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Accounting for Projection Bias in Models of Delinquent Peer Influence: The Utility and Limits
of Latent Variable Approaches

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ABSTRACT

Objectives. Projection effects have been shown to bias respondent perceptions of peer delinquency, but network data required to measure peer delinquency directly are unavailable in most existing datasets. Some researchers have therefore attempted to adjust perceived peer behavior measures for bias via latent variable modeling techniques. The present study tested whether such adjustments render perceived peer coefficients equal to direct peer coefficients, using original data collected from 538 young adults (269 dyads). **Methods.** After first replicating projection effects in our own data and examining the degree to which measures of personal, perceived peer, and direct peer violence represent empirically distinct constructs, we compared coefficients derived from two alternative models of personal violence. The first model included an error-adjusted latent measure of perceived peer violence as a predictor, whereas the second substituted a latent measure of directly-assessed, peer-reported violence. **Results.** Results suggest that personal, perceived peer, and direct peer measures each reflect fundamentally separate constructs, but call into question whether latent variable techniques used by prior researchers to correct for respondent bias are capable of rendering perceived peer coefficients equal to direct peer coefficients. **Conclusions.** Research cannot bypass the collection of direct peer delinquency measures via latent variable modeling adjustments to perceived peer measures, nor should models of deviance view perceived peer and direct peer measures as alternative measures of the same underlying construct. Rather, theories of peer influence should elaborate and test models that simultaneously include both peer measures and, further, should attempt to identify those factors that account for currently unexplained variance in perceptions of peer behavior.

KEY WORDS: peers, delinquency, projection, false-consensus, perceptions

WHEN DIRECT PEER DELINQUENCY MEASURES ARE NOT AVAILABLE: THE UTILITY AND LIMITS OF LATENT VARIABLE APPROACHES

1. INTRODUCTION

Peer delinquency remains among the strongest and most consistent predictors of personal delinquency in the existing criminological literature (Agnew, 1991; Warr, 2002). However, most studies continue to measure peer delinquency based on respondent perceptions of their peers' behavior rather than peer's own self-reports. Psychological research suggests that such perceived peer delinquency measures are inaccurate, because individuals mistakenly attribute their own behavior tendencies to others via such egocentric biases as false-consensus (Marks & Miller, 1987) and projection (Newcomb, 1961). Perhaps as a result, the relationship between personal and peer delinquency is consistently much larger in studies that measure peer delinquency using respondent perceptions rather than directly-collected peer self-reports. (Aseltine, 1995; Haynie, 2001, 2002; Haynie and Osgood, 2005; Meldrum et al., 2009; Weerman and Smeenk, 2005).

The substantially weaker predictive power of direct, versus perceived, peer delinquency measures yields important implications for the interpretation of prior research and, ultimately, for delinquency-prevention policy. Insofar as perceived peer delinquency measures overestimate the etiological importance of actual peer behavior, delinquency prevention efforts that make use of this research may also be overestimating the delinquency-prevention effect of separating youth from delinquent peers. At the same time, however, collecting data directly from peers themselves via named rosters on which respondents identify their friends requires more time, involves more complex data management, and elicits greater concern for anonymity than does collecting peer data based on respondent perceptions. Thus, although perceived peer delinquency measures distort the true influence of peers on delinquency, such measures remain more common in delinquency studies than direct peer delinquency measures.

Preliminary research (Matsuda and Anderson, 1998; Rebellon, 2012), however, suggests that latent variable statistical techniques can adjust perceived peer delinquency coefficients to account for false-consensus and projection. This research acknowledges that perceived peer measures are contaminated by biases such as false-consensus and projection, but estimates the correlations among the error terms for corresponding personal and perceived peer delinquency items in latent variable analysis to “control for this contamination” (Matsueda and Anderson, 1998:291). Missing from the existing literature, however, is a formal test of the degree to which this method counteracts the artificial-inflation of the relationship between personal and peer delinquency that results from the use of perceived peer delinquency measures. After first examining whether measures of personal, perceived peer, and actual peer delinquency reflect separate theoretical constructs, the present study provides such a test.

1.1 The Relationship between Personal and Perceived Peer Delinquency

The strong correlation between self-reports of personal delinquency and perceptions of peer delinquency is well-established in the criminological literature. Traditionally, such research operationalizes personal delinquency via self-report scales comprised of multiple ordinal items probing substance behaviors such as alcohol, cigarette, and marijuana use, but sometimes probing the frequency of more serious delinquency (see Akers, 2009 for a review). The same research typically operationalizes perceived peer behavior using items that probe the proportion of a respondent’s friends that the respondent believes to have engaged in corresponding delinquent behaviors (e.g., Elliott et al., 1985).

Despite the strong and reliable correlation between personal and perceived peer behavior, there is considerable debate concerning its appropriate interpretation. First, some scholars interpret the correlation as evidence supporting Sutherland’s (1947) classic statement of

differential association theory, which argues that an individual engages in any type of behavior as a function of repeated social interaction with associates who provide models of such behavior and who espouse a preponderance of attitudes favorable to that behavior. More recently, scholars have combined Sutherland's theory with the reinforcement paradigm in behavioral psychology (Burgess and Akers, 1966) and have theorized that delinquency results from an overall learning process comprised of exposure to delinquent associates, behavioral imitation of such associates, adoption of attitudes favorable to delinquency, and post-hoc reinforcement for delinquent behavior (Akers and Lee, 1996). Indeed, numerous longitudinal studies find that perceptions of peer delinquency at Time 1 are significantly associated with personal delinquency at Time 2 even after controlling statistically for personal delinquency at Time 1 (e.g., Matsueda and Anderson, 1998; Meldrum et al., 2009).

A second interpretation of the correlation disputes whether peer delinquency causes personal delinquency. Instead, drawing on Glueck and Glueck's (1950) observation that "birds of a feather flock together," this view argues that the correlation reflects only the influence of personal behavior on the selection of similarly-delinquent social associates. To be certain, some scholars take a middle-ground view, acknowledging the possibility that personal and perceived peer behavior may be reciprocally related via simultaneous socialization and selection mechanisms (e.g., Akers, 2009; Kandel, 1996; Thornberry, 1987). Other scholars, however, remain skeptical that a reciprocal relationship exists, instead arguing that the correlation reflects only the "tendency of people to seek the company of others like themselves" (Gottfredson and Hirschi, 1990:156). Empirical research comparing these alternative views remains equivocal, with some findings suggesting evidence of only socialization (e.g., Akers and Lee, 1996), others

suggesting evidence of only selection (e.g., Rebellon, 2012), and others suggesting bidirectional causality (e.g., Matsueda and Anderson, 1998; see also Kandel, 1980).

A third and final interpretation of the correlation between personal and perceived peer delinquency suggests that neither actually wields any causal influence over the other but rather that both are alternative measures of the same underlying construct. Indeed, most studies examining socialization and selection effects employ data from juveniles (but see Akers, 2009), who are particularly prone to commit delinquent acts in groups (Warr, 2002). As such, youth may attribute the same group behaviors to both themselves and to those with whom they co-offend. By extension, respondent perceptions of peer delinquency “may merely be another measure of self-reported delinquency” (Gottfredson and Hirschi, 1990:157). As yet, only limited research has examined this issue empirically. What research exists has relied upon either exploratory (Agnew, 1991) or confirmatory (Zhang and Messner, 2000; Rebellon, 2012) factor analysis to examine whether measures of personal and perceived peer delinquency demonstrate what Campbell and Fiske (1959) have called “discriminant validity,” which exists when two sets of measures intended to reflect separate theoretical constructs are correlated at a value significantly *less* than 1.0. Results of such studies tend to suggest that “[m]easures of adolescent-reported peer deviance... overlap considerably (but not perfectly) with measures of self-reported peer delinquency” (Zhang and Messner, 2000:334).

1.2 Perceptual Bias among Measures of Perceived Peer Delinquency

Further complicating any interpretation of the correlation between personal delinquency and perceived peer delinquency is the reliable psychological finding that individuals’ perceptions of their peers’ attitudes and behaviors are systematically biased in the direction of respondents’ own attitudes and behaviors. The most commonly studied of such biases is referred to as the

“false consensus effect,” whereby individuals who engage in a certain behavior or endorse a particular attitude overestimate the prevalence of the same behavior/attitude among others (e.g., Katz and Allport, 1931; Ross et al., 1977). Typically, research demonstrates evidence of false-consensus in one of two ways. First, a researcher may divide his or her sample into one group of individuals who have engaged in a given behavior (or who endorse a given attitude) and another group of individuals who have (do) not. Evidence of false consensus is indicated to the degree that the former group’s average estimate of the given behavior/attitude’s prevalence is higher than that of the latter group. Second, if a researcher has access to data concerning the actual prevalence of a given behavior/attitude in a target population, he or she may ask a sample of individuals who have all engaged in a given behavior (or who all endorse a given attitude) to estimate the prevalence of the given behavior/attitude in the target population. In this case, evidence of false consensus is indicated to the degree that the sample’s average estimate of the behavior/attitude’s prevalence exceeds the actual prevalence in the target population. Regardless of the particular method employed, meta-analysis finds consistent evidence of false-consensus bias (Mullen et al., 1985).

