THE EFFECTS OF JOINING AND LEAVING A GANG ON DELINQUENT OFFENDING

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Abstract

Research on the effects of gang membership on offending has been guided by Thornberry et al.'s (1993) framework of selection, facilitation and enhancement, which correspond to a "kinds of persons" explanation, a "kinds of groups" explanation, and a mix of the two. Krohn and Thornberry (2008) recently summarized the research in this area, concluding that evidence in favor of facilitation effects exceeded that of selection effects. Using a recent longitudinal survey of adjudicated youth, we assess the effect of both gang joining *and* gang leaving on offending. Contrary to the extant literature, propensity score matching techniques indicate large selection effects and very little evidence of a causal impact of either gang joining or gang leaving on offending. Methodological, conceptual, and empirical implications for the literature are discussed.

Keywords: gangs, desistance, crime, delinquency, propensity score matching

JEL codes: K14, K42, J29

Gangs pose a problem of a considerable magnitude for contemporary cities. Approximately 25 percent of the homicides committed between 2002 and 2006 in the 100 largest U.S. cities were gang-related (Decker and Pyrooz, 2010). In Los Angeles and Chicago in 2004, over half of all homicides were gang-related (Egley and Ritz, 2006). Gangs also generate problems in community and school settings, increasing perceptions of disorder and fear (Howell, 2006; Katz, Webb, and Armstrong, 2003; Tita and Ridgeway, 2007). These problems extend to individuals, with evidence showing that gang membership induces increased criminal activity for individuals involved in the gang (Thornberry, 1998). Should this increased criminal activity lead to official sanctioning from the criminal justice system, additional negative consequences may follow (Huizinga and Henry, 2008).

Thornberry, Krohn, Lizotte and Chard-Wierschem (1993) proposed three explanations for the relationship between gang membership and delinquency—selection, facilitation, and enhancement. The selection model is a "kinds of persons" explanation, suggesting that crime-prone youth select into gangs and that any increased delinquency should not be attributed to gang membership. This is consistent with a propensity or syndrome explanation of delinquency (Gottfredson and Hirschi, 1990; Jessor and Jessor, 1977; Newcomb and Bentler, 1988). The facilitation model is a "kinds of groups" explanation (Akers, 2009; Sutherland, 1947), suggesting that the increased delinquency of gang members is purely attributable to the influence of the gang, particularly group processes associated with gang membership. The enhancement model combines these themes. Thornberry and colleagues' framework has attracted a great deal of attention and these explanatory themes have been tested employing a diverse set of methodologies (see Table 1). In a recent assessment of this literature, Krohn and Thornberry

(2008:147) concluded "there is a minor selection effect, a major facilitation effect, and no evidence consistent with a pure selection model."

Despite the importance of these themes, a handful of limitations constrain the literature. First, since longitudinal data are required to examine these hypotheses, studies have been limited to select datasets. Second, less attention has been devoted to desistance from gang membership in spite of equally important implications (Pyrooz, Decker, and Webb, in press). Battin, Hill, Abbott, Catalono, and Hawkins (1998:108, emphasis added) pointed out that "additional research is needed before, during, and *after* gang membership." Third, most research to date has strictly attested to the *effect* of gang membership while largely ignoring the theoretical implications. Proponents of the selection model would argue that an enhancement effect is evidence of a "kinds of persons"—not groups—theme.

This paper addresses the above shortcomings using longitudinal data consisting of 1,354 high-risk youth in Philadelphia and Phoenix, followed for a five-year period beginning in 2000. We propose to answer the following two questions: First, does joining a gang lead to increased delinquency? While this has been answered in the affirmative by prior studies, a more rigorous approach to controlling for selection into gangs on a different sample is warranted. Second, does leaving a gang lead to reduced delinquency? This aims to identify important differences between effects of gang membership while in the gang versus after leaving the gang—an original contribution to this literature.

EXPLAINING THE RELATIONSHIP BETWEEN GANG MEMBERSHIP AND DELINQUENCY

Thornberry et al. (1993) introduced three explanations for the effect of gang membership on delinquency: selection, facilitation, and enhancement. These explanations help identify

important themes driving the gang literature. The selection explanation implies that a gang is simply a collection of persons with shared individual deficits such as poor self-control. In other words, the gang itself has no causal influence on criminal behavior. This "kinds of persons" model is supported if gang members have greater delinquency involvement before, during, and after gang membership than non-gang joining youth. Since individuals who join gangs have an elevated criminal propensity, it should be evident across time (in the form of offending) regardless of gang membership.

The facilitation explanation suggests that gangs have a causal influence on delinquency—*but for* gang membership, an individual would not engage in certain actions. The causal effect of the gang extends beyond mere opportunity, arising from features of the gang itself (e.g., organization, rivalries, cohesion, etc; see Decker and Van Winkle, 1996; Kissner and Pyrooz, 2009; Short and Strodtbeck, 1965). This "kinds of groups" model is supported if the delinquency of gang youth is no different from the delinquency of non-gang youth prior and subsequent to gang membership, but is elevated during gang membership. Propensity plays no role in the pure facilitation explanation, thus there should be few differences in offending between individuals except during periods of gang membership.

The enhancement explanation combines selection and facilitation effects. This "kinds of groups *and* persons" model is supported when there is evidence of a selection effect—more delinquent youths are recruited into gangs—*and* a facilitation effect whereby delinquency is increased during gang membership relative to non-gang youths with similar criminal propensities. With both mechanisms at work, gangs attract individuals with propensities toward delinquency and then group processes associated with gang membership produce greater offending rates.

Two major undercurrents contributed to the impact of Thornberry et al.'s three explanations on the gang literature. First, scholars readily identified gangs as having a causal effect on crime, both on individuals and communities (Hagedorn, 1988; Klein, 1971; Short and Strodtbeck, 1965; Spergel, 1964; Thrasher, 1927; Vigil, 1988). Few questioned the temporal order of offending and gang membership (but see Fagan, 1990; Gottfredson and Hirschi, 1990), and as Thornberry et al. (1993:60) mentioned, ethnographic and (gang to non-gang) comparative research "focus[ed] so much attention on active gang members they los[t] sight of the fact that gang members have delinquent careers before and after they are gang members." In essence, the introduction of these explanations questioned the longstanding view that gangs cause crime.

The second reason for the impact of the explanatory models is that they had implications for the three dominant theories of crime in the field —control (Gottfredson and Hirschi, 1990), learning (Akers, 1985) and strain (Agnew, 1992). These theories were touted as explanations of all types of crime across all types of criminals, including gang members. The control perspective was seen as competing with the other theoretical camps, and the gang context was seen as especially efficacious for assessing the theoretical merits. Control theorists contended that the "gang" effect on delinquency was attributed to the self-selection of criminally-disposed individuals into gangs. Learning theorists, on the other hand, held that group processes and mechanisms occurring within the gang facilitated delinquency. The argument essentially boiled down to propensity or motivation—an ongoing debate in the criminological field (McGloin, Sullivan, Piquero, and Pratt, 2007; McGloin and Shermer, 2009).

PRIOR EMPIRICAL RESEARCH

The selection, facilitation, and enhancement explanations have attracted considerable empirical attention. Over twenty studies have provided empirical evidence testing this

relationship, employing diverse methodologies on various criminological outcomes, but we focus here on delinquency at the individual level (Table 1). An important point to consider in this research is how selection is addressed. Evidence in support of selection, facilitation, or enhancement must account for the amount of delinquency Thornberry et al. (1993) specified before, during, and after periods of gang membership. Thus, in assessing the literature on the effects of gang membership, it is important to take into account the sort of statistical technique used to account for selection, and how it accounted for the conclusions reached.

Following Krohn and Thornberry (2008) we partition our review of the literature according to the strategies used to control for selection in assessing the relationship between gang membership and offending. We divide the studies into two groups—studies without selection controls and studies with selection controls (on observable and/or unobservable factors). This allows us to better determine the strength of the evidence supporting selection, facilitation and enhancement.

