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The Effect of Prison Gang Membership on Recidivism

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Purpose: How does prison gang membership affect recidivism? In this paper, we use a unique dataset of all releasees from prisons operated by the Illinois Department of Corrections during the month of November 2000, which includes demographic information and data on gang participation. We attempt to control for confounding factors that are traditionally associated with both prison gang membership and rearrest.

Methods: We develop a potential-outcomes framework and describe the conditions under which a counterfactual can be estimated when gang membership is not randomly assigned. We combine regression analysis with Coarsened Exact Matching, which has several advantages over the more popular propensity score matching, to estimate the effect of gang membership on recidivism.

Results: Prison gang membership results in a six percentage point increase in recidivism.

Conclusions: Despite the strengths of the data, unobserved heterogeneity among inmates could still bias estimates. However, there are probably important subtleties to the gang participation decision such that experimental or quasi-experimental data are unlikely to increase our understanding of the relationship between gang-membership and post-release outcomes. We recommend incorporating ethnography with survey data collection, because ethnographers are able to document otherwise unobservable contextual information concerning the selection process which could be used to identify causal relationships.

Keywords: prison gang, recidivism, causality, Coarsened Exact Matching.

Introduction

　　How does prison gang membership affect recidivism? There are many reasons to assume that it increases the potential for reoffending relative to releasees who lack a connection to a criminal organization. Although there has been an increasing amount of research dedicated to examining the correlates of recidivism, relatively little has been devoted to the role of prison gang membership specifically. There are two primary obstacles to answering this question. First, the lack of data is a practical problem. Second, there is an important methodological challenge about how to effectively control for potential confounders in establishing a causal relationship between gang membership and post-release outcome.

　　Gang members likely have many attributes, both observed and unobserved, which are correlated with gang membership and also predict persistent criminal behavior. The researcher has to account for selection bias because other factors influence both gang membership and recidivism. If these covariates are not controlled for, then they will bias our measure of the affect of prison gangs on recidivism. In theory, an experimental design that randomly assigned offenders into treatment (prison gang affiliated) and control groups (non-gang affiliated) would eliminate the confounding influence. A practical alternative is to use a multivariate regression framework with observational data and control for potentially confounding criminogenic attributes. However, causal interpretation of regression coefficients requires strict, and often unrealistic, assumptions concerning the relationship between unobservables and the gang participation decision. Standard regression analysis focuses attention on the estimated outcomes rather than on the selection mechanism, which is often of more interest to the researcher. Additionally, regression is a purely parametric approach and requires assumptions concerning the distributions of the covariates which may not reflect actual sample properties. For example, randomized trials, in an ideal setting, match all other relevant characteristics of sample units and

then randomly apply a "treatment" to some portion of the sample. Hence, a controlled experiment would balance the covariate distributions across treatment and control units, allowing the researcher to isolate the independent affect of prison gang membership. A regression analysis, on the other hand, will often have covariate values outside the area of common support thus creating estimates which depend more on the specific assumptions associated with the estimator than the characteristics of the data.

In this paper, we examine 2,534 releasees from Illinois prisons in November of 2000 who were tracked for a period of two years after release. These data address the practical limitation of finding records on prison gang membership collected by prison authorities who are reluctant to dispense information on "security threat groups." Using a potential outcomes framework, we first address the underlying assumptions necessary to identify the average treatment effect of prison gang membership on recidivism (see Imbens and Wooldridge, 2009).[1] This is an important step because it allows us to think more clearly about the selection mechanism of gang membership without placing the problem in the context of a specific statistical model. In addition, it also points to what types of information future qualitative criminological research, particularly ethnography, could provide to guide model selection. We apply Coarsened Exact Matching (Iacus, King, & Porro 2011), a matching algorithm which has several statistical advantages over other popular matching techniques such as propensity score matching, to the sample of post releases. We find that prison gang membership results in a six percentage point increase in recidivism after balancing the sample on a key set of determinants for prison gang membership.

Review of existing literature

The reintegration question has drawn a great deal of attention from criminology and criminal justice (Petersilia 2003). Each year more than 675,000 prisoners are released (West et al., 2010). Prison beds cost state governments an average of $22,500 annually (Stephan, 2004). Finding ways of reducing the expenditures associated with recidivism would be welcome fiscal news (Baumer, 1997). The rebirth of the rehabilitative correctional philosophy offers insights into how this might be accomplished. Aiming services at high-risk offenders is one solution that has demonstrated promise (Gendreau et al., 1996). This leads to the issue that guides nearly all of criminal justice, classifying and managing risk.

The first step in the process involves locating that group of offenders who face an elevated risk of reoffending. Identifying a characteristic that captures a combination of risk factors would improve the effectiveness of classification and risk management. Alternatively, if a unique identifier, one uncorrelated with others, could be located this could help correctional authorities and service providers focus their efforts more effectively by reducing the potential for fruitless supervision and services. In the first scenario, this would involve directing resources at programs that address broad issues such as improving employment prospects or reducing substance abuse problems. The second involves the more focused project of finding ways to attenuate the causal factor itself in the hopes of reducing recidivism.

