	Coeff.	SE
Level of aggregation	.021	.099	.093	.247
U.S. origin	-1.272*	.563	.729	1.311
Variables from competing theories controlled	.278	.422	-2.149*	.892
Time dimension (1 = longitudinal analysis)	-1.359	.945	2.974†	1.609
Ratio of sample size to independent variables	.002	.004	-.039*	.018
Violent crime dependent variable	-1.135**	.325	.238	.768
Time lag (in months)	.154*	.074	-.269*	.109
Constant		.743		-1.901
-2 Log likelihood		245.066**		65.418**
Sample size		204		204

Regression coefficients are metric estimates.

* = $p < .05$

** = $p < .01$

† = $p < .10$

Table 6.10. Summary of effect size estimates for variables specified by deterrence theory.

Predictor Domain	Mean ADJz	Overall Rank	Mean Rank Across Specifications
Incarceration effect	-.317	3	5.77
Policing effect	-.054	22	27.75
Get tough policy effect	-.054	21	24.89

Table 6.11. General and theory-specific WLS regression models of the influences of methodological variations on the mean effect size estimates of variables specified by deterrence theory.

Methodological Characteristic	Incarceration Model 1	Incarceration Model 2	Police Model 1	Police Model 2
Level of aggregation	-.101* (-2.271)	-.086† (-1.992)	-.021 (-1.341)	.007 (.435)
U.S. origin	-.457* (-2.338)	-.424* (-2.268)	.122 (1.154)	.091 (.877)
Variables from competing theories controlled	-.059 (-.357)	-.271 (-1.519)	-.036 (-.576)	-.163* (-2.305)
Time dimension (1 = longitudinal analysis)	-.029 (-.179)	.051 (.323)	-.044 (-.789)	-.039 (-.696)
Ratio of sample size to independent variables	-.005a (-.015)	.013a (.044)	.005a (.209)	-.001a (-.508)
Violent crime dependent variable	-.122 (-1.040)	-.107 (-.963)	.003 (.065)	.036 (.742)
Reciprocal effects estimated (1 = not estimated)	--	.004a (.001)	--	-.075 (-1.586)
Controls for routine activities theory	--	.358** (2.802)	--	--
Identification restriction used (1 = not used)	--	--	--	-.181* (-2.429)
Policing measure (1 = arrest ratio)	--	--	--	-.213** (-4.420)
Constant	.667	.545	-.067	.304†
Model R ²	.236†	.371*	.046	.230**
Sample size	46	46	114	114

a = coefficient multiplied by 1000 for ease of presentation.

Regression coefficients are metric estimates (weighted according to the fixed-effects correction for independence), and t-values are reported in parenthesis.

* = p<.05

** = p<.01

† = p<.10

Table 6.12. Summary of effect size estimates for variables specified by social support/altruism theory.

Predictor Domain	Mean ADJz	Overall Rank	Mean Rank Across Specifications
Social support/altruism	-.216	11	12.89

Table 6.13. General and theory-specific WLS regression models of the influences of methodological variations on the mean effect size estimates of variables specified by social support/altruism theory.

Methodological Characteristic	Model 1	Model 2	Model 3	Model 4
Level of aggregation	-.055 (-1.583)	.058 (.825)	-.019 (-.493)	-.033 (-.876)
U.S. origin	-.103 (-.373)	.085 (.293)	-.251 (-.916)	-.160 (-.595)
Variables from competing theories controlled	.247 (.946)	.463 (1.536)	.614† (1.928)	.366 (1.311)
Time dimension (1 = longitudinal analysis)	.043 (.315)	-.424 (-1.464)	-.057 (-.385)	-.082 (-.534)
Ratio of sample size to independent variables	-.047a (-.206)	.003 (1.040)	-.001a (-.006)	.002 (.688)
Violent crime dependent variable	-.074 (-1.032)	-.106 (-1.453)	-.035 (-.478)	-.087 (-1.233)
Social support measure (1 = private)	--	.270 (1.354)	.176 (1.144)	-.034 (-.257)
Controls for absolute deprivation theory included	--	-.312† (-1.873)	--	--
Controls for relative deprivation theory included	--	--	-.244* (-2.259)	--
Controls for routine activities theory included	--	--	--	-.184* (-2.175)
Constant	-.077	-.714	-.386	-.155
Model R ²	.276*	.338*	.362*	.357*
Sample size	47	47	47	47

a = coefficient multiplied by 1000 for ease of presentation.

Regression coefficients are metric estimates (weighted according to the fixed-effects correction for independence), and t-values are reported in parenthesis.

* = p<.05

** = p<.01

† = p<.10

Table 6.14. Summary of effect size estimates for variables specified by subcultural theory.

Predictor Domain	Mean ADJz	Overall Rank	Mean Rank Across Specifications
Urbanism effect	.162	13	14.73
Southern effect	.125	17	16.27

Table 6.15. General and theory-specific WLS regression models of the influences of methodological variations on the mean effect size estimates of variables specified by subcultural theory.

Methodological Characteristic	Urbanism Model 1	Southern Model 1	Southern Model 2	Southern Model 3
Level of aggregation	.039** (4.458)	.010 (.631)	.012 (.638)	.009 (.565)
U.S. origin	.129* (2.142)	--	--	--
Variables from competing theories controlled	-.189* (-2.556)	-.625* (-2.562)	-.628* (-2.554)	-.616* (-2.589)
Time dimension (1 = longitudinal analysis)	-.073 (-1.538)	-.097 (-1.045)	-.104 (-1.012)	-.130 (-1.414)
Ratio of sample size to independent variables	.001** (4.988)	-.001 (-1.422)	-.001 (-1.411)	-.001† (-1.761)
Violent crime dependent variable	-.071** (-2.725)	.045 (.969)	.044 (.935)	.049 (1.068)
Controls for absolute deprivation theory included	--	--	-.007 (-.156)	--
Controls for relative deprivation theory included	--	--	--	-.108* (-2.460)
Constant	.115*	.719**	.723**	.756**
Model R ²	.200**	.090†	.09	.140*
Sample size	178	110	110	110

a = coefficient multiplied by 1000 for ease of presentation.

Regression coefficients are metric estimates (weighted according to the fixed-effects correction for independence), and t-values are reported in parenthesis.

* = p<.05

** = p<.01

† = p<.10

Appendix 1. Summary of empirical tests of social disorganization theory.

<i>Study</i>	<i>Key Independent Variables</i>	<i>Dependent Variables</i>	<i>Level of Analysis</i>	<i>Findings</i>
Bellair (1997)	Traditional social disorganization variables in conjunction with a social interaction index	Burglary, motor vehicle theft, and robbery	Neighborhood	Social interaction significantly predicted dependent variables mediated much of the effect of traditional social disorganization
Bergen and Herman (1998)	Racial heterogeneity	Collective violence (riots)	Census tracts	Measures of racial heterogeneity predicted the likelihood of a future riot
Bursik (1986a)	Racial heterogeneity	Juvenile delinquency	Neighborhood	Neighborhood-level racial heterogeneity related to rates of juvenile delinquency in cross-sectional and longitudinal data
Bursik (1986b)	Racial composition (factor) and SES (factor)	Rates of juvenile delinquency	Neighborhood	Racial composition and SES predicted rates of delinquency in a reciprocal (analytic) relationship
Bursik and Grasmick (1992)	Racial heterogeneity, unemployment, and residential stability	Rates of juvenile delinquency	Neighborhood	In a multi-level data set for Chicago, racial heterogeneity, unemployment, and residential stability predicted rates of juvenile delinquency
Bursik and Grasmick (1993a)	SES, economic deprivation variables, and regulatory capacity	Rates of juvenile delinquency	Neighborhood	The effect of racial heterogeneity on rates of delinquency is an indirect effect of economic deprivation and regulatory capacity supported (statistically)
Bursik and Webb (1982)	Changes in population size, racial composition, and levels of household density	Changes in rates of delinquency	Neighborhood	Neighborhood-level changes in racial composition and household density predicted changes in rates of delinquency