* Table 1 about here *

Studies without selection controls

Thornberry et al.'s theoretical and empirical introduction to these three explanations used the Rochester Youth Development Study (RYDS), a panel dataset collected from youth enrolled in public schools in Rochester, New York. The relationship was modeled by examining differences in self-reported delinquency between gang members and non-gang youth, and within gang members over three time periods. Results showed that gang members had higher rates of all five types of delinquency than non-gang youth across every time period. To determine the within-gang member changes, the authors partitioned their sample into transient (gang member at one time period) and stable (gang member at multiple time periods) gang members to compare

their delinquency rates across the three time periods. The authors found that the facilitation model best described delinquency for transient members, and that there was some evidence of enhancement for stable gang members, but facilitation best described person offenses. Thornberry et al. concluded that their findings overwhelmingly supported the facilitation model.

Another longitudinal study based in Denver, Colorado (the Denver Youth Study) also reported on this relationship. Esbensen and Huizinga (1993) found that delinquency was highest during periods of gang membership, but future and former gang members still had higher rates of delinquency than non-gang youth. This was deemed as evidence in support of the enhancement model, and indicated there was a substantial degree of selection occurring. A more recent investigation extended the research to a European setting. From a sample of youth gathered from Bergen, Norway, Bendixen, Endresen, and Olweus (2006) found evidence in favor of both enhancement and facilitation effects. The enhancement effect was most pronounced for general delinquency while violent delinquency was consistent with the facilitation effect.

It appears that gang members have higher rates of delinquency prior to gang joining, but gang membership enhances individual offending. Further, departing from the gang coincides with a decrease in offending, but not necessarily to levels of non-gang youth. These conclusions were reached, however, by using t-tests to compare mean offending differences, making it impossible to determine whether these effects are truly attributable to the gang or if third variables are confounding this relationship and driving the above outcomes. This is the main argument of selection proponents, which is why researchers have pursued analytic techniques that introduce statistical controls for selection.

Studies with selection controls

This body of research has proceeded by controlling for both observable and unobservable variables, such as selection into gang membership or criminal propensity. Studies employing these analytic designs attempt to sweep away selection differences and leave only the question of whether gang membership increases delinquency. Research carried out by Battin et al. (1998), Gatti, Tremblay, Vitaro, and McDuff (2005), Hall, Thornberry, and Lizotte (2006), and Zhang, Welte, and Wieczorek (1999) have employed regression-based techniques to model this relationship. These investigations inserted a host of influential control variables in the model to determine if gang membership eliminates their effects. Never has this been found to be the case, which has led these researchers to conclude that gang membership is not the only factor influencing delinquency (i.e., no pure facilitation model).

This relationship also has been modeled using analytic designs that control for both observable and unobservable population heterogeneity. In re-examining the Rochester data, Thornberry and colleagues (2003) employed a random effects strategy that assumes stable time- and individual-differences, and controlled for six additional risk factors. Both current and former gang membership had significant effects on drug sales and general and violent delinquency; however, the effect of current gang membership was at least 2.5 times greater than former leading the authors to conclude that the facilitation effect was larger than the enhancement effect.

A series of studies using data collected as part of the Montreal Longitudinal Study of Boys controlled for unobserved heterogeneity using group-based trajectory modeling (Haviland and Nagin, 2005; Haviland et al., 2007; 2008; LaCourse et al., 2003). The studies by Haviland and colleagues estimated trajectories of violent offending in combination with observed covariates of gang membership to achieve balance when creating propensity scores. Using gang membership as treatment, Haviland et al. found that gang membership had an effect on violence,

but that effect was conditioned by the trajectory to which the individual belonged. Gang membership effects were twice as great in the chronic violent trajectory than in the other two (low and declining) violent trajectories. LaCourse et al. identified three gang membership trajectories—childhood, adolescence, and never gang membership—and found that violent delinquency varied according to these trajectories over time. As gang youth had twice the rate of violent delinquency within all three trajectories, the authors (2003:183) concluded that the "facilitation effect appears homogenous over time and across developmental trajectories."

Finally, two additional studies employed a fixed effects analysis to account for unobserved heterogeneity (Bjerk, 2009; Gordon, Lahey, Kawai, Loeber, Stouthamer-Loeber, and Farrington, 2004). Bjerk used data collected as part of the National Longitudinal Study of Youth, while Gordon et al. used data from the Pittsburgh Youth Study, yet reached similar conclusions. Both studies found considerable evidence in favor of a facilitation effect for a variety of delinquency measures.[1] In fact, delinquency consistently peaked during periods of gang membership. Gordon et al. concluded that their results supported an enhancement model because delinquency was still greater for future gang members compared to non-gang youth.

CURRENT FOCUS

The literature suggests that gang membership exerts an equally robust effect on delinquency regardless of statistical technique, sample country of origin, sample type, and, as a whole, delinquency type. These results led Krohn and Thornberry et al. (2008:147) to conclude that the "weight of the evidence suggests that street gangs do facilitate or elicit increased involvement in delinquency, violence, and drugs. There is no evidence to the contrary and

[1] Bjerk examined the effect of gang membership on drug sales, assaults, property and total crimes, while Gordon et al. focused on aggression, violence, property crimes.

abundant evidence in support of this view." Yet, much more work needs to be undertaken, especially with respect to sampling frames and analytic techniques.

Accordingly, this study addresses two questions: 1) Does joining a gang lead to increased crime? and 2) Does leaving a gang lead to reduced crime? While the first question has been answered in the affirmative by prior studies, a more rigorous approach to controlling for selection into gangs is warranted. The second question points to the important difference between effects of gang membership *while* in the gang versus *after* leaving the gang.

The first question directly pertains to Thornberry et al.'s selection, facilitation and enhancement explanations. Should we find significant pre-gang joining differences between future gang members and non-gang members, we can eliminate the pure facilitation argument. To the extent that we uncover a causal effect of gang membership after controlling for the selection process, we can confirm the enhancement model. The selection model is supported should we uncover selection into gangs and no causal effect once that selection is controlled for.

Our second question has received less attention in the literature but is no less important. Typically, the selection, facilitation, and enhancement models have been studied in terms of delinquency while in the gang. Less is known about the effects of gang membership after one has left. In answering the question of the effects of gang leaving on future delinquency and criminal behavior, an important consideration is the comparison group. If we compare gang leavers to comparable youth who never joined a gang, our estimates reflect long-term effects of gang joining. If, on the other hand, we compare gang leavers to gang persisters, our estimates reflect the difference between gang desistance and persistence over a certain timeframe. Because our second question is focused on the effects of gang leaving, we make the latter comparison.

Contrasting the two questions, if we find that gang joining leads to increased delinquency, and gang leaving to decreased delinquency of equal magnitude, then there is evidence that some aspect of gang membership is largely responsible for elevated levels of delinquency while in the gang. If gang leaving is not associated with a decrease in crime, this could be interpreted in at least two different ways: 1) it could be the case that gang membership makes gang members more crime-prone, either through embeddedness in criminal networks or increases in criminal propensity, and that this effect persists even after youths leave gangs, or 2) if there are strong selection processes at work such that gang membership itself does not have any additional impact on delinquency, then gang leavers and gang persisters would be expected to have equal levels of delinquency since they were equally crime-prone prior to joining the gang and remain so after leaving.

DATA AND METHODS

In order to address these questions, we use data from the Pathways to Desistance (PTD), a longitudinal study that began with 1,354 youth who had been adjudicated guilty of either a serious felony offense (excluding less serious property crime), misdemeanor weapons offense or misdemeanor sexual assault in juvenile or adult courts in Phoenix, Arizona or Philadelphia, Pennsylvania. All youths in the sample were between the ages of 14 and 17 at the time of their offense (Schubert et al., 2004). The study began in 2000, with 6 month follow-up interviews for three years, and yearly follow-ups thereafter. Including the baseline wave, there are currently nine waves of data spanning five years available. Although these data have been used to examine a number of research questions (Brame et al., 2004; Cauffman et al., 2007; Chassin et al., 2010; Chung and Steinberg, 2006; Fagan and Piquero, 2007; Little and Steinberg, 2006; Loughran et al., 2009; Piquero et al., 2005), gang-related research questions have yet to be examined. As

such, PTD is useful for our purposes for a number of reasons. First, it contains many gang members, consistent with other detention samples (Decker, Katz, and Webb, 2008; Kissner and Pyrooz, 2009). Over 200 youths indicated that they were in a gang at the baseline interview, and many others had joined gangs prior to the baseline interview or in subsequent waves. Second, those who are not in gangs, who will be used as comparison cases, are serious juvenile offenders. Compared to a school or general population survey, the sampling frame for this study goes a long way towards reducing selection bias, and is consistent with recommendations in the literature for analysis of serious juvenile offenders generally (Mulvey et al., 2004), and the causes/correlates of persistence/desistance among this group in particular (Laub and Sampson, 2001). Finally, because this survey is longitudinal, we are better able to temporally separate causes and consequences of gang membership, and we are able to assess longer-term consequences of gang membership.