Despite the outward appearance that gang affiliation is the source of the reoffending problem, it might be that other factors determine both gang membership and reoffending. This poses a problem for how to allocate resources. For example, offenders may have more opportunities to reoffend once they join a prison gang. Street gang members often maintain ties to their organizations or create new alliances while serving prison sentences. Past work suggests

that their commitment to the gang identity is likely to gain strength while incarcerated (Moore, 1978) which might have a bearing on behavior post-release (Moore, 1996). Another complicating factor is in differentiating those inmates with street gang connections (Varano et al., 2011) from those who only have a prison gang affiliation. Separating these complicated and intertwined issues will be instrumental in terms of generating more effective post-release supervision and service strategies.

Criminology has typically approached the issue of gang membership and recidivism in two parallel literatures that both indirectly address the topic: recidivism in general and the factors that influence the choice to join gangs. We merge the two literatures through a comparison of inmates who the Illinois Department of Corrections (IDOC) identified as prison gang members and non-members. A reading of the two literatures suggests that the independent effect of gang membership increases reoffending post-release. We will motivate this hypothesis in two stages and follow it with an empirical foundation in the remainder of the work. First, we survey the research that examines factors affecting the risk of ex-offender recidivism in general. This literature gives an account of the characteristics that are correlated with post-release success and failure.

The second related literature provides theoretical explanations about why gang identified inmates are at an increased risk of reoffending. Here we offer possible explanations for why gang members are less likely to stop offending. Canvassing the street gang and prison gang literature lends credibility to the argument that there are numerous reasons to suspect that gang membership, in and of itself, will draw offenders back into the criminal justice system.

Recidivism

There have been several major efforts to determine what factors influence recidivism on both the national level and within the state of Illinois. Regarding the former, there have been two studies of recidivism drawing on large, nationally representative samples. The first measured recidivism as rearrest and reconviction with return to prison, the two most common measures found in the literature. Within three years, 63 percent of releasees were rearrested for a felony or serious misdemeanor and 41 percent were recommitted to prison during the same period (Beck and Shipley, 1989). Survival analysis revealed that achieving success during the first year of release is critical, as two-thirds of the rearrests occurred within the first year. Younger, minority, male, high school dropouts, who committed property offenses, and those who had more extensive arrest histories were more prone to recidivism (ibid).

Subsequent research, also conducted by the Bureau of Justice Statistics (Langan and Levin, 2002), using the same definition and measure of recidivism found an increase in the overall level of recidivism. The rearrest rate and reincarceration rates increased to 68 percent and 63 percent. The former is relatively slight in comparison to the marked boost in the recommitment rate, suggesting the correctional system had become more risk averse. The analysis reaffirmed a few of the findings of the earlier study in terms of the correlates evident in the recidivism patterns. Rearrest rates were significantly curtailed following the first year of release and the association between age, gender, race, conviction for a property offense and recidivism was consistent with the earlier study.

These same general patterns were found in a series of studies conducted in Illinois in the 1980s. Sixty percent of releasees followed for 27 to 29 months were rearrested (Illinois Criminal Justice Information Authority—ICJIA, 1986b) and 32 percent reincarcerated (ICJIA, 1985). The first nine months of release was dubbed the "critical period" for rearrests (ICJIA, 1986a). After

this period, the rearrest rate declines. Through survival analysis, the researchers predicted that 63 percent of the sample was ultimately expected to be rearrested (ibid). Survival analysis also highlighted the effects of several factors. Younger releases, those convicted of a property offense, and those housed in maximum-security institutions experienced rearrest at a quicker pace than older, violent, and minimum security releasees. Age, race, marital status, prior incarceration, and prior arrests were all associated with rearrest. The report concludes that the factors that proved best in predicting rearrest were the number of prior arrests, prior state prison incarcerations and the holding offense classification (ICJIA, 1986b).

How are these elements related to prison gang status? The same factors that are responsible for enhancing the chances of post-release rearrest or recommitment appear in greater proportions of gang members than non-affiliated inmates. Analysis of a cohort of prisoners confined in Nebraska found that gang members were younger, less likely to have a high school diploma, and less likely to be married or have children than their non-gang counterparts (Kreinert and Fleisher, 2001). Furthermore gang members had a more criminally involved past. For instance, gang members reported an earlier age of arrest, less education, less commitment to legal employment, more drug use and more prior arrests than non-gang members. Gang members also have a similar number of prior convictions when compared with non-gang members despite their being younger overall (ibid). Combining these results with the well substantiated findings about recidivism leads to the conclusion that gang members are likely to encounter rates of failure higher than those of their non-affiliated counterparts. A review of the gang literature offers rationales for why gang membership, in and of itself, can produce these effects.

