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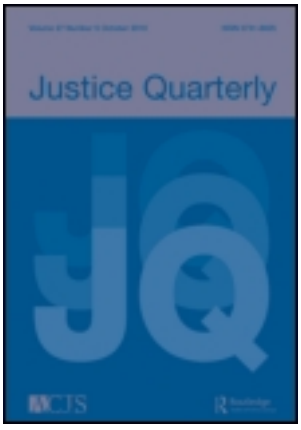

Justice Quarterly
Publication details, including instructions for authors and subscription information:
http://www.tandfonline.com/loi/rjqy20

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Published online: 25 Nov 2009.

To cite this article: Travis C. Pratt , Francis T. Cullen , Christine S. Sellers , L. Thomas Winfree Jr. , Tamara D. Madensen , Leah E. Daigle , Noelle E. Fearn & Jacinta M. Gau (2010) The Empirical Status of Social Learning Theory: A Meta-Analysis, Justice Quarterly, 27:6, 765-802, DOI: 10.1080/07418820903379610

To link to this article: http://dx.doi.org/10.1080/07418820903379610

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JUSTICE QUARTERLY VOLUME 27 NUMBER 6 (DECEMBER 2010)

The Empirical Status of Social Learning Theory: A Meta-Analysis

Travis C. Pratt, Francis T. Cullen, Christine S. Sellers, L. Thomas Winfree, Jr., Tamara D. Madensen, Leah E. Daigle, Noelle E. Fearn and Jacinta M. Gau

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Social learning theory has remained one of the core criminological paradigms over the last four decades. Although a large body of scholarship has emerged testing various propositions specified by the theory, the empirical status of the theory in its entirety is still unknown. Accordingly, in the present study, we subject this body of empirical literature to a meta-analysis to assess its empirical status. Results reveal considerable variation in the magnitude and stability of effect sizes for variables specified by social learning theory across different methodological specifications. In particular, relationships of crime/deviance to measures of differential association and definitions (or antisocial attitudes) are quite strong, yet those for differential reinforcement and modeling/imitation are modest at best. Furthermore, effect sizes for differential association, definitions, and differential reinforcement all differed significantly according to variations in model specification and research designs across studies. The implications for the continued vitality of social learning in criminology are discussed.

Keywords social learning theory; differential association; meta-analysis

In 1969, Hirschi's *Causes of Delinquency* did much to redefine the theoretical enterprise in criminology. Although exceptions existed (see, most notably, Shaw & McKay, 1929), the field was largely populated with elegant theories presented in the absence of empirical data (e.g., Cloward & Ohlin, 1960; Cohen, 1955; Sutherland, 1939). In contrast, Hirschi outlined an alternative strategy for advancing criminological knowledge: define theories clearly; derive testable propositions; measure the theories' variables and design studies to assess these propositions empirically; and support or reject the extant theories based on the resulting data. As he commented in *Causes of Delinquency*:

ISSN 0741-8825 print/1745-9109 online/10/060765-38
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DOI: 10.1080/07418820903379610

> As anyone who has tried it knows, it is easier to construct theories "twenty years ahead of their time" than theories grounded on and consistent with data currently available. But the day we could pit one study of delinquency against another and then forget them both is gone. We are no longer free to construct the factual world as we construct our explanations of it. As a consequence, our theories do not have the elegance and simplicity of those of an earlier period. I take consolation in the certain hope that they are somehow nearer the truth. (Hirschi, 1969, Preface)

To a large extent, in the intervening years, criminologists have followed Hirschi's admonition to test rival theories, publishing—literally—hundreds of studies. But embedded in Hirschi's work was the expectation that the process

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of matching theories to the "factual world" would result in some theories being endorsed and others discarded. Criminologists, however, have been reluctant to forfeit many theories, even as newer ones have come on the scene (Cullen & Agnew, 2006; Lilly, Cullen, & Ball, 2007; Vold, Bernard, & Snipes, 1998). As Vold et al. (1998) noted, "criminology has been blessed (or cursed, depending on one's point of view) with a very large number of scientific theories" (p. 3).

Theories can persist for many reasons: they provide research "puzzles" that can yield publications; they resonate with criminologists' experiences and professional ideology; they have politically palatable policy implications (Cole, 1975; Lilly et al., 2007; Pfohl, 1985). Yet another consideration is that despite a large body of empirical research, it is difficult to conclude which theories should be retained and which should be discarded. Theories are often stated discursively—rather than as a set of formal, testable propositions—and datasets do not always contain measures that allow for complete or definitive tests of competing theories (Cao, 2004). But the failure to relegate some theories to the criminological dustbin while placing more faith in alternative theories is also due to another salient factor: the failure of criminologists to *systematically* review the existing research studies that test the merits of competing theoretical paradigms.

Within criminology today, there is a general movement to organize the extant empirical research and to "take stock" of what we do and do not know (Cullen, Wright, & Blevins, 2006b). This movement is more advanced in, and has had an important impact on, the areas of correctional intervention and crime prevention (see, e.g., Andrews & Bonta, 2006; Cullen & Gendreau, 2000; Lipsey & Cullen, 2007; Losel, 1995; MacKenzie, 2006; Sherman, 2003; Sherman, Farrington, Welsh, & MacKenzie, 2002; Welsh & Farrington, 2001). Scholars in these areas have called for an "evidence-based" approach to intervention/prevention programs based on "systematic reviews." They have also tended to advocate the use of meta-analysis as a useful technique to quantitatively synthesize large and diverse bodies of empirical knowledge.

Students of criminological theory, however, have been slower to take up the challenge of systematically reviewing contemporary perspectives and, in particular, of using meta-analytic techniques to synthesize the knowledge that has accumulated. Some useful reviews have appeared in the literature (see, e.g., Burton & Cullen, 1992; Kempf, 1993; Nagin, 1998; Paternoster, 1987; Pratt & Cullen, 2000, 2005; Pratt, Cullen, Blevins, Daigle, & Madensen, 2006; Walters, 1992; see also Kubrin, Stucky, & Krohn, 2009, and the reviews in Cullen et al., 2006b; Thornberry & Krohn, 2003), yet more often, reviews of the extant research on crime theories are found either in the introductory sections to journal articles or in textbooks (see, e.g., Agnew, 2001; Akers & Sellers, 2009; Cao, 2004). These assessments are helpful, but they tend to be selective rather than systematic. They focus on the more prominent studies and tend to offer general conclusions as to whether, on balance, the evidence is favorable or unfavorable to a given theory. The assessments thus are broad rather than precise.

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In this context, the current study was undertaken to help fill this void in the research literature by reporting a meta-analysis of empirical tests of social learning theory. This perspective, which overlaps with and extends Sutherland's differential association model, is generally considered to be one of the major contemporary theories of crime (Agnew, 2001; Cao, 2004; Cullen & Agnew, 2006). Nevertheless, to date, the extant studies have not been subjected to quantitative synthesis. We present such a meta-analytic assessment that explores the empirical status of social learning theory and, hopefully, offers direction for future theoretical and empirical exploration.

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Assessing Social Learning Theory

Summary of Theoretical Principles

As specified by its most notable advocate—Ronald Akers—social learning theory is an explicit effort to extend Sutherland's "differential association" perspective. As Akers (2001) observed, "social learning theory retains all of the differential association processes in Sutherland's theory" (p. 194), but then adds additional considerations. This perspective is well-known, in part, because Akers has summarized the details of the model on a variety of occasions (see, e.g., Akers, 1998, 2001; Akers, Krohn, Lanza-Kaduce, & Radosevich, 1979; Akers & Sellers, 2009; see also, Akers & Jensen, 2003b). Yet to establish a context for the meta-analysis to follow, we will briefly recount the essential details of social learning theory.