Analytic Strategy

Because studies assessing the effects of gang membership must rely on observational (as opposed to experimental) data, accounting for selection bias in gang membership is of utmost importance. To a great extent, the strategy chosen to account for selection bias depends on the richness of the data available. Sparse data, in which key determinants of gang membership or offending are unobserved, require statistical techniques that deal with unobserved selection bias. Fixed- and random-effects techniques deal with *static* unobserved selection bias, but do not address *dynamic* sources of bias due time-varying characteristics or to time-stable characteristics with time-varying effects. Because adolescence is a time of rapid developmental change, dynamic selection effects are a major concern. Richer data, where the case can be made that all key determinants of gang membership are observed, allow for the use of selection on observables

techniques such as ordinary regression and propensity score matching. We employ propensity score matching because PTD is a rich longitudinal study and because it poses a number of advantages over ordinary regression techniques: (1) it reveals whether the regression assumption of "holding all else equal" is reasonable, (2) it allows finer distinctions in parameter estimates, and (3) it allows for explicit modeling of selection and gang membership effects.

We use observed individual characteristics to construct a propensity score for gang joining (Q1) and gang leaving (Q2). In either case, the propensity score is defined as "the conditional probability of assignment to a particular treatment given a vector of observed covariates" (Rosenbaum and Rubin, 1984:516; see also Rosenbaum and Rubin, 1983, 1985). Using the gang joining model as the example, we write the propensity score in the following way, $e(x) = P(Gang = 1 \mid X)$, where *Gang* is a dichotomous treatment indicator and *X* represents a vector of observed covariates that are presumed to be correlated with either the treatment or the outcome. For our gang joining model, treatment entails joining a gang at some point in the year following the first interview. Because we are interested in the effect of first-time gang joining, we drop all youths who report current or past gang membership at the baseline interview. For the gang leaving model, we start with baseline gang members and treatment is defined as leaving the gang by the first follow-up interview (6 months). We use the cumulative logistic function with a theoretically relevant set of prospective predictors from the initial interview to estimate the propensity score.

The goal of propensity score matching is to balance the observed covariates between the treated and non-treated individuals, conditional on the propensity score $e(x)$. If this goal is met, and no important covariates are unobserved, treatment is assumed to be random conditional on the propensity score (this is known as the conditional independence assumption). Evidence for

the conditional independence assumption (CIA) is assessed through a measure of standardized bias that compares covariates among the treated and matched untreated individuals. This measure, first described by Rosenbaum and Rubin (1985:36), begins with calculation of *unadjusted bias*, which is the difference between the treated and untreated on a particular characteristic divided by an equally weighted combination of the standard error within the two groups (multiplied by 100). If this statistic exceeds 20, the characteristic is considered unbalanced. *Adjusted bias* is calculated in the same way except the matched treated cases are used instead of all untreated cases. The standard error remains the same. If the matched sample reduces the bias below 20, it is considered balanced. To the extent that propensity scores balance pre-treatment covariates, including those not used to create the propensity score, the CIA is supported. Treatment effect estimation then proceeds by comparing the observed outcome of the treated individuals to the observed outcome of their matched, untreated counterparts.

Once propensity scores are obtained, there are a number of methods for matching untreated to treated cases (Smith and Todd, 2005). The simplest is *nearest neighbor matching*, in which the untreated case with the closest propensity score to a treated case is used as a comparison. There are several variants to the method—matching can be done with or without replacement, and individuals can be matched to one or several of their nearest neighbors within a certain range. *Kernel matching* weights untreated cases according to their distance from treated cases on the propensity score metric. In fact, all matching methods may be characterized as weighting functions, but kernel matching allows for finer distinctions in weighting than other methods. As with nearest neighbor matching, there are numerous variations to kernel matching. Here, we use kernel matching with the Epanechnikov kernel, which is equal to zero outside of a specified bandwidth, to create a weighted untreated comparison sample.

There are several attractive characteristics of matching for our study. First, matching techniques highlight the issue of common support. Practically, they show how many of the untreated individuals actually resemble the treated individuals on observed characteristics. In the case of propensity score matching, there may be no untreated cases above a certain propensity score threshold. For these cases, we are unable to estimate a counterfactual. For example, if all youths with a propensity score for gang joining above .8 join a gang, then there are may be no usable comparison cases and we cannot construct a plausible counterfactual case. We can say, however, that given a certain mix of risk factors, treatment is inevitable, at least in the sample used in the analysis. Regression techniques, on the other hand, obscure this issue and can extrapolate treatment effect estimates based solely on functional form when treated and untreated groups are actually not comparable. In many applications, only a small proportion of the untreated population is useful for estimating counterfactual outcomes. Another major advantage of propensity score matching is that it allows the researcher to be specific about what is being estimated. In the case of gang joining, for example, depending on how one applies weights between the gang joiners and gang abstainers, one can estimate 1) the effect of gang joining for the group of individuals who actually join a gang (average treatment on the treated, or ATT), 2) the effect of gang joining for the group who do not join a gang (average treatment on the untreated, or ATU), or 3) the overall average across all individuals, the average treatment effect (ATE). Typically, regression-based models do not make explicit the nature of the parameter so that false conclusions may be drawn about what they actually imply.

Dependent Variable

We use two offending measures as the dependent variable in this analysis. First, from 22 self-reported offending items, we construct a variety scale ranging from 0 to 22 indicating how

many of the items the respondent reported since the previous interview. These items include: destroying/damaging property, fire setting, burglary, shoplifting, trafficking in stolen property, credit card/check fraud, motor vehicle theft, selling marijuana, selling other drugs, carjacking, driving under the influence, paying for sex, rape, murder, shooting at someone (hit), shooting at someone (miss), armed robbery, unarmed robbery, assault, in a fight, beating up someone as part of a gang, and carrying a gun. A variety scale of offending has several desirable properties (Hindelang, Hirschi, and Weis, 1981). First, unlike frequency scales, it is not dominated by less serious property crimes and drug offenses. Second, unlike a prevalence scale, which dichotomizes the sample into offenders and non-offenders, it maintains variations in seriousness between offenders. Since this sample includes only adjudicated youth, prevalence scales are less meaningful. Only 20 percent of the sample reports no participation in any of the 22 items in the six months prior to the first interview, and nearly all of the respondents report having engaged in at least one of the items prior to the first interview. Finally, variety scales tend to correlate very highly with more complicated scales of latent offending propensity (Osgood, McMorris and Potenza, 2002). Generally speaking, highly criminal youth are involved in a variety of illegal behaviors (Monahan and Piquero, 2009), as the violent offender does not refrain from property crime (Piquero, Farrington, and Blumstein, 2003).

We also use official reports of arrest aggregated from juvenile and adult court systems in Phoenix and Philadelphia, and FBI reports. Although we do not have official arrest data from the full range of the study, three outcome waves of official arrests for the gang joining analysis and four waves for the gang leaving analysis are available. The advantage of using official reports in addition to self-reports of offending is that their sources of measurement error are different. Thus, should we arrive at the same answer using both measures, we can be more confident of our

findings. We use raw counts of arrests between interview dates instead of constructing variety scores because offense type is not readily available and arrests are much less common than self-reported offenses.[2]

Treatment variable

Self-reported gang involvement is used as the treatment variable. In every interview wave, youths are asked to report on their gang activity. At the baseline interview, the question is: "In the past six months before you came into the juvenile system on this charge, were you a member of a street gang or posse?" Similar language (i.e., "street gang or posse") is used throughout the survey. Self-nomination is the traditional technique used in the gang literature to operationalize gang membership, and has been described as a "robust measure of gang membership" (Esbensen, Winfree, He, and Taylor, 2001:147; see also Katz, Webb, and Decker, 2005). The largest methodological problem with this approach is that we rely on individual definitions of the words 'gang' and 'posse.' What is particularly problematic is that socially constructed meanings of these words appear to vary quite a bit across sites such that the measured prevalence of gang membership in Phoenix is four times higher than that of Philadelphia at the baseline interview (28% vs. 7%).[3] At the same time, Philadelphia youths report roughly the same amount of peer delinquency using a peer delinquency scale constructed from 12 items. In addition, gang membership is more strongly associated with peer delinquency in Phoenix ($r=.40, p<.001$) than in Philadelphia ($r=.18, p<.001$). Because of this, for the second

[2] The self-reported delinquency variety scale is positively correlated with number of arrests, ranging from .24 to .33 across the outcome waves.