Cognitive and social psychologists have offered four primary explanations for the false consensus effect. First, some scholars “believe that selective exposure is the primary factor generating misperception of the commonness of one’s preferred positions” (Marks and Miller, 1987:77). Research, for example, finds that a person’s perception of the prevalence of smoking in the general population is correlated with the prevalence of smoking among the friends with whom he or she tends to associate (Sherman et al., 1984). Part of the false-consensus effect may therefore reflect individuals’ mistakenly attributing acquaintances’ tendencies to the broader population. At the same time, some research finds a greater, rather than smaller, false-consensus

effect when the target population consists of friends or in-group members rather than people in general or out-group members (Holtz and Miller, 1985; Judd and Johnson, 1981). False consensus therefore seems to reflect something more than an inaccurate generalization of accurately perceived in-group characteristics. Some scholars therefore suggest that a second potential source of false consensus derives not from selective exposure to atypical individuals, but to the selective attention that individuals pay to typical individuals. In particular, “individuals may selectively attend to, utilize, and interpret information to which they are exposed” such that they focus attention disproportionately on behaviors and attitudes that match their own (Hymel, 1986:432). Illustratively, some research finds that false consensus can be increased by asking respondents to visualize, and hence focus attention upon, a behavior consistent with their prior behavioral tendencies (Kernis, 1984).

A third potential source of false-consensus may stem from inferential errors in cognitive processing. Of note, the literature makes clear that “[i]naccurate estimates [of a given attitude or behavior’s prevalence] do not necessarily imply flawed reasoning” (Krueger and Clement, 1994:596; see also Einhorn, 1986). Rather, a person’s own status on a given behavior/attitude is a valid piece of data that informs their inferences about the greater public, particularly over repeated trials and particularly when few other pieces of information are available (Dawes, 1989). At the same time, the false-consensus effect is not eliminated when respondents are informed that another individual holds a countervailing status on a particular attitude or behavior. Instead of cognitively weighing equally their own behavior/attitude and another’s countervailing behavior/attitude, respondents who are explicitly taught about the false-consensus effect attribute false-consensus bias to those with countervailing positions while ironically continuing to exhibit the false consensus bias themselves (Krueger and Clement, 1994). Such findings suggest that a

fourth potential source of the false-consensus effect may stem from motivational processes that are not “cognitive,” but that instead take place automatically when an individual experiences an emotional need for social self-enhancement or personal self-esteem maintenance (Marks and Miller, 1987). Of particular relevance, for example, research finds significant false-consensus effects when respondents are asked to estimate the prevalence of behaviors/attitudes that they hold but that are not universally approved (e.g., Sherman et al., 1984) and these results are stronger among adolescent respondents than among adult respondents (e.g., Sherman et al., 1983). Thus, some evidence suggests that false-consensus may serve as an automatic, emotional means by which the adolescent psyche essentially “normalizes” delinquency.

1.3 Implications for Understanding and Controlling Juvenile Crime and Delinquency

As should be expected in light of the false consensus literature discussed above, research suggests that youth who engage in deviant behavior tend to overestimate both the prevalence and frequency of such behavior among their friends (e.g., Iannotti and Bush, 1992; Prinstein & Wang, 1995; Urberg et al., 1990). Most of this research is focused on health risk behaviors with a number of studies finding evidence of robust false-consensus effects for such behaviors such as substance use (e.g. Iannotti and Bush, 1992; Kandel, 1980; Sherman et al., 1983; Urberg et al., 1990; Wolfson, 2000) and only preliminary evidence finding somewhat weaker false-consensus effects for serious forms of delinquency like violence (Prinstein and Wang, 2005). Such effects may help to explain why correlations between personal and peer delinquency “are generally two to three times higher” when researchers operationalize peer delinquency via respondent perceptions rather than direct peer reports (Kandel, 1996:298). Specifically, the false consensus effect may artificially inflate the similarity that an individual perceives between his/her behavior and that of his/her associates. Thus, while studies using direct peer reports

continue to yield equivocal evidence concerning whether socialization or selection better accounts for the correlation between personal and peer behavior (e.g., Kandel, 1978; Knecht et al., 2010, Weerman, 2011), they have simultaneously led some to conclude that “estimates of influence [i.e., socialization] are grossly overstated in analyses relying upon respondents’ perceptions of their friends’ behavior” (Aseltine, 1995:103; see also Haynie, 2001; Kandel, 1996; Meldrum et al., 2009; Weerman and Smeenk, 2005).

Given that false-consensus effects may exaggerate the relationship between personal and peer delinquency in studies using perceptual measures of peer behavior, two critical questions arise about how to interpret prior research concerning peer influences on delinquency and about how best to conduct future etiological research on such influences. First, do perceived peer delinquency measures reflect only the composite of actual peer delinquency and measurement bias, or do perceived peer delinquency measures reflect a theoretical construct that is fundamentally distinctive from actual peer behavior? To the degree that perceived and direct peer delinquency measures reflect *theoretically* distinctive constructs rather than alternative indicators of the same underlying construct, criminological theory will need to pay greater attention to describing the manner in which *both* measures of peer delinquency may separately impinge upon personal behavior, as well as the manner in which both measures may be related to each other (see e.g., Jussim and Osgood, 1989). Further, insofar as perceptions of peer behavior influence personal delinquency but come from more than actual peer behavior and measurement bias, effective delinquency-prevention will require identifying the other sources of such perceptions.

Second, can latent variable methods successfully counteract the artificial inflation of coefficients linking perceived peer delinquency with personal delinquency? Insofar as studies

using perceived peer measures have overestimated the socializing influence of peers, interventions that aim to isolate youth from objectively-delinquent peers may be less useful for controlling delinquency than perceived peer coefficients imply. A more realistic expectation of such interventions' effectiveness would be garnered from research examining the effect of directly-measured peer delinquency on respondent behavior. As yet, however, a majority of empirical studies in the criminological literature continues to employ perceived peer delinquency measures, perhaps as a result of the added time, effort, and confidentiality issues involved in collecting and managing network data like those available in the National Longitudinal Study of Adolescent Health (AddHealth). Thus, while limited research purports to control for false-consensus bias (e.g., Matsueda and Anderson, 1998; Rebellon, 2012) via latent variable adjustments to perceived peer coefficients, such studies' datasets have not included the direct peer measures necessary for verifying the effectiveness of such latent variable adjustments.

1.4 The Present Study

The present study addresses the above two questions. It begins by attempting to replicate egocentric bias that has been unveiled in prior research. However, whereas much of the above-cited research concerns false-consensus bias, whereby individuals' own behaviors/attitudes bias their perception of a given behavior/attitude's prevalence among peers, the present study uses dyadic data to examine the related issue of whether an individual's own frequency of a given behavior leads him or her to misperceive the frequency with which one particular friend engages in the same behavior. While this bias has in some cases been subsumed under the umbrella term "false consensus" (e.g., Prinstein and Wang, 2005), it is technically more akin to "projection," whereby an individual incorrectly attributes his or her own tendencies unto specified others (Holmes, 1978; Newcomb, 1961). Criminological research finds evidence of projection effects

that matches existing evidence concerning the false-consensus effect. In particular, research finds that respondents assume specified friends' behavior is more similar in frequency to their own than is objectively warranted (e.g., Boman et al., 2012, Prinstein and Wang, 2005).

After examining whether the dyadic data that we have collected yield projection effects similar to those found in prior research, we examine whether perceived peer delinquency reflects a construct that is fundamentally distinctive from directly-measured peer delinquency. We test for discriminant validity (i.e., Campbell and Fiske, 1959) using latent variable modeling techniques outlined by Loehlin (1992) and used in prior criminological research to test Gottfredson and Hirschi's (1990) claim that personal and perceived peer delinquency are actually alternative measures of the same construct (e.g., Zhang and Messner, 2000; Rebellon, 2012). Specifically, we examine whether latent variable correlations between perceived and directly-measured peer delinquency are significantly lower than 1.0 (as indexed by two standard errors).