Akers embraced Sutherland's central proposition that crime is learned through social interaction. Within any society, people vary in their exposure to behavioral and normative patterns through their associations with others (thus the notion of *differential association*). Differential association with others shapes the individual's definitions, described by Akers (2001) as "one's own attitudes or meanings that one attaches to given behavior" (p. 195). Moving beyond Sutherland, Akers further argued that definitions may be general (broadly approving or disapproving of crime) or specific to a particular act or situation. Definitions may also be negative (oppositional to crime), positive (defining a criminal behavior as desirable), or neutralizing (defining crime as permissible).

Unlike Sutherland who downplayed the role of modeling, Akers (2001) held that crime—especially when first initiated—can be influenced by *imitation*, which he defined as "the engagement in behavior after the observation of similar behavior in others" (p. 196). Even so, the key, novel explanatory concept added by Akers was *differential reinforcement*, which he defined as "the balance of anticipated or actual rewards and punishments that follow or are consequences of behavior" (p. 195). Acts that are reinforced—either by the reward or the avoidance of discomfort—are likely to be repeated, whereas acts that elicit punishment are less likely to be repeated. Although

reinforcement can be physical (e.g., bodily changes from taking drugs), Akers contended that the most important reinforcers are *social* (e.g., those coming from members of one's intimate social group). The stability of criminal behavior is therefore more likely when an individual is embedded in a social environment where misconduct is reinforced and where differential association with pro-criminal definitions and behavioral patterns is readily available.

Empirical Assessments of Social Learning Theory

The core constructs of Akers's social learning theory are differential association, definitions, imitation, and differential reinforcement. In a sequence that unfolds over time, individuals first initiate criminal acts (mainly through differential association and imitation) and then learn either to cease or to persist in their offending (mainly through differential reinforcement). Nevertheless, existing studies generally do not explore the full "social learning process" outlined by Akers (2001, pp. 196–198). Instead, depending on the measures in the dataset under inspection, studies typically explore how one or more of these four constructs are independently related to delinquent or criminal involvement. To the extent that positive associations between the social learning constructs and misconduct are revealed, support for the theory is inferred.

Until recently, the standard approach for assessing studies in criminology (and other social sciences) was the "narrative review." A narrative review involves an investigator collecting, reading, and "making sense" of a body of research studies. The articles may be categorized into two groups: those finding or not finding that a theoretical variable has a statistically significant association with the dependent variable. This is called a "ballot-box" approach, because the procedure involves counting "ballots" in favor or against a theory. Or the investigator might simply review each study and then attempt to draw conclusions about whether the theory under consideration seems to be more or less supported overall and in specific ways. This approach is largely qualitative, because it relies on the judgment of the investigator as to what all the studies, taken as a whole, actually mean (Rosenthal & DiMatteo, 2001).

In general, narrative reviews have reached favorable conclusions about social learning theory and its incorporated predecessor, differential association theory (see Akers, 1998, 2001; Akers & Jensen, 2006; Akers & Sellers, 2009), yet such support is not unqualified (see the discussion by Gottfredson & Hirschi, 1990, regarding the potential spuriousness of the relationships to crime of the variables specified by social learning theory). Cao (2004), for example, concluded that social learning theory has "received empirical support" and that whether "tested alone, or with other criminological theories," the perspective's "main arguments are largely supported" (p. 97). Although noting that the theory has

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been disproportionately tested with regard to substance abuse and minor forms of deviance, Warr (2002) nonetheless stated that "the evidence for social learning theory is extensive and impressive" (p. 78). Kubrin et al. (2009) noted that "key issues remain," but that the findings from the "large body of research" conducted on the theory are "consistent with this approach" and thus that "social learning theories have earned an important place among individual explanations of crime" (p. 164). Consistent with these conclusions, Agnew (2001) ended his narrative review by noting that "social learning theory has much support" and by calling the perspective a "leading" explanation of delinquency (p. 103).

Although still in short supply, evidence from meta-analyses offers further confirmation that social learning theory may have a solid empirical foundation. First, in a meta-analysis of Gottfredson and Hirschi's (1990) general theory of crime, Pratt and Cullen (2000) assessed studies that included measures of both self-control and social learning theory. Although based on a limited number of studies, the meta-analysis revealed positive effect sizes for differential association (.232) and for delinquent definitions (.175) that rivaled the effect sizes for self-control and, more broadly, for other leading predictors of criminal involvement (see, e.g., Lipsey & Derzon, 1998). Second, other meta-analytic studies have quantitatively synthesized the existing research on predictors of crime, recidivism, and institutional misconduct. They have found that variables identified by social learning theory—antisocial attitudes and antisocial peer/parental associations—are among the strongest predictors of offending behavior (Andrews & Bonta, 2006; Gendreau, Goggin, & Law, 1997; Gendreau, Little, & Goggin, 1996; Lipsey & Derzon, 1998).

A third source of support for social learning theory can be inferred from the evaluation research on correctional rehabilitation interventions with offenders. Cressey (1955) long ago observed the significance of differential associations for changing offenders. More recent experimental and quasi-experimental studies can be seen as empirical tests of criminological theories (Cullen, Wright, Gendreau, & Andrews, 2003). Either explicitly or implicitly, interventions are based on an underlying theory of crime because they select for change certain risk factors that are believed to cause the offending conduct. If reductions in the risk factors subsequently lead to reductions in the misbehavior, then support for the theory is merited; by contrast, changes in the risk factors that do not effect changes in offending can be taken as evidence contrary to a theory. In this context, it is noteworthy that treatment interventions consistent with social learning theory—especially cognitive-behavioral programs—that target for change antisocial attitudes, thinking, and associations are among the most effective in achieving reductions in offending (Andrews & Bonta, 2006; Cullen et al., 2003; Lipsey, Chapman, & Landenberger, 2001; MacKenzie, 2006). These results thus provide experimental and quasi-experimental evidence supportive of social learning theory (Cullen et al., 2003).

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Research Strategy

The existing research lends credence to the conclusion that social learning theory has achieved empirical support from several quarters. This observation encourages further efforts to confirm social learning theory's status as a worthy criminological theory, hopefully in a way that is more complete, precise, and illuminating for future research. In this regard, the current study attempts to move toward this goal in four respects.

First, our research strategy starts with the attempt to accumulate a more systematic sample of studies than has been used in previous reviews. This approach involves including in the analysis all studies measuring social learning variables that were published in the leading criminal justice/criminology journals from 1974 to 2003.[1] In all, we consider results published in 133 studies. As noted previously, we review the sample of studies using the statistical technique of meta-analysis. Although initially invented in 1904 by Karl Pearson, meta-analysis was not used extensively in medicine, education, sciences, and the social sciences until the last two decades (Hunter & Schmidt, 1996; Rosenthal & DiMatteo, 2001). Meta-analysis is now a standard approach for reviewing studies, and thus its strengths and weaknesses do not need to be detailed here (for such a discussion, see Rosenthal & DiMatteo, 2001). We will note, however, that its main advantage is that it helps to mitigate four issues that challenge the utility of more traditional narrative reviews: the potential for investigator bias, in a qualitative review, to influence the interpretation of results; the failure to specify the criteria used to review studies, thus making replication of the review difficult, if not impossible; the challenge of handling the substantial amount of information inherent in a large body of research studies; and the inability to furnish more precise point estimates that makes it difficult to compare theoretical variables within and across theoretical paradigms (Rosenthal & DiMatteo, 2001).