[3] In a supplemental analysis, we examined prevalence rates of gang membership in the Gang Resistance Education and Training (GREAT) study (Esbensen, 2003) and found that at the final two waves of the study when youth in the two cities were 15 and 16, respectively, that 3% and 4% of Phoenix youth reported gang membership, compared to 1% and 0% of Philadelphia youth. As a whole, Phoenix youth were more likely to report current gang membership than Philadelphia youth. In a study on youth in a Phoenix juvenile detention facility, Katz et al. (2005) found that approximately 1 in 5 reported current gang membership—much in line with the present study.

question, we restrict our attention to Phoenix youths. We retain youths from both cities for the first question because so few individuals join gangs for the first time after the baseline interview.

It bears noting that while the nature of our sample allows special opportunities for answering questions about gangs, in some respects it limits our ability to generalize our findings. Specifically, when it comes to measuring the effects of gang joining and gang leaving, we must recognize that this is a sample of youths who were arrested for a serious offense between the ages of 14 and 17. Further, since we are examining the effects of gang joining and gang leaving *after* arrest, this colors the nature of the estimated parameter. We are in fact estimating the effect of first-time gang joining in the year *after* a serious arrest, and gang leaving in the six months *after* a serious arrest. This necessary sequencing is not particularly problematic for our estimate of gang leaving, since it is a relatively normal progression associated with desistance. Yet, it is atypical for a delinquent to first be arrested before joining a gang (Huff, 1998; Klein and Maxson, 2006).[4] Thus, our gang joining estimate applies only to small sub-population of gang members.

Background variables

Of utmost importance in a selection-on-observables strategy (Heckman and Hotz, 1989) such as propensity score matching, is to ensure that the selection process is adequately modeled. While this can never be definitively shown, to the extent that known precursors of the treatment variable are controlled, our case is bolstered. Thus, selection-on-observables strategies are best used in the context of rich data and a treatment with a well-understood selection process. On these two counts we are on firm ground. PTD covers a multitude of topics at every interview date. We draw from these measures, guided by the literature on selection into gangs, to model

[4] We examined age distributions of first gang joining and first arrest in the National Longitudinal Survey of Youth, 1997 cohort. Of the 1,045 youths who ever reported gang membership, over 90 percent were either never arrested or arrested after joining the gang.

selection into and out of gang membership, and to assess the adequacy of the selection models. All background variables are drawn from the baseline interview.

We control for several standard demographic variables, including age, sex, site (a dummy variable for Phoenix), and a set of four mutually exclusive race/ethnicity variables (white, black, Hispanic, other). In both our gang joining and gang leaving models, we control for sex by dropping females, since they exhibited much lower levels of gang activity. For example, only two females joined a gang for the first time in the year following the baseline interview. Also, in the gang leaving model, we control for site by restricting our attention to Phoenix, since gang membership may have different connotations in the two cities.

In addition to these demographic controls, we either include in our models, or assess equivalence of groups (using the adjusted bias statistic) on parental characteristics, parent-child relationship variables, peer measures, unstructured routine activities, social capital, consideration of others, temperance, IQ, educational attainment, employment and work hours, neighborhood disorganization and victimization experiences. We employ over 50 variables to assess equivalence between groups and draw from these variables to build our propensity score models.

Our gang leaving model starts with baseline gang members. We construct a gang embeddedness scale using a mixed graded response model (Samejima, 1969, 1997) applied to a set of five variables that are asked to gang members only: frequency of contact with the gang (four categories), position in the gang (three categories), importance of gang to respondent (five categories), proportion of friends in the gang (five categories), and frequency of gang-involved assaults (four categories). We combine these into a single scale using a graded response model because 1) this approach is flexible enough to incorporate categorical variables with different numbers of categories into a single scale; 2) these items hold together as a latent construct which

we call "embeddedness", and 3) were we to enter these separately into a propensity score model, it would require 16 dummy variables. We separately measure gang expectations ("Do you expect to be a member when back on the street?") because it degrades the reliability of the embeddedness model. In addition, we create a gang organization scale from five yes/no questions pertaining to gang insignia, rules, sharing money, sharing drugs, and having punishments for breaking the rules. We also measure time in the gang based on current age minus self-reported age of gang joining.

RESULTS

Gang Joining

Of the full sample of 1,354 youths, 228 (17%) report active gang membership at the time of the first interview. An additional 87 report previous gang membership at the initial interview, and 4 did not report. This results in a sample of 1,035 youths who report no gang involvement up to the baseline interview. We drop previously gang-involved youths in order to reduce the possibility of reciprocal causation between delinquency and gang membership. We then create a treatment variable based on reported gang involvement among these 1,035 youth at the 6- and 12-month follow-up interviews. Twenty-six youth join gangs by the six month follow-up, and by the 12th month another 13 become involved in gangs, for a total of 39 gang-involved youths, compared to 996 non-gang-involved youths. Of these 39, only two are female (13.6% of the full sample is female), so we exclude females and are left with 37 male gang joiners in the 12 months following the baseline interview, and 842 male gang abstainers over the same timeframe.

Table 2 compares these groups on a subset of relevant variables. We also report Rosenbaum and Rubin's (1985) unadjusted and adjusted bias statistics for each variable. It should be clear from Table 2 that selection effects are evident. Gang joiners are significantly

different from gang abstainers on a number of important dimensions. Importantly for the Thornberry model, they differ in levels of delinquency and correlates thereof. Gang joiners commit more crime, are twice as likely to be high school dropouts, and are three times more likely to have been shot at in the six months prior to the baseline interview. Their parents are less educated, more likely to be born outside of the country and monitor their children less closely. There is also a clear correlation between place, ethnicity, and gang joining as gang joiners are much more likely to be from Phoenix than Philadelphia, and they are much more likely to be Hispanic and less likely to be black. Overall, we assessed pre-treatment unadjusted bias using 52 variables. Nearly half (24) of these variables were unbalanced prior to matching, indicating significant potential for selection bias.

* Table 2 about here *

Our propensity score model for gang joining, shown in Appendix A, incorporates 25 covariates plus some missing observation indicators. The model distinguishes between gang joiners and gang abstainers quite well. The average propensity score for gang joiners is .30 while for gang abstainers it is .03. In fact, there is some loss of sample size due to lack of common support. The two highest estimated propensity scores for gang abstainers are .49 and .64. Of the 37 gang joiners, 10 have estimated propensity scores higher than .49, with the highest reaching .89. As a result, using a bandwidth of .05, 5 of the 37 gang joiners cannot be matched to gang abstainers. Increasing the bandwidth to .10 yields only one additional case on support. A regression-based analysis would retain these off-support cases, relying heavily on distributional assumptions in order to estimate the average effect of gang joining. Only three of 52 variables are unbalanced after matching, providing evidence of meeting the conditional independence assumption.

Table 3 shows two types of estimates across six waves of data for two different offending outcomes. First, we provide unadjusted estimates of the treatment effect of gang joining, which is simply the difference between gang leavers and gang abstainers without any adjustments, with statistical significance assessed with an independent samples t-test. These estimates include the five off-support cases. Second, we provide matching estimates of the effect of gang joining, with statistical significance assessed with weighted independent samples t-tests.[5] These comparisons do not include the off-support cases.