Finally, we provide what we believe to be the first empirical test to examine whether latent variable modeling techniques (e.g., Matsueda and Anderson, 1998; Rebellon, 2012) are capable of adjusting structural estimates of the relationship between personal and peer delinquency to counteract the inflation that appears to exist among coefficients linking perceived peer behavior with personal delinquency. Such techniques have been discussed at length by Matsueda and Anderson (1998:291) who "...tested the hypothesis that measures of delinquent peers and delinquent behavior are contaminated due to (1) respondents' imputing their own behavior to that of their friends, (2) respondents' imputing friendship to those who share their delinquency, and (3) respondents reporting... rumors that are correlated with their own delinquent status" (Gottfredson and Hirschi, 1990). According to Matsueda and Anderson (1998:291): "Each of these mechanisms implies that measurement errors for a specific measure

of delinquent peers (e.g., peers' breaking and entering) should be contemporaneously correlated with the corresponding measure of delinquent behavior (e.g., respondents' breaking and entering)." Results of their study yielded significant error correlations, thus suggesting to Matsueda and Anderson (1998:291) that "in examining the reciprocal relationships between [perceptual measures of] delinquent peers and delinquent behavior, it is important to control for this contamination" (see also Rebellon, 2012). Implicit in Matsueda and Anderson's (1998) analysis is the notion that, by estimating contemporaneous correlations among corresponding error terms for personal and perceived peer delinquency items, a researcher can bring structural estimates closer into line with those that *would* have emerged had direct measures of peer delinquency been available to use in place of perceived measures.

There exist, however, several reasons to question whether the above techniques are likely to close the entire gap between structural results from models using error-adjusted perceived peer measures and models using direct peer delinquency measures. Saris & Aalberts (2009) for example, suggest that the most probable explanation for significantly correlated disturbance terms is shared method variance, which exists when some unmeasured variable simultaneously affects manifest indicators of two ostensibly separate theoretical constructs (see also, Loehlin, 1992). By extension, even assuming that significant error term correlations are unveiled, those error term correlations might reflect something other than egocentric biases like false consensus or projection. They may, for example, reflect the influence of a trait like self-control on an individual's responses to both sets of measures or reflect the effect of co-offending on perceptions of both personal and peer behavior. Further, even if the error term correlations in question *did* successfully capture egocentric biases like false consensus and projection, it remains possible that, as discussed previously, perceived peer delinquency is a fundamentally

separate construct that is theoretically distinctive from directly measured peer delinquency. Perceptions, for example, might come partly from actual peer behavior and partly from egocentric biases, but may simultaneously be affected by variables like exaggerated bragging, false rumors, or embellished gossip. Whether the error correlation estimates suggested by Matsueda and Anderson (1998:291) do or do not serve to “control for this contamination” remains an open question that has yet to be evaluated empirically.

In the present study, we therefore use new data from young adult dyads to provide an empirical examination of this issue. In particular, we estimate two separate models of delinquency. In the first, we include a measure of peer delinquency derived from our primary respondents’ perceptions of their friends’ delinquency and allow error terms for perceived peer items to correlate with error terms for corresponding personal delinquency items. In the second, we remove the perceived peer measure and substitute a measure of peer delinquency derived directly from peer self-reports. To the degree that the perceived peer coefficient from the first model remains substantially larger in magnitude than the direct peer coefficient in the second model, results would call into question whether error-correlation adjustments are capable of counteracting the artificial inflation of perceived peer coefficients that would appear to result from projection and other sources of contamination.

2. METHOD

2.1 Participants and Procedure

Given that the measures we needed were not all available in existing datasets, we collected original data from undergraduate student dyads in a university setting. Primary participants received course credit for their participation and were asked to bring a friend with them to the study. In order to facilitate respondents’ recruitment of friends, we promised

prospective respondents that their friends would only be required to fill out a shortened version of our survey instrument. The study took place during the spring semester in order to ensure that freshmen respondents had time to form relations with their peers. Primary respondents and their friends were seated separately and filled out surveys independently. Permission was granted from appropriate IRB personnel, all participants were given an informed consent form, and emphasis was placed on maintaining anonymity by not including names on surveys.

When a respondent-friend dyad came to the designated laboratory setting, we gave each member of the dyad a pre-arranged packet. Respondent packets contained a full survey (including gender, year in school, self-reported delinquency, perceived peer delinquency, and self-control measures). Friend packets contained a shorter survey (including demographic information and a self-reported delinquency scale). Surveys within each respondent-friend dyad were linked via an arbitrary ID number to allow matching of dyads while simultaneously maintaining anonymity. Upon completion of questionnaires, all participants were thoroughly debriefed. After dropping the four respondents who did not report being at least “somewhat close” with their chosen “friend”, our data collection yielded 269 dyads comprised of 538 individuals. Each dyad included a primary respondent and one of his or her friends. In these dyads, seventy-five of our primary respondents were male with the rest being female. We first ran our analyses using all available dyads. To control for potential cross-sexed effects (e.g., McDougall and Hymel, 2007), we also re-ran analyses using only the 73% of dyads that were same-sex. Results of both sets of analyses were substantively identical and we therefore present only results from the full sample of dyads. Primary respondents were distributed across all four college levels, with a greater preponderance of freshmen and sophomores than juniors and seniors (Freshmen = 48%, Sophomores = 33%, Juniors = 13%, Seniors = 6%). Given IRB

concerns about respondent identification in a setting of extremely limited racial diversity, we did not collect data concerning race or ethnicity, whose limited variance would likely not have yielded significant effects regardless.

2.2 Measures

Violence. Our study employs measures of respondent violence, measures of friend's violence based on respondent perceptions, and measures of friend's violence based on each friend's self-reports. Responses for all sets of measures related to four behaviors: hitting/punching; pushing/shoving; kicking; and using force to take something from another individual. Responses to all violence items were based on a nine-point scale, ranging from 1 ("Never in the past year") to 9 ("Two to three times a day"), with high scores indicating high levels of delinquent behavior. This response range is derived from the National Youth Survey (Elliott et al., 1985), which has been used extensively in the existing delinquency literature. Items are listed in Table 1 along with descriptive statistics.

(Table 1 about here)

Low self-control. We include self-control in our structural models for three reasons. First, low self-control is related to deficits in interpersonal skills, and inadequate relational functioning may help explain why some individuals are especially inaccurate in estimating their peer's delinquent behavior. Illustratively, Gottfredson and Hirshi proposed that low self-control results in poor relational competencies (Gottfredson and Hirshi, 1990:157), and research has empirically related low self-control to poor quality relationships (Evans et al., 1997) and diminished interpersonal functioning (Tangney et al., 2008). By controlling for low self-control, our models attempt to account for errors in estimation that are attributable to weak interpersonal skills. Second, individual differences in self-control may also account for variability in the

predisposition to associate with delinquent peer groups or to gravitate toward social situations in which the opportunity for delinquency is greater (e.g., Osgood et al., 1996). Because individuals who are low in self-control are also likely to congregate and co-offend together (e.g. McGloin, and Shermer, 2009; Wills and Dishion, 2004), partialling out self-control should, at least in part, control for divergent probabilities that a respondent has directly witnessed, co-offended, or otherwise deduced his peer's involvement in antisocial behavior. Third, our data are cross-sectional. As such, we cannot adjust directly for prior behavioral tendencies. However, accounting for self-control allows us to control for an individual's disposition to commit crime, which appears to be relatively stable across time (Caspi et al., 2006). For example, Gottfredson and Hirschi (1990) argue that interpersonal differences in self-control become fixed within the first decade of life, so that accounting for self-control should control for past, present, and future criminal propensity. Although there is some evidence of malleability in emotional regulation (e.g. Côté et al., 2002), several psychological studies also indicate that some dimensions of self-control are relatively constant across the life-course (Caspi et al., 1997; Oliva et al., 2012, Roberts & DeVecchio, 2000). Likewise, a body of criminological work represents self-control as an enduring criminal propensity, albeit with a loss of some explanatory power across time (Pratt and Cullen, 2000). By controlling for self-control, then, in some measure our models account for previous criminal behavior. Self-control was therefore included in our primary respondent survey and was measured with the scale created by Grasmick et al. (1993). Responses were measured on a four point Likert-type scale ranging from 1 ("Strongly disagree") to 4 ("Strongly agree") and with high scores indicating low self-control. Based on Gottfredson and Hirschi's discussion of the major dimensions of low self-control (see also Grasmick et al., 1993),

we constructed six subscales, each reflecting one of the six dimensions of self-control and each calculated as the mean of its four constituent items.