Byrne (1986)	Physical and population-compositional variables	Robbery, burglary, larceny, and motor vehicle theft	Cities	After controlling for compositional and social disorganization, physical characteristics predicted
Crutchfield et al. (1982)	Residential mobility	Multiple rates of violent and property crimes	SMSA	Along with population mobility was the best predictor of the property crime rate
Curry and Spergel (1988)	Racial/ethnic heterogeneity and poverty	Delinquency rates and gang homicide rates	Neighborhood	Racial/ethnic heterogeneity related to rates of homicides, but poverty related to burglary
Fleisher (1966)	Residential mobility, racial heterogeneity, divorce rate, income, along with unemployment and region	Overall delinquency rates	City	Only rates of fear (Southern dummy variable) were predictors of delinquency
Greenberg (1986)	Perceptions of physical deterioration, neighborhood disorder, and economic viability	Fear of crime	Neighborhood (attitudinal items aggregated up)	Neighborhood characteristics of fear; rather than the rate of crime or its influence
Heitgerd and Bursik (1987)	Household characteristics (factor), racial composition, and residential stability	Changes in rates of delinquency	Neighborhood	Levels of social disorganization and community delinquency
Krivo and Peterson (1996)	Economic disadvantage, racial composition, residential instability, and unemployment	Violent and property crime rates	Census tract	Extremely disadvantaged tracts to experience the highest local structural rates of crime in white and black areas
Liska et al. (1998)	Racial composition	Violent crime rate	Cities (suburbs)	Racial composition was found to be the best predictor of robbery rates, but not of racial composition

Messner and Sampson (1991)	Sex ratio and family disruption	Murder and robbery rates	Cities	The sex ratio and relationship with crime. In particular, the relationship to family disruption is then positive.
Miethe et al. (1991)	Ethnic heterogeneity, residential mobility, along with routine activities controls (exposure, target attractiveness, and guardianship proxies)	Homicide, robbery, and burglary rates	Cities	Variables from routine activities theory predict crime rates. Ethnic heterogeneity and residential mobility are stronger and more important for the violence.
Morenoff and Sampson (1997)	Principal components model of ecological characteristics (changes in population size, racial makeup, and socioeconomic disadvantage)	Changes in population	Census tract	High rates of neighborhood change result in declines in crime rates.
Sampson (1986)	Income inequality, unemployment, racial composition, residential mobility, structural density, and family structure	Rates of theft and violent victimization	Neighborhood	Even when structural variables are controlled, neighborhood variables are significant predictors of crime rates.
Sampson (1987)	Male unemployment, family disruption	Race-specific rates of homicide and robbery	Cities	Structural variables (unemployment, deprivation) significantly influence black family disruption, which is directly related to robbery (especially in cities).
Sampson and Groves (1989)	SES, residential mobility, racial heterogeneity, family disruption, and urbanization; intervening	Total victimization, robbery, mugging, burglary, theft, and	Neighborhood	Social disorganization influenced the rate of crime by influencing all of the particular salient factors.

	variables included local friendship networks, unsupervised peer groups, and low organizational participation	vandalism rates		
Sampson and Raudenbush (1999)	Collective efficacy, residential stability, concentrated disadvantage, general disorder	Levels of disorder, homicide, robbery, and burglary	Census tracts	Combining official observations, were significant rates, even when
Sampson, Raudenbush, and Earls (1997)	Concentrated disadvantage, immigrant concentration, residential stability; intervening variable included collective efficacy	Factor of violent crimes	Multilevel (individual and neighborhood levels)	Social disorganization 70% of the variance which, in turn, explained the direct effects
Smith and Jarjoura (1988)	Poverty, racial heterogeneity, residential mobility, family disruption	Violent crime rates	Neighborhoods	Across various specifications poverty main relationships significant in variable
Veysey and Messner (1999)	Replication of Sampson and Groves (1989) study using LISREL analysis	Total victimization rate	Neighborhoods	Consistent with analysis indicating variables do mediate antecedents (residential stability, urbanization)
Warner and Pierce (1993)	Poverty, residential mobility, racial heterogeneity, and structural density	Assault, robbery, and burglary rates	Neighborhoods	Each of the social predicted crime strongest and Interaction terms and heterogeneity predicted
Warner and Roundtree (1997)	Poverty, ethnic heterogeneity, residential stability, local social ties	Rates of assault and burglary	Census tracts	Poverty, heterogeneity, residential stability all explained rates, along with poverty and structural density negatively related

Weicher (1970)	Unemployment, residential mobility, racial heterogeneity, and family disruption	Delinquency rates	Neighborhoods	Across various specifications, residential mobility has a consistent effect
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Appendix 2. Summary of empirical tests of macro-level strain/anomie theory.

<i>Study</i>	<i>Key Independent Variables</i>	<i>Dependent Variables</i>	<i>Level of Analysis</i>	<i>Findings</i>
Chamlin and Cochran (1995)	Economic deprivation and strength of non-economic institutions (family, religion,	Property crime rate (robbery, burglary, larceny)	States	General support for institutional anomie; effect of economic deprivation contingent on the strength of non-economic institutions

	polity); multiplicative terms between economic deprivation and non-economic institutions included			economic
Glaser and Rice (1959)	Age and gender-specific unemployment rates	Rates of overall property and violent crimes	National and city	Male unemployment related to crime rates (under 24) more serious offenses
Messner and Rosenfeld (1997)	Decommodification index; with socioeconomic development, and economic discrimination	Homicide rates	National	After controlling for socioeconomic influences, the decomposition is negatively related
Nuestrom and Norton (1995)	Unemployment	Rates of larceny, arson, burglary, and MV theft	State (LA only)	In a series of bivariate correlations, unemployment was negatively related to larceny and arson, but not to burglary and MV theft
Piquero and Piquero (1998)	Interaction terms between economic conditions and education, the polity, and the family	Property and violent crime rates	States	Economic conditions had a significant direct effect on crime rates, but interaction terms were not significant predictors as well as somewhat sensitive to the dependent variable

Appendix 3. Summary of empirical tests of absolute deprivation/conflict theory.

<i>Study</i>	<i>Key Independent Variables</i>	<i>Dependent Variables</i>	<i>Level of Analysis</i>	<i>Findings</i>
Bailey (1984)	Poverty and income inequality	Murder rates	Cities	High rates of poverty and murder rates, but no relationship
Bailey (1999)	Poverty and income inequality	Rape rates	Cities	Measures of absolute deprivation (SES) among females were strongly and consistently related to rape rates than measures of relative deprivation
Boswell and Dixon (1993)	Class exploitation and inequality	Deaths from violent rebellion	National	Across different nations, inequality had the strongest relationship with deaths from violent rebellion (partial support for hypothesis was found)
Box and Hale (1984)	Female liberation- emancipation in the labor force	Rates of violent crime, and multiple rates of property crimes	National (England and Wales)	Female labor force participation was sporadically related to crime rates but was strongly related to crime rates
Chamlin (1989)	Racial and economic distributions	Police killings	States	Consistent with hypothesis, "composition of state population and rate of crime"
Crutchfield (1989)	Dual labor market, poverty, and income inequality	Multiple rates of violent crime	Census tract	After controlling for dual labor market (full-time workers), neighborhood maintained a statistically significant relationship with violent crime