* Table 3 about here *

The first item of interest in Table 3 is the unadjusted estimates. Of six unadjusted comparisons of self-reported delinquency variety, only three of the comparisons are significant prior to adjusting for selection bias. Further, none of the three unadjusted comparisons of arrests are significant. Thus, even without selection controls, there is little evidence for a lasting impact of gang joining on offending. Once we match gang joiners to comparable gang abstainers, the three significant comparisons are no longer significant and the effect magnitudes drop dramatically. There is only one new significant difference and it is in the opposite direction of expectations. Overall, these models provide little evidence of a causal impact of gang joining on offending. Bear in mind however, that these estimates apply to the very small slice of juveniles who join gangs for the first time *after* having been arrested (37 gang joiners for unadjusted estimates, 32 for matching estimates).[6]

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[5] These tests do not take into account error associated with estimated propensity scores. As a result, we underestimate standard errors, and are at elevated risk of Type I error. Because we find very few significant differences, this is of little concern, as taking this additional error into account would lead us to similar conclusions.

[6] As a sensitivity analysis, we re-estimated our propensity score matching models omitting youths who spend over 90 percent of the year after the baseline interview incarcerated. This condition applies to 201 individuals, including about half of the gang joiners. It is possible that youths who join gangs for the first time while incarcerated are joining prison gangs rather than street gangs. Further, if they remain incarcerated in subsequent waves, the estimated treatment effect could be underestimated due to restricted opportunities for offending in that environment. After

Gang Leaving

Next we examine our second question: Does leaving a gang lead to less crime? We begin with the 228 active gang members at the time of the first interview. Of these, over one-third (81 of 228) report no longer being in a gang just six months after the initial interview. In an attempt to build a propensity score model that balanced gang leavers and gang persisters, we came to the conclusion that the process is sufficiently different for males and females that we should focus our attention on males only. There are 206 initial male gang members, 70 of whom desist by 6 months. However, because ethnicity, city, and leaving the gang are inter-related, we decided to focus our attention on Phoenix males only, leaving a final sample of 163 male gang members. Of this initial sample, 42 leave their gang within six months and 121 do not. Paring down our sample in this way allows us to generate more reliable estimates at the cost of reducing external validity.

Descriptive statistics on select variables for gang leavers and gang persisters, unadjusted bias, and adjusted bias figures are shown in Table 4. The biggest differences have to do with gang-related items. The gang leavers are less embedded in their gangs at the initial interview, their gangs are less organized, and their expectations for staying in the gang are only half that of the gang persisters. Not surprisingly, if a youth is less committed to a gang, he is more likely to leave it after being arrested. Also, gang leavers and gang persisters are quite similar in terms of recent delinquency and victimization. The second largest difference between the two groups is in parental monitoring as parents of future gang leavers monitor their children more closely than the gang persisters' parents. Overall, we assess pre-treatment unadjusted bias using 56 variables. Of these, 17 are unbalanced prior to matching, indicating significant potential for selection bias.

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removing these individuals, our gang joining estimates remain consistent with our initial estimates (i.e., within the initial parameter confidence intervals), indicating that incarceration status does not affect our estimates.

* Table 4 about here *

Our propensity score model for gang leaving, shown in Appendix B, incorporates 25 covariates plus some missing observation indicators. The average propensity score for gang leavers is .46 while for gang persisters it is .10. There is considerable loss of sample size due to lack of common support. Of the gang leavers, 14 have estimated propensity scores for gang leaving above .7; there are only 2 gang persisters whose propensity scores are above .7. As a result, using a bandwidth of .05, 11 of the 42 gang leavers cannot be matched to gang persisters with equivalent scores. Increasing the bandwidth to .10 yields only one additional case on support. Fully a quarter of our treatment sample can not be matched to comparable untreated cases, an issue we return to later. After matching, 5 of 56 variables are unbalanced.

Table 5 shows unadjusted and matching estimates across seven waves of data for two offending outcomes. The unadjusted estimates are simply t-tests of differences across gang leavers (after 6 months) and gang persisters. Since many of the gang persisters actually leave gangs after a certain amount of time, this can also be thought of as the difference between individuals who leave gangs in less than six months after being arrested to those who take more time to leave gangs after being arrested. Also, gang leavers after six months may return to the gang in subsequent waves. By the last wave in the survey, for example, 12.5% of the "gang leavers" are back in a gang, and 34.8% of the "gang persisters" are still in a gang, illustrating the fluid nature of gang membership (Krohn and Thornberry, 2008).

* Table 5 about here *

Comparing the two groups without adjusting for pre-existing differences, very few significant differences emerge. In fact, of seven self-reported delinquency and four official arrest comparisons, only one is significant without adjusting for pre-existing differences, simply

comparing all gang leavers to all gang persisters. Once we adjust for pre-existing differences through propensity score matching (and drop 11 unmatched gang leavers), none of the comparisons are significant. Neither of these sets of estimates provide evidence of an appreciable decrease in offending following gang leaving.

The effects of gang leaving for the 11 gang leavers who could not be matched to gang persisters cannot be directly estimated using propensity score matching. It is worth pointing out that these 11 unmatched gang leavers have the highest propensity scores for gang leaving. They are the least delinquent, and the least embedded in their gangs to begin with, and quickly leave their gangs after having been arrested. When all gang leavers (including these 11) are contrasted with all gang persisters in the unadjusted estimates, there is little evidence of gang leaving effects, even without adjusting for differences between the two groups.

DISCUSSION

Using data from a large longitudinal study of adjudicated delinquents, this study sought to provide evidence regarding the influence of gangs on delinquency within Thornberry et al.'s framework of selection, facilitation, and enhancement, which correspond to a "kinds of persons" explanation, a "kinds of groups" explanation, and a mixture of the two. Specifically, we sought to contribute to the knowledge base on the link between gang membership and delinquency by (1) introducing a previously unexamined source of data collected from a large sample of serious youthful offenders followed after arrest, (2) applying propensity score matching using a rich set of covariates, and (3) assessing both self-report and official offending outcomes over several outcomes waves. Specifically, we modeled the effect of both joining and leaving the gang on delinquency, with the latter being an equally important, yet underemphasized, aspect of the relationship.

Counter to previous studies concluding that evidence in favor of facilitation effects exceeded that of selection effects, our analysis provided evidence in favor of a selection effect. Although we uncover a few significant differences in analyses of gang joining and gang leaving, our most common finding is that there was no difference between gang and non-gang subjects after accounting for selection bias. In fact, there were no significant differences in official arrests between any of the identified groups even without adjusting for pre-existing differences. On balance, our findings support the selection over the facilitation or enhancement hypotheses. Gang joiners are more delinquent than gang abstainers before they join gangs, while gang persisters are more embedded in their gangs than gang leavers. Controlling for these and other differences, there appears to be very little effect of gang joining or leaving on offending.

To be sure, the divergence between this study and previous findings calls for consideration of the characteristics of the current study that may account for these differences. The biggest difference between ours and previous studies of the effects of gang joining is that we use an adjudicated sample of youth. This has a number of implications. First, our gang joining models have limited application to gang joining in general, because they are assessing an unusual sequence of juvenile delinquency: joining a gang for the first time after having been arrested. It is much more common for youths to join gangs prior to being arrested, or to join and leave gangs without ever being arrested (Huff, 1998). Second, because this is an adjudicated sample differences between gang members and non-gang members are lessened. All the comparison cases are serious juvenile delinquents as of the baseline survey. While this increases the chances of identifying comparable comparison cases, it also provides a more stringent test of the effects of gang joining and leaving. Selection bias in a population sample would likely be much larger and more difficult to fully capture with a "selection on observables" strategy. Third, while we are

interested in the "treatments" of joining and leaving a gang, these effects may be overshadowed by the "treatment" shared by all youths in the survey at the baseline interview: adjudication (Huizinga and Henry, 2008).

A second difference regards the composition of our sample. Most of the gang joiners (51%) were Hispanic males in Phoenix even though they made up just 20% of the total sample. Our gang leaving models were restricted to Phoenix males. This is a unique sample in the gang literature, with 71% of the gang leaving sample being Hispanic. This provides us an opportunity to test the effects of gang membership in an understudied but important subpopulation.