2.3 Analytic Strategy

We divide our analyses into three sets. The first provides an item-by-item analysis examining the degree to which there is projection bias among the respondents in our sample. For each of the four behaviors listed above, it compares the discrepancy between a respondent's personal and perceived friend measure with the discrepancy between a respondent's personal and direct friend measure. To eliminate negative values, we square each difference score.¹ To the degree that respondents suffer from projection or false consensus bias, they should underestimate how different their friend's behavior is from their own and, as such, the mean of squared *perceived* differences should be significantly lower than the mean of squared *actual* differences.

Our second set of analyses employs confirmatory factor analytic procedures outlined by Loehlin (1992) to provide a test of the theoretical structure underlying our personal delinquency, perceived peer delinquency, and actual peer delinquency items. In particular, we construct a three-factor latent variable model in which items reflecting primary respondents' violence load on one factor, perceptions of peer violence load on a second, and directly-measured peer violence measures load on a third. We then use *Lisrel 8* (Jöreskog & Sörbom, 1993a) to estimate the inter-factor correlations in our model. Perceived measures of violence bear discriminant validity (see Campbell and Fiske, 1959) vis-à-vis the other factors insofar as their latent variable correlates with these other factors at a value that is significantly different from 1.0, as indexed by two standard errors. Following Loehlin (1992, pp. 66-67), we further compare the fit of our

¹ While some studies "have used a residual score approach to compute discrepancy scores" rather than a difference-score approach, some research has used both methods simultaneously, "yielding discrepancy scores that were nearly identical (i.e., r 's $>.95$) to one another and producing an identical pattern of results" (Prinstein and Wang, 2005:297-298).

three-factor model with the fit of alternative measurement models in which (a) perceptions of peer violence load on the same factor as direct peer measures; (b) perceptions of peer violence load on the same factor as personal violence measures; and (c) all three sets of measures load on only a single factor.

Our third set of analyses employs two structural equation models to examine the etiological implications of measuring friends' delinquency via respondent perceptions versus friend self-reports. The first model follows the measurement strategy of Matsueda and Anderson (1998) by modeling projection effects via the correlation between the error term for each manifest indicator of personal behavior and the error term for each corresponding indicator of perceived peer behavior. Matsueda and Anderson (1998) argue that each significant error correlation should capture the influence of projection, as well as such potential influences as false rumors and peer bragging, thus rendering the perceived peer coefficient similar in magnitude to the coefficient that would have resulted using direct peer measures to reflect peer behavior. However, whereas Matsueda and Anderson (1998; see also Rebellon, 2012) used data from the NYS that did not contain direct peer behavior measures with which to test empirically the notion that estimating such error correlations brings structural estimates of perceived peer behavior into line with those that emerge using direct peer measures, our data do allow for such a test. We perform this test by estimating a second structural equation model in which we substitute direct peer behavior measures for perceived peer behavior measures and by then comparing the structural relation between peer behavior and personal behavior across each model.

2.4 Estimation Method

Given that our respondent and peer delinquency items are positively skewed (see Table 1) thus violating the maximum likelihood assumption of multivariate normality (Browne, 1984) and that Pearson correlations have been shown to underestimate the relations among ordinal measures (Muthén, 1984), we have opted not to rely on traditional maximum likelihood estimation using a Pearson correlation matrix. Instead, following prior research using ordinal measures similar to our own (e.g., Aseltine, 1995; Matsuda & Anderson, 1998; Rebellon, 2012) we first use the *Prelis 2* (Jöreskog and Sörbom, 1993b) statistical package to compute polychoric correlations and their associated asymptotic covariance matrix. We then use these correlations and weights to estimate our measurement and structural equation models via the diagonally-weighted-least-squares (DWLS)² algorithm. This estimation strategy provides three primary benefits for our present purposes. First, “[r]ather than assuming normality among observed items, this asymptotic distribution-free method of estimation assumes normality of the latent structure underlying each item as well as bivariate normality underlying the estimated polychoric correlations” (Rebellon and Waldman, 2003:315). Second, simulation research (Babakus et al., 1987:227) finds that, relative to alternative designs using ordinal data, “[t]he polychoric correlation produce[s] better results on the basis of the accuracy of parameter estimates and estimated standard errors.” Third, from a practical standpoint, prior research employing the error-adjustments whose integrity we wish to test (e.g., Matsueda and Anderson, 1998; Rebellon, 2012) has used polychoric correlations. Thus, to examine whether the results of those studies using perceived peer measures do or do not reflect results that would have emerged had direct peer measures been available, we follow their methodology as closely as possible.³

² Models run via fully-weighted least squares estimation failed to converge, likely as a result of our models’ complexity and limited sample size.

³ One reviewer suggested that we consider an alternative strategy involving transforming our indicator items to reduce skew and estimating our models via the traditional maximum likelihood algorithm. We therefore ran an

3. RESULTS

Figure 1 presents the mean squared-difference between each respondent's self-reported violence and each corresponding friend's violence. The first bar in each pair depicts the mean squared-difference between personal violence and *perceived* friend violence while the second depicts the mean squared-difference between personal and *directly-measured* friend violence. Across each pair of bars, the first is smaller than the second, suggesting that respondents systematically overestimate the similarity between their own behavior and that of their friend. Our data therefore suggest that projection effects take place not only for the sexual and substance-related behaviors examined in the majority of existing psychology studies, but also for more severe forms of deviance like violence.

(Figure 1 about here)

Figure 2 examines whether the biases in perceived measures are (a) sufficiently large to render perceived peer violence a fundamentally distinct construct vis-à-vis directly-measured peer violence and (b) so large as to render perceived peer violence nothing more than an alternative measure of personal violence. Following prior research (e.g., Zhang and Messner, 2000; Rebellon, 2012), it does so via confirmatory factor analysis. A number of results are noteworthy. First, all factor loadings are high and statistically significant, suggesting that pushing, hitting, kicking, and using force are, in fact, good indicators of an underlying behavioral tendency. Second, each of the statistics presented at the bottom left of Figure 2 suggests a good overall fit of the theoretical model to our data. Third, three error-correlations are positive, two of

alternative set of models in which we (a) computed the natural log of each of our skewed indicator items; (b) computed traditional Pearson correlations; and (c) estimated all measurement and structural equation models via the maximum likelihood algorithm. Substantive results (available upon request) were identical to those presented herein, with one minor exception, noted below, that does not change our conclusions in any way. Because conclusions remained the same, and for the reasons discussed in our section entitled “2.4 Method of Estimation”, we present DWLS results using polychoric correlations.

them to a statistically significant degree, and this is consistent with measurement bias as per Matsueda and Anderson (1998). Fourth, even after estimating the error correlations among corresponding items for personal and perceived peer behavior, perceived peer behavior is correlated significantly less than 1.0 with directly-measured peer behavior. Thus, insofar as each friend's directly-measured self-reports of violence are taken to be a relatively valid baseline measure of actual behavior (see Huizinga and Elliott, 1986), respondent perceptions are not merely measurement-biased indicators of actual peer behavior but, instead, indicators of a construct that is fundamentally distinct from actual peer behavior. Fifth, personal behavior and directly-measured peer behavior are significantly correlated, but the magnitude of this correlation is much lower than the magnitude of the correlation between personal and perceived peer behavior. Finally, while the magnitude of the correlation between personal and perceived peer violence is extremely large in Figure 2, it differs significantly from a value of 1.0, as indexed by two standard errors, thus calling into question Gottfredson and Hirschi's (1990) assertion that indicators of personal and perceived peer delinquency are merely alternative indicators of the same underlying construct.⁴

(Figure 2 about here)

Given clear evidence from Figure 1 that respondents systematically overestimate the similarity between their own behavior and that of their chosen friend, as well as clear evidence that the items depicted in Figure 2 reflected three distinct constructs, Figure 3 depicts a structural equation model estimating the latent variable correlation between personal and perceived peer

⁴ For purposes of comparison, the three-factor model presented in Figure 2 fit the data better than did (a) a one-factor model ($\Delta\chi^2=525.10$, $df=7$, $p<.05$); (b) a two-factor model in which perceived and direct peer behavior loaded on one factor ($\Delta\chi^2=322.98$, $df=6$, $p<.05$); (c) a two-factor model in which personal behavior and perceived peer behavior loaded on one factor ($\Delta\chi^2=182.81$, $df=6$, $p<.05$); and (d) a three-factor model omitting error-correlation estimates of projection ($\Delta\chi^2=39.90$, $df=4$, $p<.05$). Respectively, in comparison to the Figure-2 model's RMSEA of .026, the above alternatives yielded poorer fits of .191, .153, .115, and .058.

violence net of adjustments for gender and low self-control. As in Figure 2, and again following Matsueda and Anderson (1998), we model projection effects by estimating the correlation between error terms for manifest indicators of perceived friend's behavior and corresponding error terms for manifest indicators of personal behavior. Two of the four error correlations are statistically significant, thus paralleling measurement results from Figure 2. Males have higher perceptions of friend's violence and have lower levels of self-control. As expected, low self-control is associated with greater personal violence, and males have higher levels of violence even after controlling for self-control and perceived peer violence. Most importantly, even after adjusting for the contemporaneous correlations among manifest indicators of perceived peer behavior and personal behavior, Figure 3 demonstrates that the structural coefficient linking perceived friend's violence and personal violence is statistically significant and very high in magnitude.