Crutchfield et al. (1999)	Labor market stratification	Homicide rate	Census tract	Size of secondary indirect relationships operating through education levels, a
Hansmann and Quigley (1982)	Various dimensions of heterogeneity (religious, ethnic, linguistic, and economic)	Homicide rates	National	Ethnic heterogeneity homicide, which heterogeneity homicide (econ
LaFree et al., (1992)	Access to legitimate economic opportunities (employment and education)	Racially-disaggregated robbery, burglary, and homicide rates (all logged)	National (U.S.)	Traditional measures opportunity were rates for whites on positively relate
Loftin and Parker (1985)	Poverty with instrumental variable (infant mortality rate)	Disaggregated homicide rates	Cities	Using the instr approach, it was f consistently sig ho
Lynch et al. (1994)	Surplus value (wealth concentration), police expenditures, unemployment	Violent and property crime rates	National (U.S.)	In structural equat exerted a direct e crime rates, and polic
Messner (1982)	Poverty and inequality	Homicide rate	SMSA	In multivariate significant direct e but significant inv
Messner and South (1986)	Economic deprivation and criminal opportunity structure	Racially disaggregated robbery rates	Cities	In multivariate m variables (per segregation) were robbery rates tha variables (p
Patterson (1991)	Poverty and income inequality	Burglary rates	Neighborhoods	The level of absol stronger predictor deprivation; neit part

Phillips et al. (1972)	Youthful male unemployment	Index crime rate	National (U.S.)	Young male unemployment related to crime rates; the effect was
Smith and Bennett (1985)	Poverty, racial inequality, and family disruption	Rape rates	SMSAs	While racial inequality at the bivariate level carry over into the strongest predictor of poverty, family disruption
White (1999)	Decline in manufacturing jobs, unemployment	Total crime rates (disaggregated into rates of assault, burglary, drugs, robbery, and murder)	Cities	In lagged correlation was positively and of burglary, robbery unrelated to rates
Williams (1984)	Poverty, inequality, racial composition	Homicide rates	SMSAs	Unlike the findings and Messner (1984) significant positive when included
Williams and Drake (1980)	Inequality, unemployment, racial heterogeneity	Multiple rates of violent and property crimes	SMSAs	In multiple regression predicted only composition (percentage related to rates
Williams and Flewelling (1988)	Poverty, family disruption, and racial distribution	Homicide rates (disaggregated by homicide type)	Cities	Net of statistical deprivation (poverty each type of homicide acquaintance,

Appendix 4. Summary of empirical tests of relative deprivation/inequality theory.

<i>Study</i>	<i>Key Independent Variables</i>	<i>Dependent Variables</i>	<i>Level of Analysis</i>	<i>Findings</i>
Austin (1987)	Racial inequality	Homicide, aggravated assault, simple assault	National	Racial inequality crime rate over time purely structural elements p
Balkwell (1990)	Ethnic inequality	Homicide rates	SMSAs	Ethnic inequality homicide rates even general economic culture ha
Bennett (1991)	Inequality and economic development variables	Homicide and theft rates	National	Inequality significant crime, while economic had a non-linear dependence
Blau and Blau (1982)	Income and SES inequality (in comparison to poverty)	Multiple rates of violent crime	SMSA	While poverty is a bivariate level, this introducing statist
Blau and Golden (1986)	Racial SES inequality and southern region	Multiple rates of violent crime	SMSA	Racial inequality violent crimes southern population of violent crimes southern subcult
Danziger and Wheeler (1975)	Income inequality, along with unemployment, age distribution, and police clearance and incarceration rates	Rates of burglary, aggravated assault, and robbery	Nation (U.S.)	Income inequality stable effect on inca
Ehrlich (1975)	Income inequality, SES,	Robbery, burglary,	State	After controlling

	and educational attainment	larceny, auto theft, and overall property crimes		inequality was various t
Farley (1987)	Urban-suburban inequality	Multiple rates of violent and property crimes	SMSA	After controlling characteristics, un related to rates o unrelated to rate burgl
Fowles and Merva (1996)	Wage inequality	Mutiple rates of violent and property crimes	SMSA	Across various inequality was pos to homicides, bu burglar
Gauthier and Bankston (1997)	Sex ratio of employment (proxy for gender employment equality)	(lg) Sex ratio of intimate killings	Cities (U.S.)	Gender employm and inversely relat
Golden and Messner (1987)	Racial inequality	Overall violent crime rate (disaggregated into rates of murder, rape, robbery, and assault	SMSAs	The effect of rac largely by how th based racial ineq out-perform ince measures, and the white SES differe the un
Harer and Steffensmeier (1992)	Overall and racially disaggregated inequality	Homicide, assault, rape, and robbery rates	SMSA	The measure of o consistently a stro crime than any of
Jacobs (1981)	Income inequality (Gini)	Property crimes (disaggregated by burglary, larceny, and robbery rates)	SMSA	Income inequ significantly rela crime rate in mul was strongest
Kovandzic et al. (1998)	Multiple measures of inequality, poverty	Total and disaggregated homicide rates	Cities	Inequality (regard both had signifi homicide (to

Krahn et al. (1986)	Inequality (Gini)	Homicide rates	Cross-national	In multivariate models, the effect was fairly consistent. The effect was strong in the 1970s and in the 1980s.
Krohn (1976)	Inequality and unemployment	Homicide rate, property crime rate, and total crime rate	Cross-national	Inequality was negatively related to the homicide rate, property crime rate, and total crime rate, though fairly weak.
London and Robinson (1989)	Inequality, international corporate penetration	Political violence	Cross-national	International corporate penetration was positively related to the rate of political violence.
Maume (1989)	Inequality (both total and racially-disaggregated inequality)	Rape rates	SMSA	General and racial inequality were positively related to the effect of inequality on rape rates.
Messner (1989)	Economic discrimination and inequality	Homicide rates	Cross-national	Economic discrimination was positively related to homicide rates, but to be a forerunner to income inequality.
Messner and Golden (1992)	Racial SES inequality	Homicide rates (total and racially disaggregated)	Cities	Net of statistical controls, racial SES inequality exerted a consistent positive effect on homicide rates, disaggregated by race.
Messner and Tardiff (1986)	Economic inequality, poverty, and family dissolution	Homicide rates	Neighborhoods	Economic inequality and family dissolution were positively related to the homicide rate, but family dissolution was not.
Muller (1985)	Inequality, sociocultural heterogeneity	Political violence	Cross-national	Both inequality and sociocultural heterogeneity were positively related to political violence in a cross-national context. Inequality was related to political violence in the 1973-1980 period.
Peterson and Bailey (1988)	Inequality (racial and overall) and poverty	Rape rates	SMSA	Both dimensions of inequality were positively related to rape rates, and both were capable of predicting rape rates.

				limited to SMSA rape rates (one SD only inequality (ra pre
Peterson and Krivo (1993)	Racial segregation, racial inequality	Homicide rates (racially disaggregated)	Cities	Racial segregation consistent effect than did levels of
Rosenfeld (1986)	Relative deprivation index (inequality), unemployment	Multiple rates of violent and property crimes	SMSA	Measure of relative related to rates burglary, and large of robbery a
Sampson (1985b)	Income inequality, poverty, racial composition, arrest probability	Violent crime rates (also with disaggregated models for robbery and burglary)	Cities	Income inequality effect on rates of effect, while weak white violence control/deterrence was positively r
Shihadeh and Steffensmeier (1994)	Inequality (overall, within-race, and between-race)	Homicide and robbery (racially disaggregated)	Cities	Net of statistical co for blacks was the the three inequality crime rate. Black an indirect effect of black
South and Messner (1986)	Racial inequality, residential segregation, heterogeneity	Violent crime rates (disaggregated into rates of rape, assault, and robbery)	SMSAs	Structural charact size and resident predicted crime r ineq
Stack (1984)	Inequality, and an interaction between inequality and egalitarian culture	Property crime rates	Cross-national	Income inequality influence on the p interaction with egalitarianism (un party, emp

Kennedy et al. (1991)	Economic inequality (Gini), unemployment (along with family structure, population, and density)	Homicide rates	CMA (Census Metropolitan Areas in Canada)	The strongest inf first-order regress prior homicide rate related) and unemp ho
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Appendix 5. Summary of empirical tests of routine activities theory.