As with all meta-analyses, our assessment is restricted by the quality of the studies included in the sample. When reviews are based on a sample of studies that is limited in size or methodological rigor, the conclusions that are drawn from the meta-analysis are suspect (this is the "garbage in and garbage out" problem; see, e.g., Rosenthal & DiMatteo, 2001). Scholars have argued, however, that this problem is most likely to plague meta-analyses in which a large portion of the sample of studies being assessed comprised either unpublished works or studies published in non-peer-reviewed outlets where the quality of such work may be suspect (Pratt, 2002). Thus, we argue here that

1. 2003 was considered as a cut-off date because it marked the publication of Akers and Jensen's (2003b) edited volume in the *Advances in Criminological Theory* series, which was the last major theoretical and empirical statement of the theory.

because we use a sample of studies that is large and has been vetted by the peer review process, this potential problem is diminished.[2]

Second, our research strategy also attempts to organize the meta-analysis around the key components of social learning theory: differential association, definitions, differential reinforcement, and modeling/imitation. This approach is intended to assess the absolute and relative influence of these theoretical components of social learning theory on delinquent and criminal involvement. Toward this end, we calculate the mean effect size estimates for the variables delineated by social learning theory. This analysis is intended to illuminate which portions of the theory are best supported by the literature, and also which elements of the theory have yet to be adequately tested by researchers.

Third, a valuable feature of meta-analysis is that it allows for the assessment of the extent to which certain key methodological factors may condition the outcomes rendered by theoretical variables. Considering these "moderating variables" is important because it has implications for whether the effect size estimates, we report reflect empirical reality or are mainly a methodological artifact (for example, whether the source of social influence is measured as peer versus parental reinforcements; whether studies separate versus combine components of social learning theory; whether studies include statistical controls for variables specified by competing theories; whether studies are cross-sectional versus longitudinal, and so on). In the current study, we explore how the social learning effect size estimates are or are not shaped by these methodological issues.

Finally, we wish to emphasize that the current meta-analysis—similar to all meta-analyses—is not being offered as the definitive test or final word on social learning theory. As with any empirical investigation, replication is encouraged. This process is facilitated with meta-analysis because the coding decisions in this approach are "public." Further, the database for a meta-analysis is cumulative.

2. We recognize that using only published work in a meta-analysis brings with it a certain degree of controversy over the potential inferential errors that could be made concerning "publication bias" (see Rosenthal, 1979); in particular, that the effect sizes may be inflated because of the tendency of studies revealing non-significant relationships to be more likely to be either rejected for publication or to remain unsubmitted to journals by authors. Nevertheless, the two works that scholars typically cite as evidence of this potential problem are quite problematic themselves. For example, Cooper, DeNeve, and Charlton's (1997) survey of 33 psychologists found that null findings (which was not a mutually exclusive response category) were actually rarely invoked as an explanation among scholars for why they believed their work was not eventually published—lack of researcher interest and methodological problems were by far the most common reasons cited by their respondents. Second, Gerber and Malhotra's (2008) assessment of publication bias in sociology journals was only able to assess 14% of the studies that were originally gathered. Indeed, the remaining 86% of those studies failed to meet the inclusion criteria necessary for the statistical method the authors chose to use. Thus, in an odd bit of methodological irony, it is difficult for Gerber and Malhotra (2008) to rule out in their own study the very form of publication bias that they were trying to expose. Taken together, we are confident that the large sample of studies we assess here does not suffer from any form of publication bias that would not also be present in any other review—narrative or quantitative—of the criminological literature. Further, at the very least, our meta-analysis provides a baseline of results that future research can built upon using a wider sample of studies, including unpublished works. In the interim, we furnish the most systematic quantitative synthesis of findings on social learning theory.

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As additional studies are conducted and published, they can be added to the sample of studies and the empirical relationships of interest can be reassessed. For those remaining skeptical of the results of the meta-analysis, another option remains: assess the potential shortcomings of the analysis, and then undertake a new, rigorous individual study that attempts either to falsify or to revise the conclusions reported in the meta-analysis. In this way, meta-analyses may prove useful not only for organizing existing empirical knowledge but also for prompting and/or guiding future research.

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Methods

Sample of Studies

We extended our search for empirical tests of social learning and/or differential association theories historically to 1966, when Burgess and Akers published an initial version ("differential association-reinforcement theory") of what was later to be known generally as social learning theory (Akers, 1973). The search used a variety of strategies. We began with a general search of *Sociological Abstracts*, *Psychological Abstracts*, and *Criminal Justice Abstracts* with the key phrase "social learning." This strategy produces an exorbitantly high number of abstracts related more generally to Bandura's version of social learning theory, but not specifically to Akers's social learning theory of deviance. We then undertook a series of more narrow searches, including a search of the aforementioned indexes for the phrase "differential association," which yielded articles related almost exclusively to either Sutherland's differential association theory or Akers's social learning theory. To produce articles more specifically testing Akers's social learning theory, we searched the *Social Sciences Citation Index* for articles that had cited seminal works that had advanced the theoretical statement of social learning over the years (Akers, 1998, 2001; Akers et al., 1979; Burgess & Akers, 1966). Since the *Social Sciences Citation Index* has not indexed consistently all the leading journals in criminology, we also examined manually all issues of *Criminology*, *Justice Quarterly*, and *Journal of Research in Crime and Delinquency* for tests of social learning and/or differential association theory.[3] Finally, a published volume of *Advances in Criminological Theory* devoted specifically to social learning theory (Akers & Jensen, 2003b) was searched for empirical tests of social learning theory as well.

Regardless of the search strategy employed, all articles were selected for the meta-analysis if they were empirical articles deliberately intended to test the empirical validity of social learning or differential association theory. This

3. These journals were selected because they have been consistently ranked as being among the "top tier" journals in the field of criminology and criminal justice in terms of journal prestige (see Sorensen, Snell, & Rodriguez, 2006).

search and selection procedure produced a total of 133[4] empirical studies published between 1974 and 2003 that tested social learning/differential association theory alone, in competition with other theories, or within an integrated theoretical model. These 133 empirical studies,[5] which generated 246 statistical models, contained a total of 704 effect size estimates, representing the

4. The studies included in the sample for the meta-analysis are those by: Adams (1974), Agnew (1991, 1993), Akers and Cochran (1985), Akers and Lee (1996, 1999), Akers et al. (1979), Akers, Greca, Cochran, and Sellers (1989), Alarid, Burton, and Cullen (2000), Ary, Tildesley, Hops, and Andrews (1993), Aseltine (1995), Bahr, Hawks, and Wang (1993), Bahr, Marcos, and Maughan (1995), Bellair, Roscigno, and Vélez (2003), Benda (1994, 1999), Benda and Corwyn (2002), Benda and DiBlasio (1991, 1994), Benda and Toombs (1997), Benda and Whiteside (1995), Benda, DiBlasio, and Kashner (1994), Boeringer, Shehan, and Akers (1991), Brezina (1998), Brezina and Piquero (2003), Brownfield and Thompson (1991, 2002), Bruinsma (1992), Burke et al. (1987), Burkett and Warren (1987), Burton, Cullen, Evans, and Dunaway (1994), Byram and Fly (1984), Costello and Vowell (1999), Covington (1988), Curran, White, and Hansell (1997), Damphousse and Crouch (1992), DiBlasio (1986, 1988), DiBlasio and Benda (1990), Dull (1983), Durkin, Wolfe, and Phillips (1996), Edwards (1992), Ellickson and Hays (1992), Ennett and Bauman (1991), Fagan, Weis, and Cheng. (1990), Farrell and Danish (1993), Flay et al. (1994), Goe, Napier, and Bachtel (1985), Gottfredson, McNeil, and Gottfredson (1991), Griffin and Griffin (1978a, 1978b), Grube, Morgan, and Seff (1989), Hawkins and Fraser (1985), Hayes (1997), Heimer (1997), Heimer and DeCoster (1999), Heimer and Matsueda (1994), Hochstetler, Copes, and DeLisi (2002), Hoffman (2002), Hunter, Baugh, Webber, Sklov, and Berenson (1982), Hwang and Akers (2003), Jackson, Tittle, and Burke (1986), Jaquith (1981), Jarjoura and May (2000), Jiang and D'Apolito (1999), Johnson (1988), Johnson, Marcos, and Bahr (1987), Kaplan, Martin, and Robbins (1984), Krohn, Lanza-Kaduce, and Akers (1984), Krohn, Skinner, Massey, and Akers (1985), Krohn, Lizotte, Thornberry, Smith, and McDowall (1996), Kruttschnitt, Heath, and Ward (1986), Lanza-Kaduce and Capece (2003), Lanza-Kaduce and Klug (1986), Lewis, Sims, and Shannon (1989), Lo (1995, 2000), Makkai and Braithwaite (1991), Marcos, Bahr, and Johnson (1986), Massey and Krohn (1986), Matsueda (1982), Matsueda and Anderson (1998), Matsueda and Heimer (1987), McCarthy (1996), McCarthy and Hagan (1995), McGee (1992), Mears, Ploeger, and Warr (1998), Melby, Conger, Conger, and Lorenz (1993), Menard (1992), Michaels and Miethe (1989), Morash (1983), Napier, Goe, and Bachtel (1984), Neapolitan (1981), Needle, Su, Doherty, Lavee, and Brown (1988), Nusbaumer, Mauss, and Pearson (1982), Orcutt (1987), Paternoster and Triplett (1988), Reed and Rose (1998), Reed and Rountree (1997), Ried (1989), Reinarman and Fagan (1988), Rodriguez and Weisburd (1991), Sellers and Winfree (1990), Sellers, Cochran, and Winfree (2003), Simons, Miller, and Aigner (1980), Skinner and Fream (1997), Smith and Brame (1994), Smith and Paternoster (1987), Sommers, Fagan, and Baskin, (1994), Strickland (1982), Thornberry, Lizotte, Krohn, Farnworth, and Jang (1994), Wang and Jensen (2003), Warr (1993), Warr and Stafford (1991), White and LaGrange (1987), White, Johnson, and Horwitz (1986), Wiltfang and Cochran (1994), Winfree and Bernat (1998), Winfree and Griffiths (1983), Winfree, Theis, and Griffiths (1981), Winfree, Griffiths, and Sellers (1989), Winfree et al. (1993), Winfree, Mays, and Vigil-Backstrom (1994), Winfree, Vigil-Backstrom, and Mays (1994), Winslow, Franzini, and Hwang (1992), and Wong (1998).