A third difference is that we identified processes unique to the gang experience that assisted considerably for understanding the gang/delinquency relationship. Our rich measures of gang embeddedness, gang organization, and gang expectations were particularly informative. Studies which have limited measures in these categories may overestimate the effects of gang joining or leaving. That is, the heterogeneity of the gang experience (in terms of treatment dosage) varies according to these categories. In fact, we found that when we removed these items from our propensity score model for gang leaving, there was evidence in two of the seven outcome waves of gang leaving resulting in lower delinquency. On the surface, this would appear to tilt the evidence in favor of an enhancement effect since characteristics associated with the gang could be attributed for the decline in delinquency for the treated (gang leaving) group. These characteristics, however, are central for identifying propensity to desist, and once that propensity was established, neither self-reported delinquency or official arrest differences remained indiscernible from zero.

Finally, our study is differentiated from much of the gang literature by its use of propensity score matching to estimate the effects of gang joining and leaving. This is not the first

study to employ propensity score matching to assess these questions (Haviland et al., 2007; DeLisi et al., 2009; Gibson et al., 2009).[7] These studies share in common the use of rich longitudinal data, which allow for selection-on-observables strategies such as propensity score matching and ordinary least squares regression. Like ours, the Haviland et al. study uncovered a significant lack of common support: their most violent trajectory group was unlike non-gang members in their sample, and so they were dropped from the analysis. While we dropped a substantial number of unmatched gang joiners and gang leavers from our analyses, we do not think this explains our results. First of all, even without employing any controls for selection bias, just comparing outcomes from gang joiners and gang leavers, we found very few significant effects. Second, in order to confirm our propensity score matching estimates, we estimated the effects of gang joining and leaving using negative binomial models. These models, which extrapolate average treatment effects using cases for which there are no comparable counterfactuals, confirmed our finding of little to no effect of gang joining or leaving. The DeLisi et al. and Gibson et al. studies did not mention support issues, but did confirm that their results remained the same when employing a logistic regression to assess treatment effects. In general, propensity score matching and regression estimates using the same control variables should not diverge provided there is common support and the regression model does not violate its major assumptions.

It should be noted that this research has several limitations. First, our measurement of gang membership depends in large part on socially-constructed definitions of the words 'gang'

[7] Both of these studies assessed adjusted balance only on those variables that were included in their propensity score models. In expectation, all variables included in propensity score models are balanced (Rosenbaum and Rubin 1983, 1985), so a more stringent test of the conditional independence assumption assesses balance on variables not included in the propensity score model. This is the strategy that we employ, and we feel it provides stronger evidence for the plausibility of the conditional independence assumption, although it also led us to create larger propensity score models.

and ‘posse.’ We found some evidence for differences in these definitions across Philadelphia and Phoenix despite lack of differences in correlates of gang membership such as peer delinquency. Since gang membership is imperfectly measured, we may be matching individuals who are both gang joiners or gang leavers, but who differed in their understanding of the meaning of the words ‘gang’ or ‘posse.’ Thus, our treatment variable may be confounded with other characteristics. We tried to minimize these problems by confining our gang leaving model to just Phoenix.

Second, gang joining is correlated with time incarcerated in both treatment and outcome waves. We found that many of the gang joiners were joining gangs while incarcerated. These differences persisted over several years, with gang joiners spending, on average, twice as much time incarcerated as non-gang joiners up to two years after the baseline interview. Our sensitivity analyses, excluding individuals who spent over 90 percent of the treatment waves incarcerated indicated that this did not affect our results. In addition, for both the gang joining and gang leaving analyses, we replicated the results using a measure of official arrests divided by street time, which assessed offending while free. Using these outcomes, we confirmed our earlier null findings, although these tests have less statistical power due to greater variance of the outcome.

With results and limitations in mind, three central points guide this discussion. First, there is great heterogeneity within the broad category of gang members. Our identification of gang process variables greatly increased the explanatory power of our gang leaving analysis and showed that embeddedness and expectations in particular are strong predictors of desistance from gangs. Some who call themselves gang members do not much care about the gang, do not expect to be part of the gang in six months, have few friends who are gang members, do not engage in crime with the gang, and belong to gangs with very little organization. Others are deeply committed to the gang, call only gang members friends, engage in many crimes as part of

the gang, and would face serious consequences should they try to leave the gang. In order to move the gang literature forward, these two individuals at opposite ends of the gang embeddedness spectrum should not be pooled into the same category; that is, gang members are not the homogenous collection of individuals that some of the literature purports them to be. Thornberry et al. (1993) recognized this and used length of gang membership as a proxy for core/fringe membership since embeddedness-type items were not included in survey instrument.

It would be preferable to measure gang embeddedness across all youths, not just those who self-identify as gang members. Former gang members are likely to retain ties to their previous gang network, especially since these individuals are undoubtedly their friends, neighbors, classmates, and family members (Decker and Lauritsen, 2002; Pyrooz et al., in press). We were unable to gauge whether our subjects retained these types of ties since our embeddedness items were asked only of gang membership—this could be contributing to the selection effects. All youth are embedded within at least one type of social network; some have characteristics that reach the threshold of a gang while others do not. It would be expected that those more deeply embedded in a network will comply more closely with the norms of the group, whether that group is pro-social (e.g., high school student government, or a sports team) or anti-social (e.g., gang, or tagging crew). We see great potential in employing the Eurogang operationalization of gang membership that uses a progressing sequence of the components of a gang (Esbensen, Taylor, and Peterson, 2009), combined with measures of gang embeddedness, as a way to place groups in the illegal behavior spectrum and to situate individuals according to their embeddedness within these groups.

Self-definition as a gang member itself could be construed as further evidence of gang embeddedness. It is an empirical question whether this would contribute to the reliability of the

gang embeddedness construct. We would hypothesize that the likelihood of self-identification as a gang member would increase with gang embeddedness and would probably pass 50% somewhere in the lower ranges of gang embeddedness. And rather than assess the impact of gang joining and gang leaving as broad categories, it would be beneficial to assess the impact of gang joining combined with a certain level of embeddedness within the gang. Likewise, the effect of gang leaving is likely conditioned by how embedded in the gang the individual is to begin with. Turning to a more continuous measure of gang membership would also allow for a move away from analyzing effects of status changes towards analyzing effects of continuous changes (e.g., changes in gang embeddedness on offending).

Second, the theoretical implications deriving from studies examining the effect of gang membership on delinquency are often ignored. Instead, scholars have used the gang context as a methodological exercise because gang membership constitutes a "natural experiment" over the course of a sizeable population of youth. Thornberry et al. (1993) introduced these theoretically informed explanations at a time when criminology was undergoing shifts in its theoretical bedrock. Gangs have provided an ideal context to examine whether sociological explanations (i.e., facilitation) are still relevant in the face criminal propensity (i.e., selection) explanations. Put simply, controlling for criminal propensity, confirmation that gang joining and gang leaving corresponds to increases and decreases in delinquency reinforces sustained criminological interest. Gottfredson and Hirschi (1990:209) attempted to explain away gang facilitation effects, holding that gangs act as a "mask and a shield," diffusing and confusing responsibility insofar that gangs provide crime conducive opportunities. Most scholars investigating the gangs/delinquency link have avoided the discussion of "opportunities" (but see Bendixen et al.,

2006; Kissner and Pyrooz, 2009), instead commenting on the absolute effect of gang membership relative to Thornberry et al.'s explanations.

As we mentioned above, despite our findings in favor of selection, the nature of our sample does not allow us to conclude in the affirmative with Gottfredson and Hirschi's (or other selection proponents, for that matter) interpretation of gangs. Alternatively, we interpret our findings to be more in line with a combined persistent heterogeneity/state dependence argument outlined by several scholars (Laub and Sampson, 2003; Nagin and Paternoster, 1991; Paternoster et al., 1997; Sampson and Laub, 1993; 1997). We do not doubt that selection plays a role in gang joining, and, admittedly, gangs are likely to place individuals on the "front lines" for offending and victimization; however, it is difficult to reconcile that gangs are important insofar as understanding the process of homophily. Laub and Sampson (1993:320) commented that while people "sort themselves out" in terms of environments, "once in place, those environments take on a history of their own in a way that invalidates a pure spuriousness or self-selection argument." In this sense, it should be unsurprising that the accumulation of disadvantages during periods of gang membership (e.g., arrests, victimizations, education) neglects to induce observable decreases in offending and arrest upon desistance. This leads to our next point.