<i>Study</i>	<i>Key Independent Variables</i>	<i>Dependent Variables</i>	<i>Level of Analysis</i>	<i>Findings</i>
Allan and Steffensmeier (1989)	Unemployment	Rates of Youth Crime	States	High rates strongly related opposed
Brenner and Swank (1986)	Economic conditions (age and gender-specific rates of	Homicide rates	National (U.S.)	Changes (specifica

	unemployment)			homicide rate
Britt (1997)	Unemployment	Multiple rates of violent and property crimes	National (U.S.)	Regarding unemployment, unemployment, and property crimes
Bryant and Miller (1997)	Household activity ratio, unemployment, and location quotient (motivated offenders)	Violent crime rates	Cities (U.S.)	Partial support for key variables, crime rates and results were
Cantor and Land (1985)	Unemployment	Multiple rates of violent and property crimes	National (U.S.)	Unemployment related to crime model was specified with relationship specified with indicate separate and
Carroll and Jackson (1983)	Household activity ratio and inequality	Burglary, robbery, and personal violent crimes	Cities	The effects of on crime rates operating t
Chamlin and Cochran (1998)	Economic conditions	Burglary (total, residential, and commercial)	City	The effects of conditions (m asymmetric
Cohen and Felson (1979)	Household activity ratio	Multiple rates of violent and property crime	National	Household activity significantly rape, assault, robbery rate
Cohen et al.	Household activity ratio	Multiple rates of	National	General su

(1980)	(termed “residential population density ratio”) and unemployment	property crimes		theory: ho consistently rates, and un inversely rel
Cohen and Land (1987)	Household activity ratio, unemployment, and age structure	Rates of motor vehicle theft and homicide	National	While age str significantly rates, the ho generally ha
Cook (1978)	Gun availability	Robbery and robbery murder	Cities	Gun densit availability) rates of robb through v
Copes (1999)	Age distributions and structural density	Rates of motor vehicle theft	Census tracts	Variables ce (age and ge only consiste the depend
Devine et al. (1988)	Male unemployment along with economic stability, incarceration rate, sex ratio, and opportunity	Homicide, robbery, and burglary	National	Changes unemploy predictors of and bur
Gillis (1996)	Marital dissolution	Domestic homicide rate	National (France)	Higher rates associated v homicid of
Hesseling (1992)	Offender mobility	Multiple rates of violent and property crimes	Neighborhood	Offender m crimes than f neighborhood
Jackson (1983)	Household activity ratio, racial income inequality, and city size	Multiple rates of violent and property crimes	Cities	Household a positive effec property crim was not full

				income inequities consistent, positive
Kapuskinski et al. (1998)	Gender-disaggregated unemployment rates	Homicide	National (Australia)	While total unemployment is positive, employment effects are mixed
Land et al. (1995)	Unemployment	Multiple rates of violent and property crimes	National (U.S.)	In multivariate models, unemployment (instant) in the neighborhood (guardianship effect) on crime rates
Massey and McKean (1985)	Marital status, residential density, and sex ratio	Homicides	Census tracts	In full regression models, marital status and the percentage of single dwellings were associated with homicides (control variables were also included)
Mazerolle et al. (1998)	Place managers (as a proxy for guardianship), and block-level characteristics	Male drug dealing	Neighborhood-block	Experimental place management in neighborhoods
Messner and Blau (1987)	Non-household and household activity indexes	Multiple rates of violent and property offenses	SMSAs	The volume of non-household activity index was inversely related to crimes (excluding non-household activity related to household burglary)
Nuestrom et al. (1988)	Unemployment	Multiple rates of violent and property offenses	Counties (parishes)	Using zero-order correlations, unemployment was positively related to larceny, but not to homicide, rape, or drug offenses
O'Brien (1991)	Sex ratio	Rape rates	National	The sex ratio was positively related to rape rates (controlling for control and other variables)
Osborn et al.	Residential density, male	Property crime	Neighborhood	Residential density was negatively related to property crime

(1992)	unemployment, sex ratio	rates		to property crime unemployment inverse
Roncek and Maier (1991)	Presence of bars or taverns (recreational drinking establishments)	Multiple rates of violent and property crimes	City blocks	The presence of bars or taverns was a consistent predictor of property crime rate,
Stahura and Sloan (1988)	Motivation (unemployment, opportunity (employment concentration), and guardianship (female employment)	Violent and property crime rates	Suburbs	Opportunity theory predicts that property crime rates are predicted by guardianship theory. Both violent and property crime rates are
Witt et al. (1999)	Unemployment (along with inequality and police per capita)	Property crime rates	Neighborhoods	Across all of the cities, the lagged measure of unemployment was positively related to property crime rates. The relationship was generally positive, but the effect of police per capita was generally negative.

Appendix 6. Summary of empirical tests of deterrence/rational choice theory.

<i>Study</i>	<i>Key Independent Variables</i>	<i>Dependent Variables</i>	<i>Level of Analysis</i>	<i>Findings</i>
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Antunes and Hunt (1973)	Certainty and severity of punishment	Index offenses	States	Moderate association between independent variables and crime rates
Avio and Clark (1976)	Police clearance rates	Theft, fraud, breaking and entering, and robbery	Provincial (Canada)	Clearance rates increase with type of crime
Bailey (1980)	Death penalty	Homicide rates	States	Despite statistically significant correlations, in general, neither certainty or celerity had a significant predictive effect
Bailey (1983)	Death penalty	Homicide rates	City (Chicago)	Executions produced a statistically significant effect on both first-degree and second-degree homicide rates
Bailey (1990)	Death penalty	Homicide rates	National	Television public executions had a statistically significant effect on homicide rates from 1976 through 1989
Bailey and Peterson (1989)	Death penalty	Homicide rates	National	In a reanalysis of the data, the effect of executions on monthly homicide rates was statistically significant
Barnett (1986)	Lack of industrial penalties for non-compliance	Tax non-compliance	National (Sweden)	Bivariate results showed a significant relationship between low tax non-compliance and instances of tax evasion
Black and Orsagh (1978)	Death penalty and incarceration	Homicide rates	States	No consistent evidence of a deterrent effect of either the death penalty or incarceration using statistical analysis
Brown (1978)	Certainty of arrest	Index crime rate	Cities and Counties (CA and FL)	At the bivariate level, the effect of the certainty of arrest on crime rates was more pronounced in California than in Florida. The effect is more pronounced in California than in Florida.

Cappell and Sykes (1991)	Unemployment and incarceration	Index crime rates	National (U.S.)	Crime rates v unemployment series design, and levels of
Carr-Hill and Stern (1973)	Police strength and probability of incarceration	General crime rates	National (UK and Wales)	Analyses reveal deterrence perspective on incarceration effectiveness factors (e.g., age) consistently
Carr-Hill and Stern (1977)	Police strength and probability of incarceration	General crime rates	National (UK and Wales)	Partial support for certain measures predicted crime expenditures and was no significant
Chamlin (1988)	Arrests	Robbery, burglary, larceny, and auto theft	City	Using time series (ARIMA), no correlation between arrest
Chamlin (1991)	Arrest clearance rates	Rates of robbery, burglary, larceny, and auto theft	City	Using a series of consistent deterrence revealed. Arrests sporadically related to cities, where clearance
Chamlin et al. (1992)	Arrests	Robbery, burglary, larceny, and auto theft	City	Using various time series models, support for on crimes is most shortest time
Chapman (1976)	Arrest ratio	Rates of violent and property crimes	Cities	In models estimating arrest ratio was significant for property offenses