(Figure 3 about here)

Figure 4 examines the degree to which the error-correlation adjustments outlined by Matsueda and Anderson (1998) do or do not narrow the gap between perceived and directly-measured peer coefficients. In particular, it repeats the structural analysis presented in Figure 3 with two exceptions. First, it substitutes directly-measured peer behavior in place of perceived peer behavior. Second, given its direct measures of peer behavior, it does not estimate error term corrections. Comparing the structural coefficients across Figures 3 and 4 indicates clear evidence that, even after adjusting for error term correlations in Figure 3, the coefficient linking perceived peer behavior and personal behavior is still *much* larger in Figure 3 than is the corresponding coefficient linking directly-measured peer behavior and personal behavior in Figure 4. The Figure-4 coefficient, in fact, fails to reach statistical significance net of self-control

and gender⁵ thus suggesting that the significant correlation between personal and directly-measured peer behavior in Figure 2 is spurious.

While the error correlations estimated in Figure 3 do not bring the Figure 3 peer coefficient fully into line with that of Figure 4, we note that two of these error correlations are significant. It therefore remains possible that omitting the error correlations in Figure 3 would yield an even more biased peer coefficient. To examine whether the error correlation strategy may at least limit the bias resulting from the use of perceived peer measures, we re-ran the model presented in Figure 3 and omitted these error correlation estimates. The resulting model yielded an unstandardized peer coefficient of 1.09 (SE=.18), as opposed to the original unstandardized coefficient of .93 (SE=.14). This suggests that, although estimating error correlations does not render perceived peer coefficients equivalent to direct peer coefficients, it does decrease them by about 15%, thus only partly mitigating the bias that characterizes perceptions of peer behavior.⁶ Our results therefore call into question whether prior studies using the strategy outlined by Matsueda and Anderson (1998) with perceived peer measures can be said to produce peer coefficients equal to those that would emerge were direct peer measures available in such studies' datasets. Rather, it would appear that the error term correlations in question do not serve to control for the contamination that results from such sources as projection and/or that perceptions of peer behavior reflect a construct that is greater than the sum of actual peer behavior and measurement bias.

⁵ The corresponding coefficient from our maximum-likelihood model did achieve statistical significance, but the gap between Figure-3 and Figure-4 coefficients when using maximum-likelihood estimation was similar to the gap presented in Figures 3 and 4. Thus, conclusions about the research questions that we discuss at the outset were identical to those drawn from DWLS estimates, whose standard errors are likely to be more valid in light of the methodological literature we cite in our section entitled "2.4 Method of Estimation".

⁶ Given that our alternative models have used the same sample with different measures of peer violence rather than the same measures of peer violence with different samples, it is not appropriate to compare the coefficients of alternative models via the Paternoster et al. (1998) formula.

In light of the above results, it appears to us that the dominant paradigm for exploring the relationship between personal and peer delinquency may be fundamentally flawed insofar as it treats perceived and direct measures of peer delinquency as alternatives from which to choose rather than as fundamentally separate variables that might both be included in a full model of delinquency (for a notable exception, see Jussim and Osgood, 1989). In particular, our measurement model suggests the latent variable correlation between perceived and direct peer measures to be significantly (and substantially) less than 1.0 and our structural models suggest that the relationship between personal delinquency and each of the two peer measures is vastly different even after applying error correlation adjustments suggested by Matsueda and Anderson (1998). We therefore provide a supplemental analysis assessing one way in which direct and perceived measures of peer delinquency may simultaneously be related to personal behavior. In particular, given that only the portion of peer delinquency that is known to respondents may impinge directly on respondent behavior, our model includes perceived peer behavior along with self-control and gender as direct predictors of personal violence. Simultaneously, however, the model includes directly-measured peer violence as a predictor of perceived peer violence.

(Figure 4 about here)

Results from our supplemental analysis are depicted in Figure 5. They replicate the Figure 4 finding that perceived peer behavior is strongly associated with personal behavior even after adjusting for low self-control. More critically, results suggest that perceptions of peer behavior are partly reflections of directly-measured peer behavior, that males attribute greater violence to their friends than is called for on the basis of directly-measured friend's violence, and that those low in self-control perceive greater peer violence even after adjusting for gender and directly-measured peer violence. Most importantly, results depicted in Figure 5 reveal that more

than sixty percent of *perceived* peer violence remains unexplained by gender, self-control, and directly-measured peer violence. Of note, although factor loadings have been omitted from Figure 5 for clarity of presentation, they appear in Appendix A and demonstrate that two of four error term correlations are once again statistically significant. Thus, sixty percent of perceived peer violence remains statistically unexplained net of controls and net of the very error term correlations that Matsueda and Anderson (1998) suggest to control for such mechanisms as projection, imputing friendship to co-offenders, and false rumors.

(Figure 5 about here)

4. DISCUSSION

Despite criminological and psychological research demonstrating clear evidence that individuals misperceive the delinquency of their peers, measures of perceived peer delinquency will likely remain relevant in future etiological studies. Even as theorists like Akers (2009), for example, acknowledge the biases that might be claimed to affect the validity of such measures, they nevertheless observe that an individual's perceptions may have etiological import in their own right, regardless of their inaccuracy. Moreover, relatively few datasets contain direct peer measures from dyadic or network data and the added effort required to collect such data may render them rare for some time to come. Thus, even insofar as direct peer delinquency measures are desirable in etiological studies of delinquency, practical considerations suggest that perceived peer measures will remain more commonly available.

Given the continued importance of perceived peer delinquency, the present study addresses two questions about the relationship between personal and perceived peer delinquency. First, are perceived peer delinquency measures merely methodologically-biased representations of the same construct that is reflected by direct peer delinquency measures or are the measures

reflections of fundamentally different theoretical constructs? Second, to what degree do latent variable adjustments render perceived peer coefficients equal to directly-measured peer coefficients? After first demonstrating that respondents in the present sample do appear to project their own behavioral tendencies inaccurately onto friends, results provide evidence that respondent perceptions are not merely biased by measurement, but may in fact reflect a theoretical construct fundamentally distinctive from directly-measured peer behavior. Likewise, structural equation results suggest that perceived measures of peer violence inflate the magnitude of the relation between personal and peer violence substantially, but that estimating corresponding error correlations among indicators of personal and perceived peer behavior does *not* methodologically counteract the majority of this inflation.

In light of these findings, three fundamental implications emerge for both the interpretation of prior etiological research and for the construction of future tests aimed at unpacking the nature of the relationship between personal and peer delinquency. First, whether the manifest error term correlations estimated herein reflect projection, false rumors/bragging, shared method variance, or a combination of all three, they do not render perceived peer coefficients equal to direct peer coefficients. Therefore, studies using perceived peer measures, whether with or without error-correlation adjustments, cannot be interpreted as approximating the *actual* effect of peers, per se, on delinquency. Insofar as policy-makers are interested in the actual effect of peer behavior on delinquency, they will therefore need to rely on studies using explicit measures of peer behavior derived directly from the peers themselves. Decades of criminological research has, if often implicitly, assumed the strong relationship between personal and perceived peer delinquency to mean that peers themselves “matter” much more than do variables whose correlations with delinquency are relatively smaller (see Akers, 2009 for a

detailed review). Yet if perceptions reflect a construct fundamentally distinctive from directly-measured peer behavior, rather than merely measurement-biased indicators of actual peer delinquency, delinquency-prevention efforts that seek to isolate individuals from objectively delinquent peers may actually have little influence on subjective perceptions of peer behavior or on personal delinquency.