5. Several either implicit or explicit tests of social learning theory could not be included in our sample because effect size estimates could not be calculated. For example, some studies reported maximum-likelihood or metric estimates that could not be converted (Agnew & Huguley, 1989; Bailey & Hubbard, 1991; Jang, 1999; Lanza-Kaduce, Akers, Krohn, & Radosevich, 1984; Skinner, Massey, Krohn, & Lauer, 1985; Sommers, Fagan, & Baskin, 1993; Tittle, Burke, & Jackson, 1986; Warr, 1998; White & Bates, 1995). Other studies either provided no inferential statistics, or not enough descriptive information was reported (e.g., univariate descriptive statistics only) so that an effect size estimate could be calculated (Curcione, 1992; Fagan, Piper, & Moore, 1986; Kandel & Davies, 1991; Laner, 1985; Rouse & Eve, 1991; Shukla, 1976; Wood, Gove, Wilson, & Cochran, 1997). Still others estimated interaction effects only (Benda & Corwyn, 1997; Conger, 1976; Dembo, Grandon, La Voie, Schmeidler, & Burgos, 1986; Jensen & Brownfield, 1983; Linden & Fillmore, 1981; Weast, 1972) or did not assess a crime or deviance outcome measure (Capece & Akers, 1995; Gainey, Peterson, Wells, Hawkins, & Catalano, 1995; Kandel & Davies, 1991; Linden, Currie, & Driedger, 1985; Orcutt, 1978).

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integration of 118,403 individual cases. The number of statistical models exceeds the number of empirical tests because studies often estimated multiple statistical models (i.e., for different types of crime/deviance, with different model specifications). While this raises the issue of a potential lack of statistical independence (see, e.g., Pratt, 2000), we adjust for this possible threat in our analysis (see the discussion below).

Effect Size Estimate

Two possible proxies of an effect size estimate 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 & Olkin, 1985), which are typically drawn from each empirical study's correlation matrix (assuming one is provided).[6] 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 control statistically for potentially confounding variables in order to isolate the effects of the independent variables specified by a 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 Paternoster, 1987; Pratt, 1998; Pratt & Cullen, 2000; Tittle, Villemez, & Smith, 1978).

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:

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

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

6. 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 & Stouthamer-Loeber, 1986). Still other researchers have used the RIOC statistic (relative improvement over chance), which scales down certain descriptive statistics into a 2 × 2 table of whether or not a predictor variable is present and whether or not an individual engaged in delinquency (Loeber & Dishion, 1983). Both of these statistics, however, assume, at minimum, a quasi-experimental research design, and are therefore not applicable to synthesizing correlational research based on statistical control.

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standard deviations of the independent and dependent variables (Blalock, 1972; Hanushek & 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 estimates may be defended by acknowledging that the bivariate estimates are inflated. 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 the criminological research, taking this position would require making a random-error assumption that model misspecification plagues *all predictors of crime/deviance specified by social learning theory 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; the mean effect size estimates should remain uncontaminated and therefore identical in their *relative* aggregated magnitude (Hedges & Olkin, 1985).

There is little reason to believe, however, that this assumption will ever be met. To be sure, least-squares and maximum likelihood multivariate estimation techniques all begin with the basic understanding that model misspecification error contains, by definition, a systematic error component that cannot be treated solely as random fluctuations across studies that affect all predictors in exactly the same way (Hanushek & Jackson, 1977). Supporters of using multivariate effect size estimates when synthesizing non-experimental research are cognizant of this fact (see, e.g., Raudenbush, Becker, & Kalaian, 1988). Accordingly, they tend to view zero-order relationships as, at best, limited in their substantive meaning and, at worst, dangerously misleading (Pratt, 2000). 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 debate between using bivariate correlations versus multivariate effect sizes is likely to extend well into the future. Although it is not the purpose of the present analysis to make a declaration of methodological superiority for either position, the beta weights from each 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 meet the basic

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statistical assumptions more closely, surrounding random and systematic error.[7]

Using Fisher's *r* to *z* transformation (see Wolf, 1986), the effect size estimates from each empirical study (i.e., each test of the theory) were converted to a *z*(*r*) score.[8] The 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).[9] Normally distributed effect size estimates are necessary for the accurate determination of central tendency for the effect size estimates, and for unbiased tests of statistical significance (Hanushek & Jackson, 1977; Rosenthal, 1978, 1984).

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Predictor Domains

Given the statement of social learning theory by Akers (1973, 1998, 2001), variables related to differential association, definitions, differential reinforcement, and imitation were coded from each study.[10] The specific variables falling within each of these predictor domains are outlined below. For each predictor within each domain, dummy variables are coded as to whether its effect on crime/deviance was *statistically significant* in the theoretically expected direction in the statistical model. Finally, to facilitate moderator analyses, we coded how each of the predictors from social learning theory were measured and separate effect size estimates are calculated for each.[11]

Differential association

We coded the effect sizes into an overall estimate for the differential association predictors; in addition, the differential association predictors were also

7. Hedges' and Olkin's critique of using multivariate effect sizes in meta-analysis was certainly valid when it was raised over two decades ago. Since then, however, advances in both quantitative methods and computer software (in particular, Hierarchical Linear Modeling methods and software) have resulted in new approaches for handling this potential problem in methodologically defensible ways, including the approach taken here.

8. The *r* coefficient was also chosen not only for its ease of interpretation, but also because formulae are available for converting other test statistics (e.g., chi-square, *t*, *F*) into an *r*-value (see Wolf, 1986).

9. 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:

$$z\,(r) = 1.151 \log[1 + r\,/\,1 - r]$$

10. In 1998, Akers offered a further expansion of his social learning theory, providing what he viewed as the social-structural roots of the learning process. As of 2003, however, little research has been conducted on these elements and no results had entered the empirical literature as defined for this meta-analysis.