Chiricos and Waldo (1970)	Probability of incarceration	Multiple rates of violent and property crimes	National	Threat of incarceration and crime rates at the state level. Significant relationship revealed for murder.
Cochran et al. (1994)	Death penalty	Overall and disaggregated homicide rates	State	Using a week-level model, no effect of death penalty on homicide rates. Significant brutalities emerge for murder.
Decker and Kohfeld (1984)	Death penalty	Murder and non-negligent manslaughter rates	State	After controlling for sociodemographic variables, death penalty had no statistically significant effect (otherwise) on murder rates.
Decker and Kohfeld (1985)	Arrests and arrest ratios	Robbery, burglary, and murder rates	City	Arrests and arrest ratios predict rates of robbery, burglary, and murder. Significant relationship with robbery and burglary.
Ehrlich (1975)	Death penalty	Murder rates	National	Using a one-year lagged equation, execution rate has an inverse effect on murder rates.
Geerken and Gove (1977)	Police clearance rates	Multiple rates of disaggregated FBI index offenses	SMSA	At the bivariate level, clearance rates maintain a statistically significant relationship with robbery, burglary, and murder. Relationship with assault is not significant.
Greenberg and Kessler (1982)	Police clearance rates	Multiple rates of property and violent crimes	Cities	Using a panel model with control variables, no observed effect on crime rates.
Greenberg et al., (1979)	Police clearance rates	Disaggregated index offenses	Cities	Clearance rates vary with crime rates. Significant relationship with robbery, burglary, and murder.
Greenwood and Wadycki (1973)	Police expenditures and police per capita	Overall property and violent crime rates	SMSA	Police expenditures are influenced by crime rates. Significant relationship with robbery, burglary, and murder.

Hemley and McPheters (1974)	Probability of incarceration	Rates of robbery, burglary, and larceny	States	The probability of effect on robbery unrelated to rates at the
Huff and Stahura (1980)	Police employment	Rates of violent and property crimes	Cities (suburbs)	Police employment both violent and reciprocal effect (weak positive)
Jacob and Rich (1980-81)	Various dimensions of police aggressiveness	Robbery rates	Cities	Measures such as size, and arrest rate correlations with sectionally and
Joubert et al. (1981)	Structural characteristics and prison admissions	Overall crime rate	States	Prison admissions states' crime characteristics. No on crime
Kleck (1979)	Gun ownership, the death penalty, and incarceration	Homicide rate	National (U.S.)	Using structural reciprocal effect significant negative death penalty was gun ownership
Kohfeld and Sprague (1990)	Changes in arrests	Burglary	Census tract	Using weekly time inverse effect of observed follow
Langworthy (1989)	Police "stings"	Auto theft	City	Using time series of the police sting
Logan (1972)	Certainty and severity of incarceration	Multiple rates of violent and property crimes	States	Using bivariate analysis a fairly consistent incarceration (incarceration) and
Logan (1975)	Arrest clearance rates and probability of	Multiple rates of violent and	States	Using bivariate analysis inverse relations

	incarceration	property crimes		and crime, and crim
Marvell and Moody (1996)	Police per capita	Total crime rate	States and cities	In annual time series analyses) police crime rates at the (and negatively
Marvell and Moody (1999)	Prison population, death penalty	Racial and gender disaggregated homicide rates	National (U.S.)	In time series controls, the pris inversely related females, white penalty was un every
Mathur (1978)	Police expenditures, certainty and severity of punishment	Multiple rates of violent and property crimes	Cities	Police expend predict crime ra did exert, at times but, when severe punishment decre of incar
McDowall et al. (1992)	Mandatory sentencing law for gun crimes	Homicide, assault, and robbery	Cities	The adoption of th had a negative e unrelated to gun
Merriman (1991)	Structural characteristics, along with prison conviction and admission rates	Multiple rates of violent and property crimes	National (Japan)	Probability of con predict crime r variables were u controls for sin
Morris and Tweeten (1971)	Police per capita	Overall crime rate	Cities	While police per interaction eff relationships wit main effect of po signi
Nagin (1978)	Risk of imprisonment and sanction levels	Overall crime rate	States	Using a two-sta adjust for simul that high crime effect on the risk to the inverse r reversin

Parker and Smith (1979)	Certainty and severity of incarceration, poverty, urbanism	Homicide rate (total and disaggregated by primary and non-primary homicides)	States	In multiple incarceration punishment) did of homicide (to poverty measure positively related
Passel and Taylor (1977)	Capital punishment	Murder rate	National (U.S.)	In a reanalysis temporal heterog relationships di effect of cap executions to murder
Peterson and Bailey (1991)	Capital punishment	Felony murder rate	National (U.S.)	In a series of aut the variables r amount of public predicted
Phillips and Votey (1975)	Certainly of incarceration, probability of conviction	Index crime rate	Counties	Using OLS and regression an incarceration wa but the proba significant invers
Phillips and Ray (1982)	Death penalty, imprisonment	Homicide rate	State (CA)	Using time-series causality, the pro not related to probability of re exert a short
Pogue (1975)	Police expenditures,	Multiple rates of	SMSAs	While clearance r

	police clearance rates	violent and property crimes		crimes (primarily expenditures were
Sampson and Cohen (1988)	Police aggressiveness	Robbery and burglary rates	Cities	After controlling for aggregate crime composition, aggressiveness, relationship with police aggressiveness and for
Sjoquist (1973)	Police clearance rates, conviction rates, and the incarceration effect	Aggregated rates of robbery, burglary, and larceny	Cities	In econometric model, rate and the level of crime inversely related to percent non-white
Snyder and Tilly (1972)	Economic hardship along with the threat of incarceration	Rates of collective violence	National (France)	After controlling for hardship, the threat of incarceration has an inverse effect on annual
Stack (1987)	Publicized executions	Homicides	National (U.S.)	Using monthly data, publicized executions had significant inverse effects, however, those for unemployment were smaller than those for
Stack (1990)	Death penalty-executions	Homicide rates	State (SC)	The effect of publicized homicides was significant, possibly indicating a deterrent effect
Stack (1998)	Publicized executions	Homicides	State (CA)	For monthly data, publicized executions had a significant effect

Swimmer (1974)	Police expenditures	Violent and property crime rates	Cities	Using two-stage simultaneity, the inverse effect of p crime rates, but
Tittle (1969)	Certainty and severity of imprisonment	Total and disaggregated felonies (violent and property)	States	In a series of biv (rather than th (probability of i related to rate larceny, robb unrelated to rate
Tittle and Rowe (1974)	Certainty of arrest	Overall crime rates	Cities and counties	Certainty of arres rates once the particular thre
Wellford (1974a)	Police per capita and police expenditures	Total crime, violent crime, and property crime rates	Cities	Using bivariate analysis, police the amount of e
Wilson and Boland (1978)	Police arrest ratio	Robbery rates	Cities	Using two-stage accommodate sin ratio exerted a st effect
Yu and Liska (1993)	Certainty of arrest	Race-specific rates of rape, robbery, and assault	cities	In race-specific “tipping”) and a c effect of the certa rate
Yunker (1982)	Death penalty, and various sociodemographic predictors	Homicide rate	National (U.S.)	In an ana sociodemograph before and after for a drop in ex structural predic increase in crim attributed to th

Appendix 7. Summary of empirical tests of social support/altruism theory.

<i>Study</i>	<i>Key Independent Variables</i>	<i>Dependent Variables</i>	<i>Level of Analysis</i>	<i>Findings</i>
Beki et al. (1999)	Index of private economic welfare	Multiple rates of violent and property crimes	National (The Netherlands)	Index exerted relationship with
Chamlin and Cochran (1997)	United Way contributions	Rates of property and violent crime	Cities	Net of statist contributions (a significantly pro and
Chamlin et al. (1999)	Ratio of tax deductible contributions to the total number of returns	Rates of violent and property crime	States	Social altruism n positively related unrelated to
DeFronzo (1983)	AFDC assistance	Multiple rates of violent and property crimes	SMSA	AFDC assistan predictor of both controlling for relationship consiste
DeFronzo (1996)	Cost-of-living adjusted AFDC payment per person	Burglary rates	Cities	AFDC measure direct effect on indirect effe influence on f
DeFronzo (1997)	Cost-of-living adjusted AFDC payment per person	Homicides	Cities	AFDC measure effect (negative) effect on homici of family di
Fiala and LaFree (1988)	Social security and family allowances	Child homicide	Cross-National	Social-econom statistical contr rates of child h
Hannon and DeFronzo	Cost of living-adjusted public assistance	Total crime, property crime, and	County	Net of controls, direct inverse ef

(1998a)		violent crime (lg) rates		(with resource
Hannon and DeFronzo (1998b)	Adjusted AFDC payments per person	Property crime rates (disaggregated by burglary, larceny, and auto theft	County	Adjusted AFDC effect on e mul

Appendix 8. Summary of empirical tests of subcultural theory.