Third, desistance from gang membership is an understudied phenomenon, yet bears import for theory and especially policy. The analyses herein are among the first to assess the effects of leaving a gang. There are at least two ways reduced time in the gang could lead to lower offending. First, if in fact gang membership is associated with elevated offending, as much of the literature has shown, then shorter spans of gang membership would be associated with less delinquency due to the immediate reduction associated with no longer being in the gang. Second, gang membership may have less consequential long-term impacts if youths are in gangs for a

shorter amount of time. Both of these points have important implications; however, as others have recognized (Klein, 1971; Krohn and Thornberry, 2008; Pyrooz et al., in press), not much attention has been devoted to studying the impact of desisting from gang membership.

We conclude by offering recommendations for future work in this area. First, heterogeneity among gang members should be recognized and its implications for gang joining and leaving should be assessed. This has implications for measurement of gang status, the effects of gang joining, and the likelihood of and effects of gang desistance. Second, the dynamics of gang joining, arrest, and incarceration need to be better understood. We found that many individuals who joined a gang for the first time after being arrested were incarcerated at the time of gang joining. We do not know if these youths were joining prison gangs or street gangs. In our gang leaving analyses, we found that one-third of gang members had left gangs just six months after being arrested, and half had left gangs after one year. This is not atypical, as gang membership has been found to be an ephemeral status (Krohn and Thornberry, 2008). We cannot assess whether official sanctioning speeds or slows this process, since all youths in our sample were arrested, but this would be an important line of research to pursue in the future. More broadly, identifying any manipulable characteristics that speed gang leaving would be valuable for policy applications. Finally, future research in gang joining and leaving should carefully deal with selection bias. We found large selection biases for both processes, to such an extent, in fact, that a good proportion of gang leavers and gang joiners could not to matched to comparable counterfactuals. Regression-based analyses estimate average effects of gang joining or leaving regardless of the presence of comparable comparison cases. Such analyses extrapolate counterfactual estimates outside of support space. Our results indicate that when selection biases are dealt with, the effects of gang joining and gang leaving are reduced.

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Table 1: Published studies examining Thornberry et al.'s (1993) explanatory hypotheses

Study	Data Source	Dependent variable(s)	Analytic technique	Supporting theme
Battin et al. (1998)	Seattle Social Development Project	Off, SR delinquency (general)	Selection on obs (manova, SEM)	Enhancement
Bendixen et al. (2006)	Bergen, Norway	SR delinquency (violent, variety)	No selection (t-tests)	Enhancement
Bjerk (2009)	Nationally Longitudinal Study of Youth	SR delinquency (property, assault, drug sales, variety)	Selection on unobs (fixed effects, IPTW)	Enhancement (property, total) Facilitation (assault, drug sales)
Bjerregaard & Lizotte (1995)	Rochester Youth Developmental Study	SR delinquency (gun carry for protection)	Selection on obs (logistic)	Enhancement
DeLisi et al. (2009)	ADD Health	SR victimization (violent)	Selection on obs (PSM)	Enhancement
Esbensen & Huizinga (1993)	Denver Youth Study	SR delinquency (street, serious, drug use)	No selection (t-tests)	Enhancement
Gatti et al. (2005)	Montreal Longitudinal Study of Boys	Off, SR delinquency (person, property, drug use)	Selection on obs (OLS?)	Facilitation (transient) Enhancement (stable)
Gibson et al. (2009)	Gang Resistance Education and Training	SR victimization (violent)	Selection on obs (PSM)	Selection/Enhancement
Gordon et al. (2004)	Pittsburgh Youth Study	SR delinquency (aggression, violent, and property)	Selection on unobs (fixed effects)	Enhancement (violent, aggression) Facilitation (property)
Hall et al. (2006)	Rochester Youth Developmental Study	SR delinquency (general, violent, drug use, drug sales)	Selection on obs (OLS)	Facilitation/Enhancement
Haviland & Nagin (2005)	Montreal Longitudinal Study of Boys	SR delinquency (violent)	Selection on obs/unobs (traj, PSM)	Facilitation/Enhancement
Haviland et al. (2007)	Montreal Longitudinal Study of Boys	SR delinquency (violent)	Selection on obs/unobs (traj, PSM)	Facilitation/Enhancement
Haviland et al. (2008)	Montreal Longitudinal Study of Boys	SR delinquency (violent)	Selection on obs/unobs (traj, PSM)	Facilitation/Enhancement
LaCourse et al. (2003)	Montreal Longitudinal Study of Boys	SR delinquency (violent)	Selection on unobs (traj)	Facilitation
Peterson et al. (2004)	Gang Resistance Education and Training	SR victimization (violent)	No selection (t-tests)	Enhancement
Taylor et al. (2007)	Gang Resistance Education and Training	SR victimization (violent)	Selection on obs (logistic)	Enhancement
Thornberry et al. (1993)	Rochester Youth Developmental Study	SR delinquency (general, violent, drug use, drug sales)	No selection (t-tests)	Facilitation (transient) Enhancement (stable)
Thornberry et al. (2003)	Rochester Youth Developmental Study	SR delinquency (general, violent, drug use, drug sales)	Selection on obs/unobs (random effects)	Facilitation
Tita & Ridgeway (2007)	Pittsburgh neighborhoods	Off neighborhood crime (911 calls)	Selection on obs (PSM, Poisson)	Enhancement
Zhang et al.(1999)	Buffalo Longitudinal Study of Young Men	SR delinquency (general, drug use	Selection on obs (logistic, OLS)	Selection (delinquency) Enhancement (drug use)

Note: PSM = propensity score matching. Obs = observables. Unobs = unobservables. Traj = group-based trajectory modeling. Off = Officially recorded delinquency. SR = Self-reported delinquency

[1]The authors referred to their analytic technique as multiple regression—we presume that OLS regression was used.

Table 2. Descriptives, unadjusted bias, and adjusted bias for gang joiners vs. gang abstainers (standard deviation in parentheses).

	Gang joiners (N=37)	Gang abstainers (N=842)	Absolute unadjusted bias	Absolute adjusted bias
age	16.2 (1.3)	16.6 (1.1)	27.8	16.2
phoenix	0.78 (0.42)	0.39 (0.49)	85.7	2.4
black	0.24 (0.43)	0.49 (0.50)	53.3	3.5
Hispanic	0.62 (0.49)	0.25 (0.43)	81.0	2.7
other race	0.0 (0)	0.05 (0.21)	30.7	.
intact household	0.16 (0.37)	0.14 (0.34)	7.5	1.4
foreign-born parents	0.41 (0.5)	0.16 (0.37)	55.0	19.7
parents' education (reverse)	4.8 (0.8)	4.2 (0.9)	68.5	1.8
parental monitoring	2.5 (0.9)	2.8 (0.9)	39.7	14.4
enrolled in high school	0.54 (0.51)	0.78 (0.42)	50.7	12.6
high school dropout	0.30 (0.46)	0.12 (0.33)	43.2	8
employed in formal job	0.24 (0.43)	0.28 (0.45)	7.8	18.6
delinquency, ever	8.8 (4.8)	6.3 (4)	56.0	6.3
delinquency, 6 months	3.6 (4.1)	2.8 (2.9)	23.7	10.7
shot at, ever	0.57 (0.5)	0.33 (0.47)	47.7	8.9
shot at, 6 months	0.3 (0.46)	0.1 (0.3)	50.2	11.1
recent victimization scale	0.49 (0.65)	0.29 (0.63)	30.8	1.9
peer delinquency	2.6 (1.1)	2.2 (0.9)	41.6	13.6
number of close friends	4.0 (4.6)	4.4 (4.4)	9.2	7.9
unstructured routine activities	3.8 (1)	3.8 (0.8)	0.3	17.5
physical neighborhood disorder	2.2 (0.6)	2.4 (0.8)	28.5	6.8
gangs in neighborhood	2.7 (0.9)	2.5 (1)	17.2	15.6
IQ	83.7 (9.5)	84.8 (13.1)	10.0	2.9
social capital – connectedness	2.3 (0.5)	2.5 (0.5)	39.1	31.3

Table 3. Effects of Gang Joining (on joiners)

	Impact of First-Time Gang Joining after baseline interview					
Dependent Variable	T = 6 months *b* (s.e.)	T = 12 months *b* (s.e.)	T = 18 months *b* (s.e.)	T = 2 years *b* (s.e.)	T = 3 years *b* (s.e.)	T = 4 years *b* (s.e.)
Delinquency Variety						
Unadjusted estimate	.90 (.36)*	.76 (.37)*	.15 (.34)	.60 (.38)	.61 (.39)	1.66 (.42)**
Matching estimate	–.04 (.52)	–.77 (.56)	–.93 (.37)*	.12 (.66)	.12 (.61)	.73 (.78)
Official Arrests						
Unadjusted estimate	–.03 (.10)	–.07 (.10)		.05 (.12)		
Matching estimate	.02 (.13)	–.22 (.13)		–.01 (.14)		

*$p<.05$

Table 4. Descriptives, unadjusted bias, and adjusted bias for gang leavers vs. gang persisters (standard deviation in parentheses).