11. Three separate coders were used to code the sample of studies. A random sample of 20 studies was selected for coding by each coder, and reliability analysis was conducted with regard to the coding scheme. The inter-rater reliability coefficients across all dimensions of the analysis conducted here (e.g., effect size calculation, codes for moderating variables) exceeded .92.

coded separately from each empirical study according to how they were measured. These variables are related to the behaviors and attitudes of an individual's social network, including *peers' behaviors, parents' behaviors*, and *others' behaviors*. In addition, effect size estimates were coded from each study regarding *peers' attitudes, parents' attitudes*, and *others' attitudes*. If an empirical study did not separate the effects of these variables, but rather combined them into a single measure, the effect size of their relationship to crime/deviance was recorded as a *differential association index*.

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Definitions

We coded the effect sizes into an overall estimate for the definitions predictors; in addition, the definitions predictors were also coded separately from each empirical study according to how they were measured. Variables representing an individual's *antisocial or criminal beliefs/attitudes* were also coded when present. If an empirical study did not include such a specific item, but rather contained a composite of an individual's basic attitudinal beliefs, the effect size of a *definitions index* was recorded.

Differential reinforcement

We coded the effect sizes into an overall estimate for the differential reinforcement predictors. As with the previous components of social learning theory, the differential reinforcement predictors were also coded separately from each empirical study according to how they were measured. Different types of differential reinforcement were coded from empirical studies. These included *peer reactions, parental reactions, others' reactions*, and *rewards minus costs*; assuming such specific indicators were not used in the empirical study (e.g., indicators of peer and parental reactions combined), the effect size of a *differential reinforcement index* was coded.

Modeling/Imitation

Finally, we coded the effect sizes into an overall estimate for the modeling/imitation predictors. Furthermore, the modeling/imitation predictors were also coded separately from each empirical study according to how they were measured. Specifically, to assess the effect size of imitation on crime/deviance, variables indicating the *number of admired models witnessed* were coded from each empirical study. If such a specific measure of imitation was not used in an empirical study (e.g., unclear measurement strategies or indexes—as opposed to counts—of multiple potential models), the effect size of an *imitation index* was coded.

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Moderator Variables

Each empirical study was coded for a number of variables related to methodological variations that may condition the effect size of the social learning theory predictors on crime/deviance.

Sample characteristics

A number of factors related to the sample used in an empirical study were coded to assess their impact on the effect size estimates for the relationships specified by social learning theory. These characteristics include the *sampling frame* used by the study (general sample versus a school sample) as well as the sample's *gender composition* (coded as a categorical variable for mixed gender, males only, or females only), *racial composition* (coded as a categorical variable for racially heterogeneous, whites only, non-whites only, black only, Hispanic only, or black and white only), and *age composition* (coded as a categorical variable for juveniles, young adults aged 17 or under, adults, or juvenile/adult combination).

Model specification and research design

A number of factors related to each empirical study's model specification and research design were also coded to assess their impact on the effect size estimates for the relationships specified by social learning theory. Among these factors were whether variables from *competing criminological theories* were controlled in the analysis. Separate dummy variables were also coded for specific theories, including *self-control, social bond/control, classic strain, general strain, routine activity/opportunity, rational choice/deterrence*, and *labeling theories*. Other moderating variables included whether the research design was *cross-sectional or longitudinal*, and each empirical study's *dependent variable* was recorded (the categories include violent crime, property crime, drug use, alcohol use, sexual assault, theft, general crime/delinquency, vandalism, or other).

Issues in Effect Size Calculation

The choice of effect size presented us with two potential methodological problems, both of which are correctable. First, a potential bias may occur when coding multiple effect size estimates from the same dataset as we do here (i.e., the lack of statistical independence). As noted, the sample of 133 studies, which contributed 704 effect size estimates, were based on 85 independent datasets. This means that multiple studies were published using a common data

source, that most of the studies contributed more than one effect size estimate for our analysis (e.g., the same study producing multiple estimates of effect size for multiple indicators of concepts specified by social learning theory), and that the number of effect size estimates drawn from individual studies varied in number.

We had two reasons for including multiple effect sizes from individual studies. First, and most important, selecting only one effect size estimate from each study would severely limit our ability to examine how methodological variations across the studies potentially influence the effect size estimates. Second, it would be difficult to develop a methodologically defensible decision rule for selecting one effect size estimate while ignoring others from the same study. According to Pratt and Cullen (2000), selecting only one effect size estimate from these different analyses "could introduce, wittingly or unwittingly, a 'researcher' bias" (p. 941).

We do, however, recognize that coding multiple effect size estimates from the same study can *potentially* introduce a measure of bias into the meta-analysis. To the extent that one study produces a greater number of effect size estimates than do other studies, it may disproportionately affect the mean effect size reported across the sample of studies in the meta-analysis. Furthermore, similar to any other dataset with a hierarchical structure, this problem may also create estimation errors in a meta-analysis by reducing the variance estimates in effect sizes across studies, which may therefore artificially increase the likelihood that the mean effect size estimates will turn out to be significantly different from zero (see Pratt, 2000).

The second potential problem is that multivariate effect sizes will vary within datasets according to the unique ways in which statistical models are specified (i.e., how the variance–covariance matrix will vary according to the specific set of covariates in a multivariate model; see Hanushek & Jackson, 1977). Since it is a practical impossibility to develop a coding scheme that would fully capture every permutation of model specification across the 133 studies assessed here, critics of the use of multivariate effect sizes have contended that the estimation problems associated with variations in model specification are insurmountable (see, e.g., Hedges & Olkin, 1985). Recent methodological developments and software availability, however, have allowed meta-analysts to accommodate the variations in effect sizes introduced by differences in multivariate model specification by treating such variation as unobserved heterogeneity, which can be modeled statistically and incorporated into the calculation of overall effect size estimates (Pratt, Cullen et al., 2006)—an approach we adopted here.

To do so, the correction we employed for these potential sources of bias involved calculating our mean effect size estimates using a multilevel modeling strategy (HLM 6.06). The HLM multilevel modeling program offers analyses called "Variance-Known" (or simply "V-Known") models for analyzing meta-analytic data (see Hox, 1995; Raudenbush & Bryk, 2002). Individual effect sizes are specified as the Level 1 units and the studies from which the effect sizes were derived are treated as Level 2 units such that effect sizes are nested

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within studies. The variances for individual effect sizes are computed[12] and entered into the HLM model. Study characteristics can be entered as Level 2 covariates to control for their possible impacts upon effect sizes.

It is traditional to begin a multilevel modeling analysis by estimating an unconditional or intercept-only model to determine if there is significant Level 2 variability, but these significance tests can fail to pick up real effects when Level 1 sample sizes are small (Hox, 1995). Given that some of our effect size samples were low by statistical standards, we opted to forego the unconditional models and to skip to the introduction of the study-level covariates. These Level 2 predictors were each study's *sample size* and the *number of effects* produced by a given study. Each of these variables were grand mean centered to correct for the fact that neither of them have a meaningful zero point (i.e., effect sizes cannot be produced by samples with $n = 0$, nor can an effect size be derived from a study that yielded no effect sizes). The resulting "mean" effect size estimates presented in Table 1, then, are actually model intercepts and should be interpreted as the expected value of an effect size when the sample size and the number of effects per study are held constant at their respective means.

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Analytic Strategy

Our analysis proceeded in two stages. First, we estimated mean effect sizes for each of the predictors of crime/deviance specified by social learning theory. These analyses are intended to assess the relative magnitudes of the "strength of effects" between these variables and criminal/deviant behavior across the 133 studies. Second, we conducted a series of moderator analyses for each of these predictors specified by social learning theory to determine the degree to which their effects are, or are not, robust across various methodological specifications. These analyses are intended to complement the meta-analysis in that they can assess the "stability of effects" of these relationships across studies.