<i>Study</i>	<i>Key Variables</i>	<i>Independent Variables</i>	<i>Dependent Variables</i>	<i>Level of Analysis</i>	<i>Findings</i>
Archer and Gartner (1976)		National wars	Homicide rates	Nations	Wars, e legitimate v produce an
Archer et al. (1978)		Population size	Homicide rates	Cities (from multiple nations)	Populatio homicide n relative t
Bainbridge (1989)		Religious participation	Separate models estimated for murder, rape, assault, burglary, larceny, and robbery	City	Religious consisten measure
Baron and Straus (1988)		Subcultural legitimization of violence	Homicide rates	States	Legitimiza urbanism w homicide ra poverty
Gastil (1971)		Southernness index	Homicide rate	States	Using biv analysis,

				effect of t
Gibbs and Erickson (1976)	Urban-suburban population size ratio	Multiple rates of property and violent crimes	Cities (UAs and SMSAs)	Population r rates of hom unrelated assault.
Groves et al. (1987)	Islamic religion	Multiple rates of property and violent crime	National	Islamic and significant ty
Hackney (1969)	Southern region dummy variable	Homicide rate	States	Using biva analysis, sou had a positiv
Hartnagel and Lee (1990)	Population size	Rates of violent and property crime	Cities (Canada)	While population indirect property c effects o
Huff-Corzine et al. (1986)	Southern index and the percent southern-born	Homicide rate	States	Southern in to the hor disaggrega the perce maintained
Loftin and Hill (1974)	Regional dummy variable and southernness index	Homicide rate	States	The effect violence specificatio
Messner (1983)	Southern region, southernness index, racial composition	Homicide rates	SMSAs	Net of “souther predicted h models; rac direct

Sampson (1985)	Racial composition, poverty, inequality, unemployment, and population size	Homicide rates (racially disaggregated)	Cities	Factors such as income inequality and racial effects on crime rates were significant, but were not
Smith and Parker (1980)	Southern dummy variable, poverty, inequality, and racial composition	Homicide rates (disaggregated by homicide type)	States	Southern dummy variable had a significant zero-order effect on homicide rates between states. The dummy variable predicted homicide rates, while inequality was not significant.
Webb (1972)	Industrial diversification, population size, and population density	Property and violent crime rates	Cities	Using zero-order analysis, industrial diversification and population size were both significant predictors of crime rates, with industrial diversification having the strongest effect.

Appendix 9. Summary of empirical tests of various sociodemographic predictors.

<i>Study</i>	<i>Key Independent Variables</i>	<i>Dependent Variables</i>	<i>Level of Analysis</i>	<i>Findings</i>
Allison (1972)	Age, urbanism, unemployment, education, sex ratio, population density, police expenditures, and SES	Overall index crime rate	City	Unemployment male versus female aggregate-level p
Baum (1999)	SES, poverty, residential stability, sex ratio, opportunity	Drinking and driving rates	Postal areas (counties)	SES, poverty, and strongest bivarial driving rates. O (work) was also a
Chamlin and Kennedy (1991)	Racial heterogeneity, family disruption, absolute deprivation, unemployment, police bureaucracy change	Multiple rates of property offenses	City	Net of statistio bureaucratic stru significantly aff pr
Cook and Zarkin (1986)	Unemployment, average personal income, alcohol consumption	Age-adjusted homicide rates	National	Unemployment alcohol consumption rates of age-a regression m
Gartner (1990)	Welfare spending, inequality, family disruption, racial heterogeneity, labor market and age distributions (by gender), and the death	Aggregated and gender-specific homicide rates	Cross-National	Inequality, welfare the death pena heterogeneity v male and female participation wa ho

	penalty			
Gartner and Parker (1990)	Age structure of the population (specifically, the proportion of the population that is young and male)	Homicide rate	National	The changes in the rate in a nation's population influence on the
Gartner et al. (1990)	Gender economic stratification, divorce rate, female labor force participation	Homicide rate (overall and gender-gap)	Cross-national	As female and male rates more similar (i.e. gap in homicide rates)
Humphries and Wallace (1980)	Police per capita, southern region	Multiple rates of violent and property crimes	Cities	Police per capita and homicide rates in the southern region had a direct relationship with economic hardship
Land et al. (1990)	Various structural covariates	Homicide rates	Cities, MAs, and states	The effects of major structural covariates across levels of specification. Overall, the composition remained relatively stable over time periods
Mencken and Barnett (1999)	Urbanism, density, education, and unemployment	Murder and non-negligent manslaughter rates	Counties	Population density and index were the most significant independent variables. The model was correct
Mladenka and Hill (1976)	Poverty, racial heterogeneity, density, education, and income	Rates of personal and property crimes	Neighborhoods	At the bivariate level, the rates of crimes were significantly related to racial composition. The model and average income were significant at the zero-order level
Neapolitan (1998)	Racial composition, inequality, ethnic heterogeneity	Homicide rates	Cross-national	In multiple regression analyses, the research on the percent black population and homicide rates

Phillips (1997)	African-American social control, structural, and rational choice variables	African-American homicide rates	MSA	Social control v population size, inequality (Gini) significantly pred
Rushton (1995)	Racial composition (percent black)	Homicide rates	Cross-national	Using one-wa sample of 79 nati nations with la higher
Pressman and Carol (1971)	Racial heterogeneity, family income, population density	Multiple rates of violent and property crimes	SMSAs	Using partial percentage of the was the strongest property crimes significantly pred
Quinney (1966)	Traditional socioeconomic, economic development, and family variables	Multiple rates of violent and property crimes	States (disaggregated by rural and SMSA areas)	Traditional str stronger predict whereas occupat to be stronger
Skogan (1977)	City size, density, and heterogeneity	Overall crime rate	Cities	Structural predic which, in turn, h city'
Sloan (1994)	Structural and demographic characteristics of college campuses	Theft, violent crime, drinking, vandalism, and overall crime rate	College campuses	Campus size an most stable predic campuses. The acro
Smith and Brewer (1992)	Population structure, resource deprivation, family disruption, unemployment, and region	Homicide victimization rates (disaggregated by gender)	Cities	The most consi male and female were popul deprivation, fam and southern regi to male but not f

Stafford and Gibbs (1980)	Urban population ratio, racial composition, income, unemployment, and poverty	Personal and property crime rates	Cities	Both racial composition and the dominance of both exerted significant effects on rates of personal and property crimes
Steffensmeier et al. (1987)	Age cohort effect	Index crime rate (disaggregated by offense type)	National (U.S.)	Relative age cohort effects were significant for robbery and burglary, but not for homicide
Steffensmeier et al. (1992)	Age cohort effect	Homicide, assault, robbery, burglary, and larceny rates	National (U.S.)	Relative cohort effects were significant for each type of crime, but not for traditional age cohort effects
Wellford (1974b)	GNP, democratic orientation	Multiple rates of violent and property crimes	Cross-national	In bivariate correlations, GNP per capita along with democratic orientation had the strongest positive effects on crime rates

DISSERTATION META-ANALYSIS-MACRO ARTICLE CODESHEET

BETA-WEIGHT CODESHEET

PART ONE: STUDY INFORMATION

Authors of the study

Study number _____

Model number _____

Test of which theory?