Second, while scholars like Akers (2009) may ultimately be correct to suggest that perceptions of peer behavior are etiologically important in their own right, our results suggest a critical need for future research to conceive of perceptions as a *dependent*, rather than exclusively independent, variable (e.g., Young et al., 2011). Even if longitudinal research continues to find that perceptions of peer behavior exert a strong influence over a given individual's delinquency (but see Rebellon, 2012), the criminological community's ability to apply such knowledge for the practical purpose of preventing delinquency necessarily hinges on empirically identifying precise social stimuli capable of altering perceptions. Yet results of our supplemental model suggest that, even after unveiling significant independent associations of perceived peer delinquency with directly-measured peer delinquency, self-control, and gender, nearly seventy percent of the variance in perceptions of peer delinquency remained unexplained. Thus, even insofar as theorists like Akers (2009) claim that perceptions have etiological value independent of actual peer behavior, future theory and research would do well to focus less on comparing the magnitude of perceived peer coefficients with that of other predictors and to focus more attention on what precise social stimuli are most capable of altering inaccurate perceptions so as to usefully inform delinquency prevention policy. Absent further research aimed at identifying where (mis)perceptions come from (e.g., Young et al., 2011), policy-makers will continue to encounter evidence that delinquency-prevention hinges on changing perceptions of

peer delinquency while simultaneously lacking a deep well of knowledge concerning how to go about the task of altering those perceptions.

Third, given that the present study finds what we believe to be compelling evidence that perceived and direct measures of peer delinquency are two fundamentally distinct constructs, we believe that criminologists should pay much greater attention to theorizing the manner in which *both* constructs are simultaneously related to personal delinquency (and to each other). Within the limited literature that has employed both measures in etiological analysis, the vast majority of studies have conceived of the two measures as alternatives from which to choose, rather than as fundamentally distinct constructs that may each contribute to delinquency (e.g., Meldrum et al., 2009; Weerman and Smeenk, 2005; Haynie, 2001, 2002). This general approach dates back to the seminal contributions of such scholars as Newcomb (1961) and Kandel (1978), as well as to the broader false-consensus literature that flourished in the late 1970s and throughout the 1980s (see Krueger and Clement, 1994; Marks and Miller, 1987). Yet the focus of even these precedent literatures revolved primarily around identifying how much “error” exists in perceived peer measures vis-à-vis actual measures of peer delinquency. Whereas the prior literatures’ focus has led most researchers to include one *or* the other measure within a given statistical model, our results suggest to us that it might be most appropriate to include both simultaneously in a full model of delinquency. Methodologically, including both may serve to prevent omitted variable bias while, theoretically, including both may provide a more accurate representation of the complex processes that link peers, perceptions, and personal behavior.

Given the above implications, we believe three issues merit particular attention in future research. First, we believe it is critical that future research attempt to replicate this study’s results. While we are not the first to suggest that replication often receives too little attention in

criminology analysis (Lowenkamp et al., 2003), we echo this sentiment. Of particular importance, it may be the case that this study's findings may apply to severe forms of delinquency like violence, but less so to more common behaviors like drinking or substance use. Notwithstanding the still mainstream view that all forms of delinquency are "general" phenomena, reflecting the same fundamental tendency and bearing common etiological antecedents, a developing research literature finds evidence that different forms of delinquency may not reflect identical underlying constructs (e.g., Rebellon and Waldman, 2003; Osgood and Schreck, 2007). Some forms of delinquency, for example, may be particularly prone to take place in groups, to be learned from peers, or to be reinforced in certain social contexts (Warr, 2002; Rebellon 2006). Future research should therefore replicate the present findings with a similar focus on violence, but then also examine whether the present findings apply equally to other forms of delinquency. Even if further research replicates our findings for violence, it might still be the case that perceived and actual peer behaviors like substance use reflect the *same* underlying theoretical construct and that error-correlation adjustments render perceived peer substance use coefficients similar to actual peer substance use coefficients. For each of these reasons, we encourage replication of the present findings with a variety of delinquency measures and across a variety of cultures/subcultures.

Second, if research confirms that perceived and direct peer measures reflect fundamentally separate constructs, we encourage criminologists to construct and test theoretical models that include both. We have provided one preliminary example of such a model. Results show that after controlling for the expected significant relationships that self-control and biological sex bear with respondent violence, respondent violence was significantly associated with perceived peer violence. Perceived peer violence, in turn, was significantly associated with

directly-measured peer violence. We note, however, that our model can only be viewed as preliminary in light of several data limitations including our identification of only one respondent-selected friend and a cross-sectional design that precludes our assessment of reciprocal influences across time. While our inclusion of self-control as a covariate may partly mitigate our inability to control for prior delinquency, we note that even after adjusting for self-control, gender, directly-measured peer behavior, *and* shared method variance (via error correlation adjustments), nearly seventy percent of the variance in respondent perceptions of one friend's behavior remains unexplained. Thus, we suggest that future research elaborate our preliminary test by building upon latent trait-state models emerging within the psychological literature (Geiser and Lockhart, 2012) and on the longitudinal methods of Jussim and Osgood's (1989) analysis. While Jussim and Osgood's research is often cited as evidence that perceptions of delinquency are inaccurate, this study's greater import may actually stem from its longitudinal attempt to model projection effects via a method that, in light of the present results, may hold greater promise than error-correlation adjustments. In particular, we suggest that future criminological research modify Jussim and Osgood's (1989) model for use with network, rather than only dyadic, data and for use with general adolescent samples rather than officially-documented offender samples.

Third, future research should attempt to address a number of further limitations inherent to the present design. For example, although recent research suggests that university students provide valid samples for criminological research (Wiecko, 2010), our data come from a small convenience sample and are not representative of university students in general, let alone the overall population. However, while the generalizability of our results remains an open question, we note that one of our driving research questions was as assessment of whether latent variable

corrections via error term correlation estimates render structural coefficients derived from perceived peer measures equal to those derived from direct peer measures. Whether the data used to examine this methodological issue are representative should not impinge on this question. If the error-term correction did not serve to bring structural coefficients from perceived peer models into line with those from direct peer models in our data, we see no methodological reason to expect the correction to work better in another data set simply on the basis that such data may be more representative. Further, the theories from which we derived our models are explicitly claimed to be general theories that should apply across time, across place, and across gender. It is instructive, for example, that our self-control measure worked as would be expected based on prior theory and based on prior empirical research. Likewise, gender, perceived peer behavior, and directly-measured peer behavior all yielded bivariate correlations with violence in the directions expected from the existing literature.

Beyond the representativeness of our sample, our measures of perceived and actual peer violence are based on data from only one friend. Traditional measures of perceived peer delinquency, however, tend to ask about the proportion of one's friends who have engaged in a given behavior. Preliminary research (see Haynie and Osgood, 2005) suggests that the proportion of delinquent friends, as measured via network data, may be more important than the amount of total crime that any one friend has committed. At the same time, this means that our study should reflect a conservative test. In particular, it is noteworthy that data from only one friend was sufficient to unveil an extremely strong and statistically significant relation between personal behavior and perceived peer behavior, even after controlling for self-control and gender, which represent two of the strongest and most consistent correlates of delinquency. Moreover, although our fit statistics suggest adequate model fit, we again note that our purpose was *not* to test a

complete etiological model of respondent violence, but rather to (1) examine the discriminant validity of a perceived peer measure vis-à-vis a personal measure and a directly-measured peer measure and (2) and examine whether an error-adjusted measure of perceived peer delinquency could serve as a valid proxy for direct peer delinquency. Given the clearly stronger relation that our study finds between personal and perceived peer delinquency versus personal and directly-measured peer delinquency, misperception for multiple friends is unlikely to be smaller.

Similarly, if the latent variable adjustments in Figure 3 were not able to replicate structural results from Figure 4 using data from one friend, we have little reason to believe that they would be able to do so with data from multiple friends.

Finally, we have used cross-sectional data and have implicitly assumed a friend's self-report is the gold-standard by which respondent perceptions should be judged. We encourage future research to replicate the above analyses via longitudinal designs, but point out that cross-sectional designs have been deemed appropriate for exploring related issues in the recent past (e.g., Young et al., 2011; Boman et al., 2012). Further, our measurement models are not affected by the use of cross-sectional data and our structural models are primarily intended as a platform with which to test whether adjusting for measurement artifacts yields results comparable (aside from their etiological validity) to results derived from models incorporating direct measures of peer delinquency. We also recognize that there are likely multiple sources of potential error in peer self-reports. For example, it is possible that friends (and respondents themselves) are untruthful. We have attempted to guard against deliberate deception by ensuring participants of their anonymity but, even to the degree that respondents/friends attempted to answer accurately, their memory may be imperfect. However, while future research should not lose sight of the need to validate direct measures of peer behavior, prior research concludes that self-report

methodology yields generally reliable and valid measures (e.g., Huizinga and Elliott, 1986). Further, the limits of our self-reported methodology are shared by numerable micro-level studies of crime using the same approach.