Results

Strength of Effects

Table 1 contains the mean effect size estimates for all of the predictors specified by social learning theory. This table also contains the percentage of the

12. The Level 1 effect size variances are computed using the formula for estimating the standard error of the Fisher *r*-to-*z* transformation (see, e.g., Hox, 1995; Lipsey, & Wilson, 2001):

$$\sigma_j^2 = 1/(n_j - 3)$$

The full model is estimated with the equation:

$$z_j = \gamma_0 + \gamma_1(\text{study } n) + \gamma_2(\text{number of effect sizes}) + u_j + e_j$$

statistical models where the effect of each predictor was statistically significant across studies and the confidence intervals for the variables within each predictor domain. As can be seen in Table 1, the overall effect size for the *differential association* predictors is quite robust (Mz = .225, $p < .001$)[13]. This overall effect size is also based on a large number of contributing effect sizes (385), which gives us further confidence in the predictive strength of these measures of crime and deviance. Nevertheless, there is significant variation in the mean effect sizes of the relationships between differential association variables and crime/deviance, where these effect size estimates range from the considerably robust predictors of peers' behaviors (Mz = .270, $p < .001$)[14] and a

Table 1 Mean effect size estimates and statistical diagnostics for social learning theory predictors

Social learning theory predictor	% Significance	Mz	95% C.I.
Differential association overall (385)[1]	71.9	.225***	.213–.237
Peers' behaviors (166)	80.1	.270***	.268–.273
Parents' behaviors (27)	70.4	.143***	.133–153
Others' behaviors (16)	75.0	.104**	−.090–.118
Peers' attitudes (80)	58.8	.132***	.128–136
Parents' attitudes (31)	32.3	.025	.018–.031
Others' attitudes (4)	25.0	.049	.024–.074
Differential association index (61)	90.2	.406***	.398–.414
Definitions overall (143)[1]	77.6	.218***	.213–.223
Antisocial attitudes/definitions (121)	78.5	.202***	.199–.204
Definitions index (22)	72.7	.308***	.282–.334
Differential reinforcement overall (132)[1]	56.1	.097**	.089–.105
Peer reactions (46)	54.3	.083***	.077–.088
Parental reactions (29)	44.8	.061*	.050–.072
Others' reactions (7)	57.1	−.015	−.039–.009
Rewards minus Costs (30)	60.0	.116***	.107–.125
Differential reinforcement index (20)	70.0	.192***	.178–.206
Modeling/Imitation overall (30)[1]	46.7	.103**	.091–.115
Number of admired models			
Witnessed (15)	33.3	.072*	.057–.087
Differential imitation index (15)	60.0	.134**	.117–.151

*Statistically significant at $p < .05$; **statistically significant at $p < .01$; ***statistically significant at $p < .001$.

[1]Effect sizes differed significantly ($p < .05$) according to measure used.

Note. Numbers in parentheses = number of contributing effect size estimates (Mz = overall mean effect size estimate).

13. The confidence intervals for the overall effect size estimates were calculated according to the random effects method, recently advocated by Schmidt, Oh, and Hayes (2009).

14. While numerous methods for significance testing exist for meta-analysis (see, e.g., Hedges & Olkin, 1985), the significance tests in the present case are based on the most conservative method using a "t" distribution for significant values of *r* (Blalock, 1972).

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differential association index (Mz = .406, $p < .001$) to the more moderate predictors of parents' behaviors (Mz = .143, $p < .001$) and peers' attitudes (Mz = .132, $p < .001$), to the weak (and mostly non-significant) predictors of others' attitudes (Mz = .049, $p > .05$), parents' attitudes (Mz = .025, $p > .05$), and others' behaviors (Mz = .104, $p < .01$). Overall, therefore, the indicators of the effect of differential association on crime/deviance tend to be strongest when measured as peers' behaviors or a differential association index across empirical studies. In addition, Table 1 indicates that these measures were the most consistently statistically significant predictors of crime/deviance in their respective statistical models (peers' behaviors and the differential association index are statistically significant in 80.1% and 90.2% of the statistical models, respectively).

Table 1 also contains the mean effect size estimates for the effect of *definitions* variables on crime/deviance. The results indicate that the overall effect size of the definitions predictors is also rather robust at Mz = .218 ($p < .001$), which was also generated by a large number of contributing effect sizes (143). Furthermore, although Table 1 indicates that the effects of definitions variables differ significantly according to how they are measured across studies, both of the types variables used to proxy the effect of definitions on crime/deviance are quite strong in magnitude, and both were statistically significant in over 70% of the statistical models assessed. Indeed, these analyses reveal that the mean effect size of antisocial attitudes/definitions is Mz = .202 ($p < .001$), and the effect of a definitions index is Mz = .308 ($p < .001$). Despite the fact that the effect of a definitions index has a mean effect size that is stronger than that for antisocial attitudes/definitions variables, the mean effect size of the definitions index was generated by 22 contributing effect size estimates. Accordingly, the effect size of antisocial attitudes/definitions—while still sufficiently robust—should be viewed as more stable with 121 contributing effect size estimates.

Table 1 also contains the mean effect size estimates for the *differential reinforcement* predictor domain. Unlike the previous social learning theory predictors, the overall effect size of the differential reinforcement predictors was rather weak, although still statistically significant (Mz = .097, $p < .01$) and was also based on a large number of contributing effect sizes (132). Even so, similar to the effects of the differential association and definitions variables, there is significant variation in the mean effect size estimates for the differential reinforcement predictors on crime/deviance, where the effect sizes of these variables range from the relatively robust effects of a differential reinforcement index (Mz = .192, $p < .001$) to the more moderate effects of rewards minus costs (Mz = .116, $p < .001$), to the weak or even insignificant effects of peer reactions (Mz = .083, $p < .001$), parental reactions (Mz = .061, $p < .05$), and others' reactions (Mz = −.015, $p > .05$). Furthermore, the effects of these variables are less consistently significant predictors of crime/deviance, with "percent significant" scores ranging from a low of 44.8% to a high of 70.0%. Thus, on balance, the variables falling under the differential reinforcement

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predictor domain are weaker predictors of crime/deviance relative to the differential association and differential attitudes predictors.

Finally, as indicated in Table 1, the mean effect size estimates from variables indicating a *modeling/imitation* effect are modest at best. The overall mean effect size of the modeling imitation predictors was Mz = .103 ($p < .01$) and was based on 30 contributing effect sizes. These variables were statistically significant predictors of crime/deviance in less than 50% of the statistical models examined. These fairly weak effects were also consistent across different types of modeling/imitation measures used in the sample of studies, where the mean effect size of the number of admired models witnessed was .072 ($p < .05$) and an imitation index was .134 ($p < .01$). The relative weakness of the predictive strength of these variables may indicate that this portion of social learning theory may not be well supported by the existing empirical literature. Nevertheless, certain predictors falling under the differential association predictor domain—such as peers' behaviors (and possibly parents' behaviors and others' behaviors)—could be viewed as proxies of a modeling or imitation effect (e.g., see the discussion by Warr & Stafford, 1991). Moreover, as Akers (1973; see also Winfree & Griffiths, 1983; Winfree, Sellers, & Clason, 1993) observed, imitation occurs early in the learning process, and may be replaced by definitions by the time social scientists attempt to measure its presence, yielding only a residual form of imitation. In any event, of the social learning theory predictor domains discussed thus far, modeling/imitation has received the least amount of empirical support across existing studies.

Stability of Effects

Regarding the second stage of our analysis, Table 2 presents the results of our moderator analyses of the social learning predictors according to variations in sampling approaches across studies. These analyses were conducted via separate one-way analysis of variance (ANOVA) models for each predictor by each sampling characteristic (F-values are therefore reported in the table's columns). Accordingly, given these analyses, two issues are worthy of note, the first of which is that the effects of the predictors specified by social learning theory are generally robust across variations in sampling approaches across studies. Indeed, of the 55 ANOVA models that were estimated, only 10 revealed statistically significant F-values. Thus, the general trend revealed in Table 2 is that the variables specified by social learning theory appear to have "general effects," at least with respect to variations in sampling approaches.