	Gang leavers (N=42)	Gang persisters (N=121)	Absolute unadjusted bias	Absolute adjusted bias
age	16.5 (1.0)	16.6 (1.0)	3.5	5.5
black	0.07 (0.26)	0.17 (0.38)	31.3	7.0
Hispanic	0.69 (0.47)	0.72 (0.45)	6.2	6.6
other race	0.07 (0.26)	0.07 (0.25)	2.1	18.8
gang embeddedness	-0.05 (0.78)	0.46 (0.60)	73.2	5.7
gang organization	3.0 (1.6)	3.5 (1.4)	30.5	8.5
expect to return to gang	0.30 (0.46)	0.66 (0.48)	80.5	6.3
intact household	0.12 (0.33)	0.13 (0.34)	3.9	4.6
foreign-born parents	0.33 (0.48)	0.40 (0.49)	13.1	20.3
parents' education (reverse)	4.6 (1.1)	4.7 (1.0)	8.9	31.0
parental monitoring	2.9 (0.7)	2.5 (0.9)	50.3	17.0
enrolled in high school	0.43 (0.50)	0.60 (0.50)	26.7	11.4
high school dropout	0.33 (0.48)	0.29 (0.46)	9.5	2.5
employed in formal job	0.38 (0.49)	0.24 (0.43)	30.6	7.1
delinquency, ever	10.9 (4.8)	12.5 (4.2)	35.7	13.7
delinquency, 6 months	5.5 (4.7)	6.2 (4.3)	15.9	0.2
shot at, ever	0.69 (0.47)	0.73 (0.40)	8.0	2.2
shot at, 6 months	0.26 (0.45)	0.31 (0.46)	9.7	10.1
recent victimization scale	0.62 (0.91)	0.76 (1.01)	14.7	10.9
peer delinquency	2.9 (1.0)	3.1 (0.9)	21.0	3.8
number of close friends	3.6 (3.4)	4.5 (4.5)	23.5	11.9
unstructured routine activities	4.0 (0.7)	4.1 (0.7)	12.5	1.2
physical neighborhood disorder	2.3 (0.8)	2.4 (0.7)	11.2	13.2
gangs in neighborhood	2.8 (1.1)	2.9 (1.0)	6.5	6.1
IQ	84.8 (13.8)	85.2 (12.1)	3.6	4.2
social capital – connectedness	2.4 (0.4)	2.3 (0.5)	15.7	10.3

Table 5. Effects of Gang Leaving (on leavers)

	Impact of leaving gang within 6 months after baseline interview						
Dependent Variable	T = 6 months *b* (s.e.)	T = 12 months *b* (s.e.)	T = 18 months *b* (s.e.)	T = 2 years *b* (s.e.)	T = 2.5 years *b* (s.e.)	T = 3.5 years *b* (s.e.)	T = 4.5 years *b* (s.e.)
Delinquency Variety							
Unadjusted estimate	–.04 (.71)	–1.10 (.59)	–.92 (.68)	–.88 (.52)	–1.08 (.50)*	–.61 (.56)	–.26 (.53)
Matching estimate	.80 (.98)	–.95 (.70)	–.24 (.92)	–1.12 (.59)	–.75 (.57)	–.31 (.74)	.46 (.73)
Official Arrests							
Unadjusted estimate	.05 (.11)	.05 (.12)	.19 (.15)		–.05 (.13)		
Matching estimate	.17 (.18)	.11 (.18)	.25 (.23)		–.21 (.19)		

*$p<.05$

Appendix A: Results of logistic regression model obtaining propensities scores for gang joining (N=841)

	Coefficient	Standard Error	z	
Age	-7.76	5.28	-1.47	
Age^2	0.22	0.16	1.34	
Black	0.54	0.80	0.67	
Hispanic	0.64	0.62	1.02	
Phoenix	2.02	0.70	2.91	*
Household: biological mother only	0.50	0.74	0.68	
Household: biological mother, step father	-0.13	0.82	-0.16	
Household: biological father, no biological mother	-1.41	1.23	-1.15	
Household: other situation	1.11	0.69	1.60	
Ever delinquency variety score	0.26	0.08	3.41	*
Recent delinquency variety score	-0.28	0.09	-2.92	*
Parental monitoring	-0.44	0.26	-1.66	
Missing parental monitoring	0.39	0.67	0.57	
Parents' education	0.50	0.25	1.97	*
Missing parents' education	0.82	1.07	0.76	
Ever victimization variety	-0.08	0.26	-0.31	
Recently beaten up	0.97	0.71	1.37	
Recently attacked	-2.41	1.12	-2.16	*
Recently shot (missed)	1.78	0.63	2.82	*
Peer delinquency	0.37	0.28	1.29	
Missing peer delinquency	0.13	1.19	0.11	
Enrolled in school	-1.05	0.48	-2.20	*
On-track in school relative to peers	0.19	0.19	0.99	
IQ	-0.03	0.02	-1.60	
Hours employed per week	0.02	0.02	1.40	
Temperance scale	0.02	0.30	0.05	
Gang presence in neighborhood	1.23	0.41	3.02	*
Physical neighborhood disorder	-1.72	0.59	-2.92	*
Warmth of relationship with father	0.43	0.31	1.41	
Missing warm of relationship with father	-0.45	0.54	-0.84	
Constant	62.67	43.12	1.45	
χ^2		108.68*		
Pseudo R^2		.36		

Note: *$p < .05$

Appendix B: Results of logistic regression model obtaining propensities scores for gang leaving (N=163)

	Coefficient	Standard Error	z	
Gang embeddedness	-2.87	1.12	-2.57	*
Gang organization	-0.24	0.19	-1.29	
Embeddedness X organization	0.64	0.28	2.30	*
Future gang expectation	-1.98	0.57	-3.46	*
Black	-2.10	1.22	-1.72	
Hispanic	-1.10	0.92	-1.19	
Other	-1.10	1.27	-0.86	
Age	0.24	0.29	0.85	
Parental monitoring	0.60	0.34	1.78	
Missing parental monitoring	-0.09	0.83	-0.11	
Household: biological mother only	0.61	0.85	0.72	
Household: biological mother, step father	0.46	0.89	0.51	
Household: biological father, no biological mother	0.95	1.10	0.86	
Household: other situation	-0.44	0.98	-0.45	
Peer delinquency	-0.17	0.33	-0.50	
Unstructured routine activities	0.14	0.38	0.36	
Social capital	1.18	0.61	1.92	
Consideration of others	0.52	0.33	1.56	
Temperance scale	-0.63	0.40	-1.58	
High school dropout	-0.28	0.56	-0.50	
Hours employed per week	0.03	0.02	1.71	
Neighborhood disorganization	0.10	0.38	0.25	
Victimization frequency	0.02	0.04	0.48	
Recent delinquency variety	-0.01	0.06	-0.16	
Years in gang	-0.26	0.14	-1.89	
Highest grade completed	-0.40	0.25	-1.62	
Constant	-3.31	5.08	-0.65	
χ^2		62.18*		
Pseudo R^2		.33		

Note: *$p < .05$