Despite these limitations, our study provides an important latent variable test examining whether perceived peer behavior measures reflect measurement-biased indicators of directly-measured peer behavior or instead reflect a construct fundamentally distinctive from directly-measured peer behavior. Our findings demonstrate that perceived and direct peer measures reflect fundamentally separate constructs, at least for violence. Likewise, our study provides the first structural equation assessment of the degree to which error term corrections using perceived peer delinquency measures can successfully replicate results of models using direct peer delinquency measures. Our findings suggest that they cannot. Both of the above findings suggest that future research concerning the relationship between personal and peer behavior cannot bypass the collection of directly-measured peer delinquency. Instead, future research should build more systematically on Jussim and Osgood's (1989) example by formulating and testing longitudinal models of delinquency that assign a role to both directly-measured *and* perceived peer delinquency. Beyond this, both findings further suggest that future theory and research should follow Young et al.'s (2011) lead in attempting to identify and model the specific sources of perceptions that remain theoretically and empirically unidentified. By further theorizing and researching what specific stimuli impinge most versus least in the development of perceptions, criminologists could prevent policy makers with limited resources from "concluding in frustration that everything matters" (Sampson, 1999:446).

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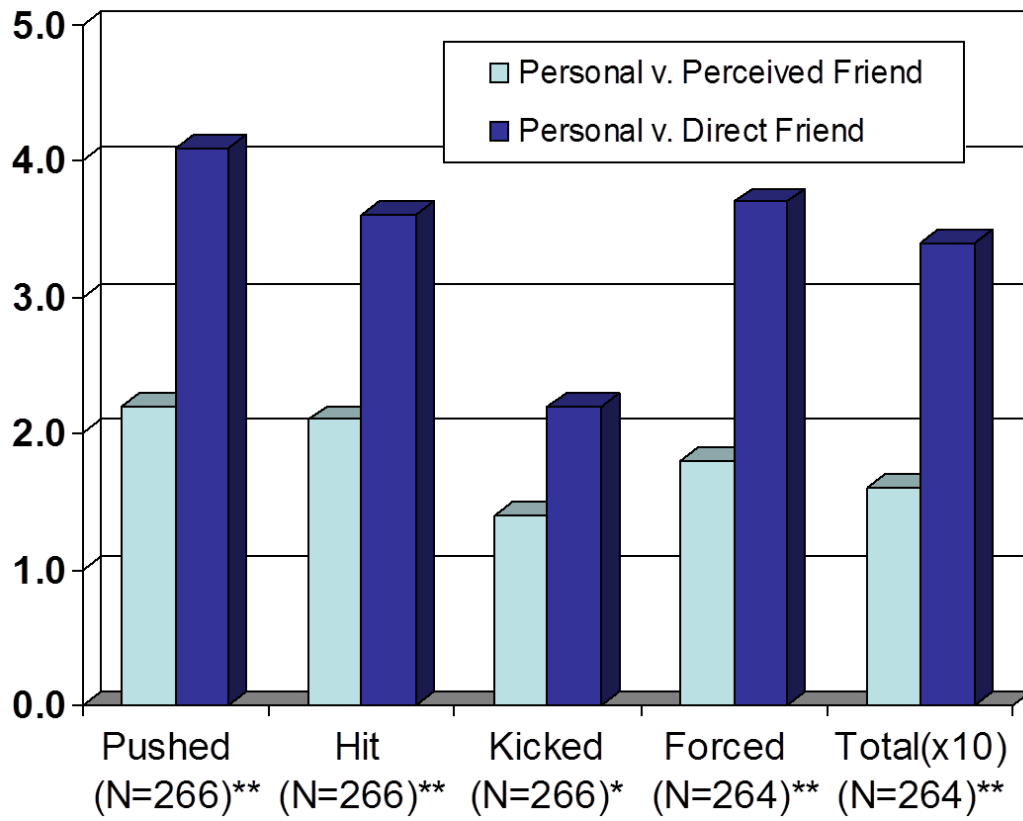
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Table 1. Descriptive Statistics

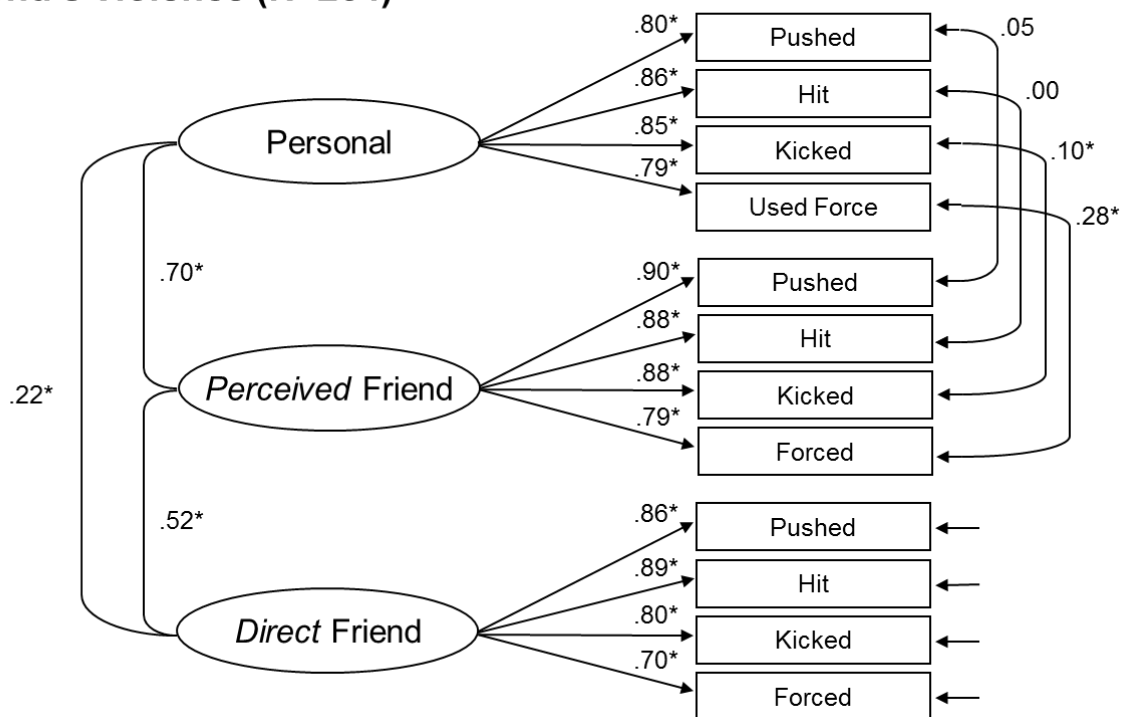
	N	Min	Max	Mean	SD	Skew
Personal Violence						
Pushed	269	1	9	2.04	1.481	1.957
Hit	269	1	9	1.88	1.450	2.348
Kicked	269	1	9	1.51	1.205	3.456
Forced	269	1	9	1.71	1.422	2.837
Perceived Friend's Violence						
Pushed	268	1	8	1.88	1.414	1.909
Hit	268	1	9	1.80	1.381	2.353
Kicked	268	1	9	1.56	1.260	3.150
Forced	266	1	9	1.67	1.386	2.612
Direct Friend's Violence						
Pushed	267	1	9	2.13	1.614	2.329
Hit	267	1	9	1.85	1.482	2.803
Kicked	267	1	7	1.50	1.016	2.906
Forced	267	1	9	1.65	1.299	3.071
Low Self-control						
Impulsive	269	1	4	2.15	.509	.457
Simple Task	268	1	4	2.07	.532	.178
Risk	268	1	4	2.60	.625	-.213
Physical	268	1	4	2.68	.594	.188
Selfish	268	1	4	1.81	.571	.890
Temper	268	1	4	1.94	.631	.357

Figure 1. Mean Squared-Difference between Respondent's Personal Violence and Friend's Violence



*p<.05, **p<.01 (one-tailed)

Figure 2. Assessing the Latent Variable Discriminant Validity of Personal Violence, *Perceived* Friend's Violence, and *Direct* Friend's Violence (N=264)^a