Nevertheless, the second issue worthy of note is that there were several substantively important methodological conditioning effects observed in these analyses. For example, the sampling frame was important for measures of parents' attitudes, where the effects were weak and insignificant in school-based samples (Mz = .010, $p > .05$) and stronger in general samples (Mz = .137,

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Table 2 Moderator analyses for social learning theory predictors according to variations in sample characteristics

Social learning theory predictor	Sampling frame	Race of sample	Gender of sample	Age of sample
Differential association				
Peers' behaviors (166)	0.002	1.548	0.364	2.665*
Parents' behaviors (27)	0.099	1.040	1.059	4.816*
Others' behaviors (16)	0.041	0.728	0.866	0.771
Peers' attitudes (80)	0.020	0.801	5.811**	8.983***
Parents' attitudes (31)	6.671*	0.319	—	0.949
Others' attitudes (4)	0.043	—	—	—
Differential association index (61)	2.671	1.713	0.045	2.758
Definitions				
Antisocial attitudes/definitions (121)	2.271	0.739	2.447	3.904*
Definitions index (22)	12.673**	2.545	0.568	0.083
Differential reinforcement				
Peer reactions (46)	0.037	0.410	0.011	1.135
Parental reactions (29)	0.072	0.689	0.001	27.216***
Others' reactions (7)	—	0.159	—	—
Rewards minus Costs (30)	2.644	0.511	3.646*	3.258
Differential reinforcement index (20)	12.728**	0.150	0.054	1.768
Modeling/Imitation				
Number of admired models				
Witnessed (15)	0.239	0.660	1.515	0.191
Differential imitation index (15)	—	—	0.867	1.155

*Statistically significant at $p < .05$; **statistically significant at $p < .01$; ***statistically significant at $p < .001$.
Note. Numbers in parentheses = number of contributing effect size estimates. Numbers in columns are *F*-statistics from one-way ANOVA models.

$p < .05$) as well as for differential definition indexes (significantly weaker in school-based samples Mz = .231, stronger in general samples Mz = .658, yet both $p < .01$), and for differential reinforcement indexes (significantly weaker in school-based samples Mz = .135, stronger in general samples, Mz = .418, both $p < .05$). The gender composition of the sample was also important for indicators of peers' attitudes and rewards minus costs, both of which were significantly weaker and statistically insignificant in mix-gendered studies. Finally, the age composition of the sample was the most consistently relevant sample-based moderator in our analyses. Specifically, measures of peers' behavior were strongest in juvenile (Mz = .287, $p < .01$) and young adult samples (Mz = .337, $p < .01$); indicators of parents' behavior were weak and insignificant in juvenile samples (Mz = .127, $p > .05$); measures of peers' attitudes were statistically insignificant in juvenile samples yet were significantly stronger in samples of young adults (Mz = .459, $p < .01$); finally, indicators of both differential

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definitions and parents' reactions were statistically insignificant in studies using juvenile samples.

Table 3 contains the results of our moderator analyses with respect to variations in model specification and research designs across studies. Like the previous set of analyses, those presented in Table 3 also indicate that the effects of the predictors specified by social learning theory are generally robust across these methodological variations; of the 47 ANOVA models that were estimated, only five revealed statistically significant *F*-values. Thus, the general trend revealed in Table 3 is that the variables specified by social learning theory once again appear to have "general effects," at least with respect to variations in model specification and research designs.

Even so, Table 3 does indicate certain important moderating influences at work here. For example, measures of others' attitudes are significantly stronger when no controls for variables specified by competing criminological theories are included in the model, yet even then the mean effect size is still relatively

Table 3 Moderator analyses for social learning theory predictors according to variations in model specification and research design

Social learning theory predictor	Competing theories	Time dimension	Dependent variable
Differential association			
Peers' behaviors (166)	1.615	3.715	2.535*
Parents' behaviors (27)	0.037	2.013	3.293*
Others' behaviors (16)	0.301	4.324	0.871
Peers' Attitudes (80)	2.219	0.262	1.006
Parents' attitudes (31)	0.178	7.317*	0.357
Others' attitudes (4)	23.865*	0.043	1.287
Differential association index (61)	0.022	3.974	0.505
Definitions			
Antisocial attitudes/Definitions (121)	1.798	7.367**	1.610
Definitions index (22)	0.959	0.079	2.678
Differential reinforcement			
Peer reactions (46)	1.936	0.305	0.576
Parental reactions (29)	0.041	0.123	0.463
Others' reactions (7)	0.222	—	3.048
Rewards minus costs (30)	0.813	0.101	2.281
Differential reinforcement index (20)	1.462	0.275	0.445
Modeling/Imitation			
Number of admired models			
Witnessed (15)	2.796	0.351	0.613
Differential imitation index (15)	2.458	0.984	0.578

*Statistically significant at $p < .05$; **statistically significant at $p < .01$.
Note. Numbers in parentheses = number of contributing effect size estimates. Numbers in columns are *F*-statistics from one-way ANOVA models.

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weak and statistically insignificant (Mz = .060, $p > .05$). Furthermore, indicators of parents' attitudes are significantly weaker in cross-sectional research designs (Mz = .019, $p > .05$), yet measures of definitions are significantly stronger in cross-sectional studies (Mz = .229, $p < .05$) than in longitudinal studies (Mz = .138, $p < .05$). Finally, the dependent variable used across studies is important for measures of peers' behavior, where the effects are strongest in studies that assess drug and alcohol abuse (Mz = .361 and .338, respectively, $p < .05$) and weakest in studies predicting violent crime (Mz = .199, $p < .05$), property crime (Mz = .057, $p > 05$), and theft (Mz = .179, $p < .05$). The measurement of the dependent variable also significantly influenced the effect sizes for indicators of parents' behavior, which were weakest in studies that assessed drug and alcohol abuse (Mz = .079 and .199, respectively, $p > .05$), and were significantly stronger in studies predicting violent crime (Mz = .288, $p < .05$) and "general" crime/deviance scales (Mz = .206, $p < .05$).

Discussion

Social learning theory has been one of the major theoretical perspectives in criminology for the last four decades (Akers & Jensen, 2003a, 2006). Not only has the learning tradition stood on its own in criminology, it has also been merged into multiple integrated theoretical frameworks, including social disorganization (Lowenkamp, Cullen, & Pratt, 2003; Sampson & Groves, 1989), rational choice (Paternoster & Piquero, 1995; Pratt, Cullen et al., 2006; Stafford & Warr, 1993), neuropsychological (Beaver, Wright, & DeLisi, 2008; McGloin, Pratt, & Maahs, 2004), and life-course explanations of criminal behavior (Wiesner, Capaldi, & Patterson, 2003). As a result, a large body of empirical work has been produced that has assessed the core propositions specified by social learning theory.

The problem, however, is that as a discipline, criminology is lagging behind other social and behavioral sciences in terms of systematic knowledge organization (see, e.g., the discussion by Cullen, Wright, & Blevins, 2006a). As a result, firm evidence concerning the relative empirical status of the major criminological paradigms is only beginning to emerge (Paternoster, 1987; Pratt & Cullen, 2000, 2005; Pratt, Cullen et al., 2006; see also Andrews & Bonta, 2006; Cullen et al., 2006b; Pratt, Cullen, Blevins, Daigle, and Unnever, 2002; Pratt, McGloin, & Fearn, 2006; Tittle et al., 1978). The work presented here is intended to place the empirical status of social learning theory within this larger picture of which theories have rightfully earned the title of a well-supported explanation of criminal/deviant behavior and should therefore continue to guide future scholarship in our field. To that end, the results of our meta-analysis lead to three conclusions.