- 1 = social disorganization
- 2 = anomie/strain
- 3 = absolute dep/conflict
- 4 = relative dep/inequality
- 5 = deprivation combo
- 6 = routine activities
- 7 = deterrence
- 8 = soc. Support/altruism
- 9 = subcultural
- 10 = general macro predictors _____

PART TWO: PREDICTOR DOMAINS

Social Disorganization Theory

Racial heterogeneity _____
V1 sig _____

- 0 = % non-white
- 1 = % black
- 2 = heterogeneity _____

Socioeconomic status _____
V2 sig _____

- 0 = mean house \$
- 1 = median house \$
- 2 = SES index _____

Residential mobility _____

V3 sig _____

Family structure/disruption _____

V4 sig _____

0 = % div/separated

1 = single-head house

2 = female-head house _____

Collective efficacy _____

V5 sig _____

Unsupervised local peer groups _____

V6 sig _____

Anomie/Strain Theory

Strength of noneconomic institutions _____

V7 sig _____

Absolute Deprivation/Conflict Theory

Poverty _____

V8 sig _____

0 = % poverty

1 = poverty index _____

0 = race heterogeneous

1 = race homogeneous _____

0 = male poverty

1 = female poverty _____

Relative Deprivation/Inequality Theory

Inequality _____

V9 sig _____

0 = Gini index

1 = Other index _____

0 = Race heterogeneous

1 = Race homogeneous _____

Routine Activities Theory

Household activity ratio _____

V10 sig _____

Unemployment rate _____

V11 sig _____

Overall rate (1 = yes) _____

If not overall rate, then:

0 = Male rate

1 = Female rate _____

age restricted (1=yes) _____

0 = race heterogeneous

1 = race homogeneous _____

Length of unempl.

Considered (1 = yes) _____

Deterrence/Rational Choice Theory

Incarceration effect _____

V12 sig _____

Police activities _____

V13 sig _____

0 = Police size

1 = Arrest ratio

2 = police per capita _____

0 = Identity restriction

1 = No restriction _____

Get tough policies _____

V14 sig _____

0 = police practices

1 = sentencing policy

2 = firearms policy _____

if firearms policy:

0 = gun control law

1 = concealed weapon law _____

Social Support/Social Altruism Theory

Social support/altruism _____

V15 sig _____

0 = Public source
1 = Private source _____

Subcultural Theory

Urban subculture effect _____
V16 sig _____

0 = pop size
1 = other urban measure _____

Southern subculture effect _____
V17 sig _____

0 = region dummy
1 = southern index
2 = % south-born _____

Effect of religion _____
V18 sig _____

0 = participation
1 = % protestant
2 = other _____

Firearms ownership effect _____
V19 _____

0 = overall ownership
1 = handgun ownership _____

Sociodemographic and Other Predictors

Age effects _____
V20 sig _____

0 = Age/year category
1 = Age cohort effect _____

Structural/population density _____
V21 sig _____

Educational variables _____
V22 sig _____

0 = % high school
1 = mean teacher salary
2 = other _____

Sex ratio (male) _____
V23 sig _____

PART THREE: IMPACT OF METHODOLOGICAL VARIATIONS

General

Year of data collection (DV) _____

Sample size _____

Level of aggregation

- 0 = neighborhood/block
- 1 = census tract
- 2 = city
- 3 = county
- 4 = SMSA
- 5 = state
- 6 = country
- 7 = multi-level model _____

Origin of sample

- 0 = US only
- 1 = other western nation only
- 2 = other non-western nation
- 3 = cross-national _____

Model specification and research design

Competing theories controlled

- 0 = no
- 1 = yes _____

Which theory? (1 = yes)

- social disorganization _____
- anomie/strain _____
- absolute dep/conflict _____
- relative dep/inequality _____
- routine activities _____
- deterrence/R-C _____
- support/altruism _____
- subcultural _____

Statistical method

- 0 = OLS regression
- 1 = WLS regression
- 2 = LISREL/path analysis
- 3 = ARIMA/time series
- 4 = nonlinear model
- 5 = stepwise regression _____

Reciprocal effects
0 = estimated
1 = not estimated _____

Time dimension
0 = cross-sectional
1 = longitudinal _____

if longitudinal:
time lag (# of mths) _____

Number of independent variables _____

Dependent variable
1 = overall violent crime
2 = overall property crime
3 = robbery
4 = burglary
5 = homicide/murder
6 = rape/forcible sex assault
7 = aggravated assault
8 = all index offenses
9 = violent delq. rates
10 = property delq. rates
11 = overall delq. rate
12 = theft/larceny (adult) _____

Logged or unlogged dependent variable?
0 = unlogged
1 = logged _____

SD of the dependent variable _____

Overall Predictive Power

R-square value _____

DISSERTATION META-ANALYSIS-MACRO ARTICLE CODESHEET

METRIC COEFFICIENT CODESHEET

PART ONE: STUDY INFORMATION

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- 9 = subcultural
- 10 = general macro predictors _____

PART TWO: PREDICTOR DOMAINS

Social Disorganization Theory

Racial heterogeneity _____
 V1 sig _____

- 0 = % non-white
- 1 = % black
- 2 = heterogeneity _____

Socioeconomic status _____
 V2 sig _____

- 0 = mean house \$
- 1 = median house \$
- 2 = SES index _____

Residential mobility _____
 V3 sig _____

Family structure/disruption _____
 V4 sig _____

- 0 = % div/separated
- 1 = single-head house
- 2 = female-head house _____

Collective efficacy _____
 V5 sig _____

Unsupervised local peer groups _____
 V6 sig _____

Anomie/Strain Theory

Strength of noneconomic institutions _____
 V7 sig _____

Absolute Deprivation/Conflict Theory

Poverty _____
V8 sig _____

0 = % poverty
1 = poverty index _____

0 = race heterogeneous
1 = race homogeneous _____

0 = male poverty
1 = female poverty _____

Relative Deprivation/Inequality Theory

Inequality _____
V9 sig _____

0 = Gini index
1 = Other index _____

0 = Race heterogeneous
1 = Race homogeneous _____

Routine Activities Theory

Household activity ratio _____
V10 sig _____

Unemployment rate _____
V11 sig _____

Overall rate (1 = yes) _____

If not overall rate, then:

0 = Male rate
1 = Female rate _____

age restricted (1=yes) _____

0 = race heterogeneous
1 = race homogeneous _____

Length of unempl.
Considered (1 = yes) _____

Deterrence/Rational Choice Theory

Incarceration effect _____
V12 sig _____

Police activities _____
V13 sig _____

0 = Police size
1 = Arrest ratio
2 = police per capita _____

0 = Identity restriction
1 = No restriction _____

Get tough policies _____
V14 sig _____

0 = police practices
1 = sentencing policy
2 = firearms policy _____

if firearms policy:
0 = gun control law
1 = concealed weapon law _____

Social Support/Social Altruism Theory

Social support/altruism _____
V15 sig _____

0 = Public source
1 = Private source _____

Subcultural Theory

Urban subculture effect _____
V16 sig _____

0 = pop size
1 = other urban measure _____

Southern subculture effect _____
V17 sig _____

0 = region dummy
1 = southern index
2 = % south-born _____

Effect of religion _____
V18 sig _____

0 = participation
1 = % protestant
2 = other _____

Firearms ownership effect
V19

0 = overall ownership
1 = handgun ownership _____

Sociodemographic and Other Predictors

Age effects _____
V20 sig _____

0 = Age/year category
1 = Age cohort effect _____

Structural/population density _____
V21 sig _____

Educational variables _____
V22 sig _____

0 = % high school
1 = mean teacher salary
2 = other _____

Sex ratio (male) _____
V23 sig _____

PART THREE: IMPACT OF METHODOLOGICAL VARIATIONS

General

Year of data collection (DV) _____

Sample size _____

Level of aggregation
0 = neighborhood/block
1 = census tract
2 = city
3 = county
4 = SMSA
5 = state
6 = country