$\chi^2=55.63, df=47, p=.18$

$\chi^2/df=1.18$

AGFI=.99

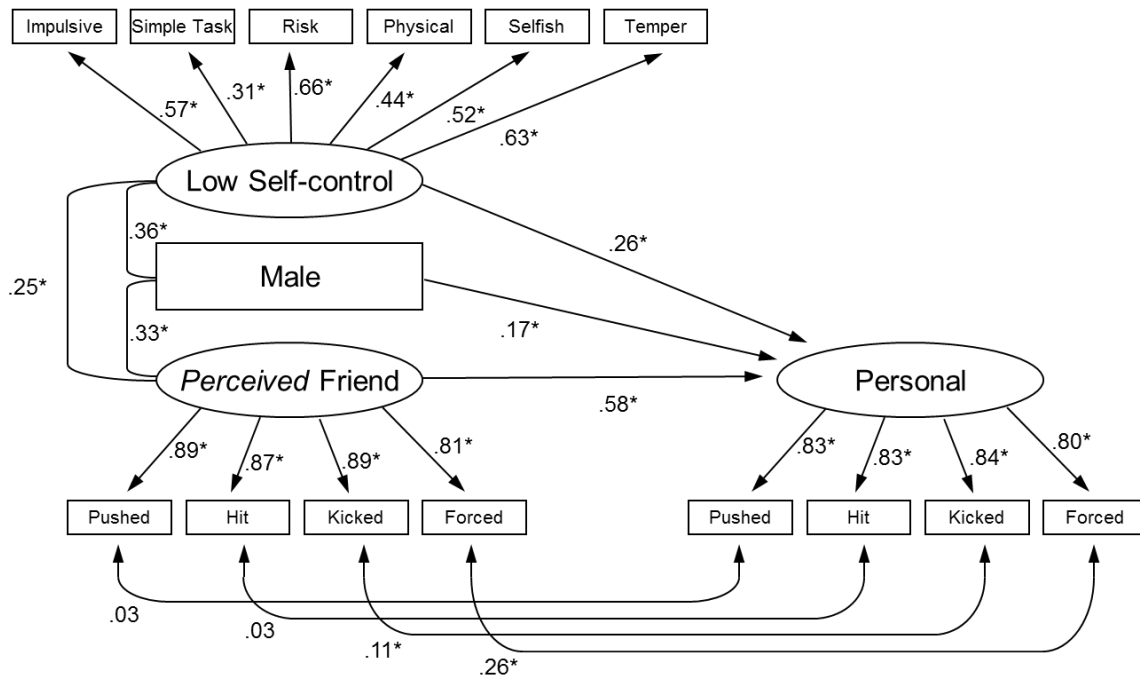
CFI=1.0

RMSEA=.026

^aFully standardized solution shown

*p<.05 (one-tailed)

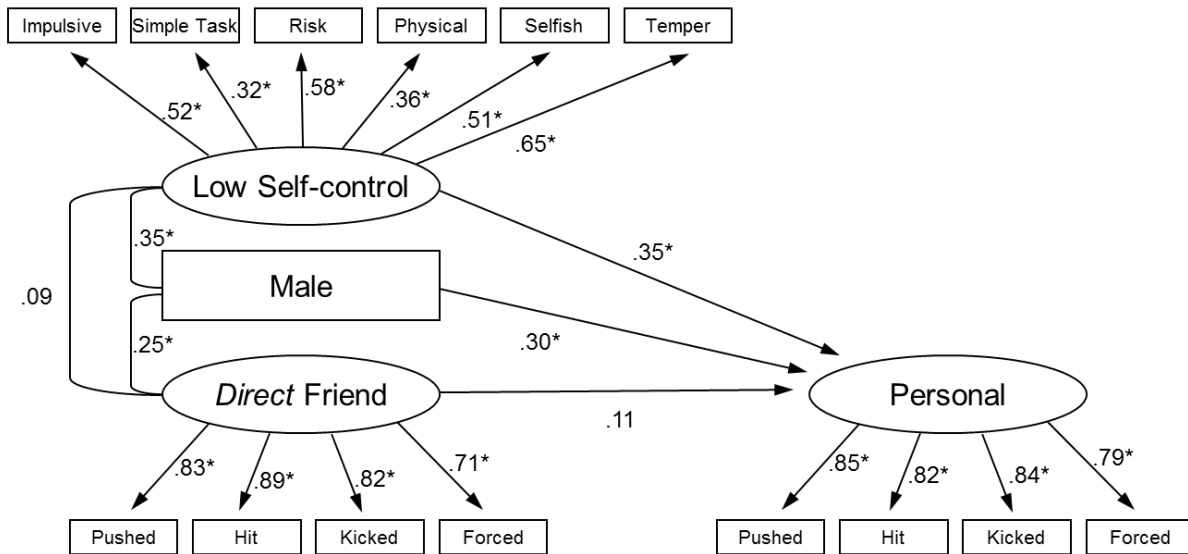
Figure 3. A Structural Model of the Relationship between Personal and Perceived Friend's Violence (N=264)^a



^aFully-standardized Coefficients
*p<.05 (one-tailed)

$\chi^2=142.99$, $df=81$, $p<.05$
 $\chi^2/df=1.77$
 AGFI=.97
 CFI=.99
 RMSEA=.054

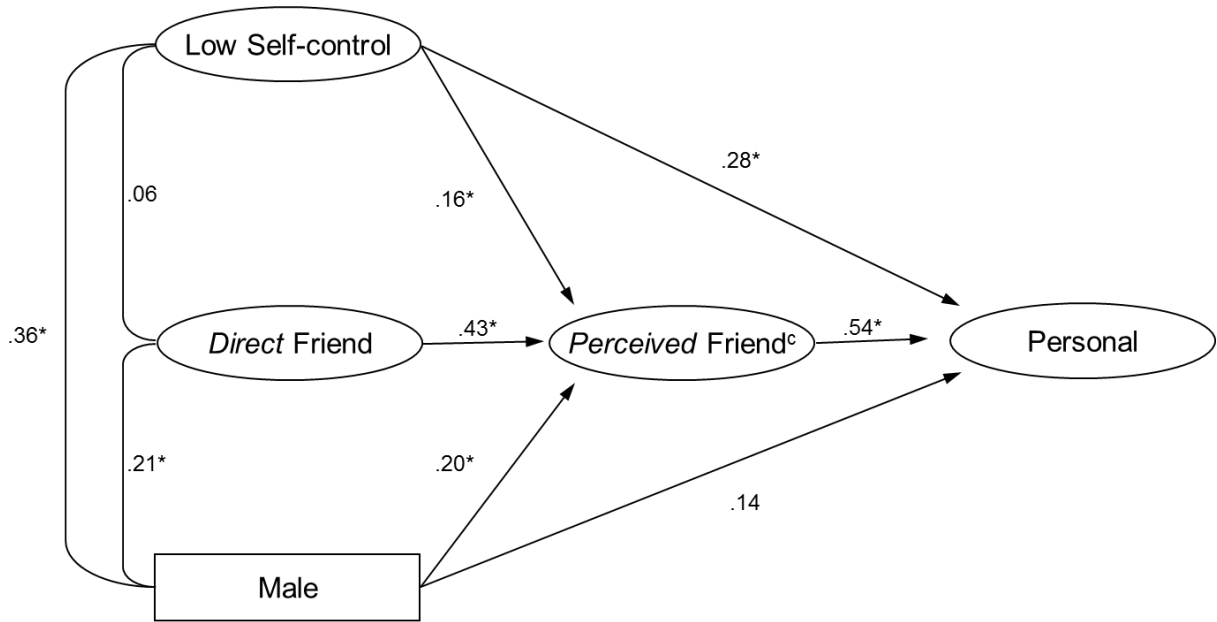
Figure 4. A Structural Model of the Relationship between Personal and *Direct Friend's* Violence (N=264)^a



^aFully-standardized Coefficients
*p<.05 (one-tailed)

$\chi^2=157.21, df=85, p<.05$
 $\chi^2/df=1.85$
 AGFI=.96
 CFI=.99
 RMSEA=.057

Figure 5. A Combined Structural Model of the Relationships among Personal, *Perceived Friend's*, and *Direct Friend's* Violence (N=264)^{ab}



^aFully-standardized Coefficients

^bFactor Loadings and Error Correlations in Appendix A

^cSixty-eight percent of variance remained unexplained

*p<.05 (one-tailed)

$\chi^2=195.55, df=140, p<.05$

$\chi^2/df=1.40$

AGFI=.97

CFI=.99

RMSEA=.039

Appendix A: Measurement Model^a

	Personal	Perceived Friend	Direct Friend	Low Self-control	Projection Error Correlation (Personal/Perceived)
Push	.82*	.91*	.86*	--	.06
Hit	.83*	.88*	.89*	--	.06
Kick	.84*	.89*	.80*	--	.14*
Force	.85*	.81*	.69*	--	.29*
Impulsive	--	--	--	.56*	--
Simple Task	--	--	--	.31*	--
Risk	--	--	--	.65*	--
Physical	--	--	--	.44*	--
Selfish	--	--	--	.52*	--
Temper	--	--	--	.53*	--

^aLoading for "Male" fixed to a value of 1.0

*p<.05