First, the empirical support for social learning theory stacks up well relative to the criminological other perspectives that have been subjected to meta-analysis. Specifically, the mean effect sizes of the differential association and

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definitions (or antisocial attitudes) are comparable in magnitude to self-control—a finding that is consistent with Pratt and Cullen's (2000) meta-analysis of the self-control criminological literature. Furthermore, the mean effect sizes presented here are generally larger than those revealed in Pratt, Cullen, Blevins, Daigle, & Madensen's (2006) meta-analysis of the variables specified by rational choice/deterrence theory (e.g., the relationship between indicators of the perceived certainty and severity of punishment and criminal/deviant behavior). Furthermore, our results indicate that the effects of variables specified by social learning theory tend to have "general effects" across variations in methodological approaches taken across studies. Even so, similar to the results of the meta-analyses of both the self-control and rational choice/deterrence literatures, the strength of effects for certain predictors of crime/deviance specified by social learning theory varies considerably according to how key concepts are measured, and the effect sizes for certain indicators of key theoretical concepts are significantly influenced by variations in samples, model specification, and research design across studies. In short, the variables specified by social learning theory tend to be strong, yet not invariant, predictors of criminal and deviant behavior.

Second, some components of social learning theory have received more empirical attention from criminologists than others. In particular, the differential association and definitions components of the theory have been tested extensively. These measures have appeared not only in studies focused on differential association/social learning theories but also in tests of virtually all of the major individual-level theories of crime (e.g., tests of strain, self-control, social bond/social control theories). The frequency with which these variables have appeared in the empirical literature is, of course, partially a result of the prominence of social learning theory within criminology as well as the presence of such measures in many of the datasets that are frequently used by criminologists in their published work (e.g., National Youth Survey, National Longitudinal Survey of Youth). Nevertheless, scholars have often included these measures mainly as control variables so as to avoid specification errors in their statistical models—an indirect, yet certainly important, indicator of the influence of social learning theory in criminology. By comparison, the differential reinforcement and modeling/imitation components have received less empirical attention relative to other elements of the theory. In an absolute sense, however, studies using indicators of these concepts contributed 162 effect size estimates—more than enough for us to reach firm conclusions about their relationships with crime and deviance.

Third, some components of social learning theory have received more empirical support than others. For example, our results show that the mean effect sizes of the differential association and definitions measures are consistently the strongest of the predictors specified by social learning theory. The mean effect sizes for the differential reinforcement and modeling/imitation predictors, however, did not fare so well. They were instead generally weak and, at times, statistically insignificant across the sample of 133 studies. The weak

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effects for differential reinforcement—with the exception of aggregated indexes of respondents' overall reinforcement contingencies (as indicated by the larger mean effect size of the differential reinforcement index predictors)—are particularly problematic since that is the key causal mechanism by which deviant peer associations are assumed to cause deviant behavior (Akers, 1998). This exception may be critical, however, especially for social learning "purists," as the rewards minus costs measure and the differential reinforcement index, the two best performers, are arguably more in keeping with what Akers described than the reactions of peers, parents, or others.

The combination of large mean effect sizes for the differential association measures with the relatively weak effect sizes for the indicators of differential reinforcement highlights two important issues. First, association with deviant others may increase one's own level of criminal/deviant behavior for reasons that have little to do with the "normative influence" of deviant peers (see Haynie & Osgood, 2005, p. 1109). Instead, scholars have argued that exposure to deviant peers may alter the opportunity structures for particular offenses, including increased access to drugs and alcohol and greater levels of exposure/proximity to "targets" for personal and property crime (see Felson, 2002; McGloin, Sullivan, Piquero, & Pratt, 2007; Osgood, Wilson, O'Malley, Bachman, & Johnston, 1996). Accordingly, it is possible that, theoretically, *reinforcement* simply may not be as important to the explanation of the peers-deviance link as Akers has consistently held that it is—a proposition echoed by Krohn (1999) who argued that such process-oriented elements should be jettisoned from tests of the theory altogether in favor of more global differential association measures. It is equally plausible, however, that it is researchers' relative inattention to measuring directly the reinforcement component of social learning theory that could be partially responsible for this finding, and as additional tests of these propositions specified by social learning theory continue to emerge, our conclusions regarding this issue may change as well.

Second, scholars have recently challenged the validity of using indirect measures of peers' deviant behaviors as an indicator of differential association (see, e.g., Haynie, 2001, 2002). This emerging body of work indicates that when direct measures of peer deviance are used (i.e., asking one's friends directly about their misbehavior as opposed to the typical strategy of relying on respondents' perceptions of such behavior), the peers-delinquency association tends to be attenuated substantially (Haynie & Osgood, 2005). The inflation of this relationship occurs because youths generally offend in groups, yet they also tend to use a rotating cast of co-offenders (McGloin, Sullivan, Piquero, & Bacon, 2008; Warr, 2002). As such, they are likely to overestimate the overall level of deviant behavior for any given peer because respondents have been found to project their own attitudes and behaviors onto their friends (Aseltine, 1995; Haynie & Osgood, 2005; Jussim & Osgood, 1989). In short, a portion of the effects of the differential association–crime/deviance relationship revealed here could conceivably be an artifact of the extensive use of indirect measures of peers' deviant behaviors as a proxy for differential association.

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Nevertheless, in this context, the finding that definitions are a firm predictor of misbehavior is theoretically salient. The differential association/social learning tradition asserts that "definitions favorable to law violation," as Sutherland called them, are the key links between differential associations and wayward outcomes. Because the effects of peer associations can have multiple interpretations, the criminogenic effect of attitudes provides clearer support for social learning theory. Phrased alternatively, if attitudes or definitions were unrelated to outcomes, then Akers's perspective would have been falsified in a central way.

Despite these conclusions, it is also important to note that there are limits to what a meta-analysis can "deliver." Indeed, we do not intend for our study to replace or otherwise obviate the need for additional independent or "primary" tests of the theory. For example, while meta-analysis can address broad patterns in the findings across a large body of studies, what it cannot do is assess the full spectrum of complex relationships among independent and dependent variables within a single dataset—that is still the domain of the primary study, to assess the nuances and contingencies among the covariates that may be of empirical and/or theoretical relevance. Viewed in this way, we hope that our meta-analysis encourages and guides future research in this area. Furthermore, given our cut-off date of 2003 for including studies into our sample, additional research on the subject continues to be produced, and as new studies emerge, scholars can reassess these relationships to determine whether the conclusions here should be altered.[15]

In the end, criminologists have remained either faithful to, or critical of, particular theories for a host of reasons. To be sure, a given theory may hold the allegiance of scholars for reasons associated with their social and/or political preferences (Gould, 1996), the ease with which it can be tested and translated into publications (and therefore professional advancement, see Cole, 1975), and even perhaps loyalty to one's intellectual mentors. We would hope, however, that as criminology matures as a social science discipline, the *empirical status* of the theory would carry at least as much weight, if not more, than these other criteria for what constitutes a "good" theory. On that front, social learning theory has performed rather well. Empirical support for its key propositions is not unqualified, and it is unlikely to unseat all theoretical contenders. Even so, the results of our meta-analysis clearly show that it deserves its status as one of the core perspectives in criminology.

15. As a final note on the issue of publication bias in meta-analysis, scholars have noted that such bias is potentially a greater threat in research involving clinical trials (see Dickersin, Min, & Meinert, 1992), yet even a large-scale study of 745 manuscripts submitted to the *Journal of the American Medical Association* found "no significant difference in publication rates between those with positive vs negative results" (Olson et al., 2002, p. 2825). Thus, the issue of publication bias for meta-analysis—even in a context where the risk of its presence is should be at its peak—is far from a "given." Nevertheless, we encourage scholars in the future to revisit our work while including unpublished studies to determine whether the inferences we have made here would be changed in any meaningful way.

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