7 = multi-level model _____

Origin of sample

0 = US only

1 = other western nation only

2 = other non-western nation

3 = cross-national _____

Model specification and research design

Competing theories controlled

0 = no

1 = yes _____

Which theory? (1 = yes)

social disorganization _____

anomie/strain _____

absolute dep/conflict _____

relative dep/inequality _____

routine activities _____

deterrence/R-C _____

support/altruism _____

subcultural _____

Statistical method

0 = OLS regression

1 = WLS regression

2 = LISREL/path analysis

3 = ARIMA/time series

4 = nonlinear model

5 = stepwise regression _____

Reciprocal effects

0 = estimated

1 = not estimated _____

Time dimension

0 = cross-sectional

1 = longitudinal _____

if longitudinal:

time lag (# of mths) _____

Number of independent variables _____

Dependent variable

1 = overall violent crime

2 = overall property crime

3 = robbery

4 = burglary

- 5 = homicide/murder
- 6 = rape/forcible sex assault
- 7 = aggravated assault
- 8 = all index offenses
- 9 = violent delq. rates
- 10 = property delq. rates
- 11 = overall delq. rates
- 12 = theft/larceny adult) _____

Logged or unlogged dependent variable?
0 = unlogged
1 = logged _____

SD of the dependent variable _____

Overall Predictive Power

R-square value _____

DISSERTATION META-ANALYSIS-MACRO ARTICLE CODESHEET

BIVARIATE CORRELATION CODESHEET

PART ONE: STUDY INFORMATION

Authors of the study

Study number _____

Model number _____

Test of which theory?

- 1 = social disorganization
- 2 = anomie/strain
- 3 = absolute dep/conflict
- 4 = relative dep/inequality
- 5 = deprivation combo
- 6 = routine activities
- 7 = deterrence
- 8 = soc. Support/altruism
- 9 = subcultural
- 10 = general macro predictors _____

PART TWO: PREDICTOR DOMAINS

Social Disorganization Theory

Racial heterogeneity _____
V1 sig _____

- 0 = % non-white
- 1 = % black
- 2 = heterogeneity _____

Socioeconomic status _____
V2 sig _____

- 0 = mean house \$
- 1 = median house \$
- 2 = SES index _____

Residential mobility _____
V3 sig _____

Family structure/disruption _____
V4 sig _____

- 0 = % div/separated
- 1 = single-head house
- 2 = female-head house _____

Collective efficacy _____
V5 sig _____

Unsupervised local peer groups _____
V6 sig _____

Anomie/Strain Theory

Strength of noneconomic institutions _____
V7 sig _____

Absolute Deprivation/Conflict Theory

Poverty _____
V8 sig _____

0 = % poverty
1 = poverty index _____

0 = race heterogeneous
1 = race homogeneous _____

0 = male poverty
1 = female poverty _____

Relative Deprivation/Inequality Theory

Inequality _____
V9 sig _____

0 = Gini index
1 = Other index _____

0 = Race heterogeneous
1 = Race homogeneous _____

Routine Activities Theory

Household activity ratio _____
V10 sig _____

Unemployment rate _____
V11 sig _____

Overall rate (1 = yes) _____

If not overall rate, then:

0 = Male rate
1 = Female rate _____

age restricted (1=yes) _____

0 = race heterogeneous
1 = race homogeneous _____

Length of unempl.
Considered (1 = yes) _____

Deterrence/Rational Choice Theory

Incarceration effect _____
V12 sig _____

Police activities _____
V13 sig _____

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Get tough policies _____
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 V19
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 1 = handgun ownership _____

Sociodemographic and Other Predictors

Age effects _____
 V20 sig _____
 0 = Age/year category
 1 = Age cohort effect _____
 Structural/population density _____
 V21 sig _____
 Educational variables _____
 V22 sig _____
 0 = % high school
 1 = mean teacher salary
 2 = other _____
 Sex ratio (male) _____
 V23 sig _____

PART THREE: IMPACT OF METHODOLOGICAL VARIATIONS

General

Year of data collection (DV) _____
 Sample size _____

Level of aggregation

- 0 = neighborhood/block
- 1 = census tract
- 2 = city
- 3 = county
- 4 = SMSA
- 5 = state
- 6 = country
- 7 = multi-level model _____

Origin of sample

- 0 = US only
- 1 = other western nation only
- 2 = other non-western nation
- 3 = cross-national _____

Model specification and research design

Time dimension

- 0 = cross-sectional
- 1 = longitudinal _____

if longitudinal:

time lag (# of mths) _____

Dependent variable

- 1 = overall violent crime
- 2 = overall property crime
- 3 = robbery
- 4 = burglary
- 5 = homicide/murder
- 6 = rape/forcible sex assault
- 7 = aggravated assault
- 8 = all index offenses
- 9 = violent delq. rates
- 10 = property delq. rates
- 11 = overall delq. rates
- 12 = theft/larceny (adult) _____

Logged or unlogged dependent variable?

- 0 = unlogged
- 1 = logged _____

SD of the dependent variable _____

Appendix 11. Mean values for the independent variables for the analyses presented in Chapter Six by dependent variable (effect size estimate).

Methodological Characteristic	Racial Heterogeneity	SES	Residential Mobility	Family Disruption
Level of aggregation	2.823	2.812	1.596	2.905
U.S. origin	.947	.842	.865	.891
Variables from competing theories controlled	.879	.792	.750	.869
Time dimension (1 = longitudinal analysis)	.102	.040	.019	.146
Ratio of sample size to independent variables	29.819	26.947	27.960	29.221
Violent crime dependent variable	.523	.525	.423	.642
Race-specific model	.042	.079	--	.080
Reciprocal effects estimated	.917	.852	.981	.978
Controls for inequality included	--	--	--	--
Controls for poverty included	--	--	--	--
Controls for routine activities included	--	--	--	--
Time lag (in months)	--	--	--	--
Identification restriction used	--	--	--	--
Policing measure	--	--	--	--
Social support measure	--	--	--	--

Appendix 11. Continued

Methodological Characteristic	Poverty	Inequality	HHR	Unemp.
Level of aggregation	3.059	3.551	3.946	3.848
U.S. origin	.948	.838	--	.902
Variables from competing theories controlled	.935	.922	.676	.740
Time dimension (1 = longitudinal analysis)	.013	.072	.270	.343
Ratio of sample size to independent variables	20.972	20.767	16.505	33.248
Violent crime dependent variable	.634	.671	.460	.500
Race-specific model	--	--	--	--
Reciprocal effects estimated	--	--	--	--
Controls for inequality included	.183	--	.378	.157
Controls for poverty included	--	.287	--	--
Controls for routine activities included	--	--	--	--
Time lag (in months)	--	--	--	4.317
Identification restriction used	--	--	--	--
Policing measure	--	--	--	--
Social support measure	--	--	--	--

Appendix 11. Continued

Methodological Characteristic	Incar.	Police	Soc. Support	Urbanism	South
Level of aggregation	4.891	2.939	3.979	3.073	3.372
U.S. origin	.870	.956	.723	.910	--
Variables from competing theories controlled	.761	.719	.745	.944	.991
Time dimension (1 = longitudinal analysis)	.217	.456	.340	.079	.091
Ratio of sample size to independent variables	17.563	69.949	27.227	29.545	25.305
Violent crime dependent variable	.391	.447	.511	.618	.691
Race-specific model	--	--	--	--	--
Reciprocal effects estimated	.761	.614	--	--	--
Controls for inequality included	--	--	.170	--	.327
Controls for poverty included	--	--	.319	--	.455
Controls for routine activities included	.435	--	.319	--	--
Time lag (in months)	--	--	--	--	--
Identification restriction used	--	.833	--	--	--
Policing measure	--	.675	--	--	--
Social support measure	--	--	.255	--	--