Gang Affiliation

Correctional officials often distinguish prison gangs from street gangs by the group's location of origin. For example, the California Department of Corrections and Rehabilitation (CDCR) define prison gangs as "any gang which originated and has its roots within the CDCR or any other custodial system" (California Department of Corrections and Rehabilitation, 2011: 381). As a matter of practice, prison gangs generally also require a lifetime membership, giving rise to the popular idea that the gang bond is "blood in, blood out."[2] One study finds that an average of only five percent of gang members drop out (Fong, Vogel, & Buentello, 1993). Prison gang members are typically more selective, secretive, criminally sophisticated, older, and violent than street gang members (Pyrooz et al., 2011: 4).[3]

Location of origin provides a useful descriptive label, but both street and prison gangs often participate in similar criminal activities. Members of prison gangs and street gangs participate in illicit markets in correctional facilities, extort inmates, and disrupt operations (e.g. Jacobs, 1977; Camp & Camp, 1985). Many prison gangs, such as the *Nuestra Familia* and Mexican Mafia have expanded their operations outside of correctional facilities. Paroled prison gang members and associates conduct criminal activities on behalf of the prison gang organization. For powerful prison gangs like the Mexican Mafia, street gangs actually pay tribute to the gang in exchange for protection in jail and prison and the exclusive right to sell drugs in certain neighborhoods (Skarbek 2011).

Membership in a prison gang is likely to increase recidivism in three ways: signaling a commitment to a criminal lifestyle, altering social and human capital, and invoking an institutional response. First, prison gang membership can provide a signal about the given and unobservable characteristics of a particular inmate. Because prison gangs require lifetime commitment, only those inmates who are most dedicated to a criminal lifestyle will agree to join.

Similarly, personal and journalistic accounts suggest that those inmates who perceive prison gang members to have the highest status in the criminal world will be most likely to seek membership (Blatchford, 2008). These two mechanisms connect prison gang membership with a higher likelihood of recidivism via a correlation with an unobserved criminal identity.[4] These mechanisms would suggest that prison gang membership reveals underlying propensities to offend rather than making a person more likely to offend.

A second way to explain the relationship between prison gang membership and reoffending is that, holding agent type constant, participating in a prison gang makes one more likely to recidivate. Several possible mechanisms exist. First, spending time with people who have longer criminal histories and offenses that are more serious increases one's odds of recidivating (Chen and Shaprio, 2007; Bayer, Hjalmarsson, & Posen, 2009). Interaction with prison gang members leads to an increase in one's knowledge of how to conduct criminal activity (Moore, 1996). A second mechanism that makes prison gangs membership lead to more recidivism is to expand or alter the inmate's social network. Participating in a prison gang leads to new criminal opportunities once released because the inmate has new associates and is part of a more criminal social network. Alternatively, a prison gang member will be perceived to be more trustworthy because of his affiliation, and this trust facilitates cooperation in criminal activities (Gambetta, 2009). A related mechanism that increases the risk of recidivism is the use of prominent tattoos among prison gang members. These improve the operations of prison gangs by making it more difficult to participate in alternative, legitimate activities and act as a costly signal of the member's dedication (Iannaconne 1992). This further diminishes the already limited probability of securing legal employment post release (Pager, Western, & Sugie, 2009), which is

an important hallmark of desistence. As a result, prison gang members engage in relatively more criminal activity and are therefore more likely to recidivate.

A third way that prison gang membership leads to greater recidivism is by the response from the legal community. Law enforcement officials may pay greater attention to inmates identified as gang members once they are released. Law enforcement officials will be more likely to observe gang member's post-release crimes, so prison gang members will be more likely to be rearrested or recommitted. The labeling perspective has explained how identities imposed by criminal justice authorities can produce negative outcomes, especially for those who are branded as being part of criminal organizations (Braithwaite, 1989; & Maruna, 2001). These external agents also include rival gang members who fail to realize that a given member has renounced his gang ties (Vigil, 2002). More recent theoretical innovations offered through developmental criminology have pointed to path dependence as being responsible for generating impediments to success as well (Piquero, Farrington, & Blumstein, 2007).

The best developed body of research on the gang-membership recidivism connection has been generated from samples of formerly institutionalized juveniles. The results are split between those finding null-effects (Lattimore et al., 1995; Brownfield, Sorenson, & Thompson, 2001) and those that find statistically significant variation between gang and non-gang members. The latter studies resulted from analyses of the records of 248 adolescents released from the Arkansas Division of Youth Services. Applying logistic regression techniques to the data and controlling for a variety of other factors reveal that gang members earned odds ratios slightly above 2 for recommitment one year following release (Benda & Tollett 1999; Tollett & Benda, 1999) and were a little over four and a half times more likely to be rearrested as adults than those

not involved in a gang (Benda, Corwyn, & Toombs, 2001). It is unclear, however, if gang membership denoted street or institutionally oriented organizations.

Two related literatures also examine the how gang membership affects offending while incarcerated and the relationship between in-custody offending and recidivism. It may be that prison gang affiliation increases institutional misconduct and this, in turn, increases post-release misbehavior. An established body of work has argued that joining a prison gang increases prison misconduct (Ralph & Marquart 1991; Fong et al. 1992; DeLisi et al. 2004; Berk et al. 2006). This finding holds for inmates being chronically non-compliant and disruptive (Morris et al. 2012), assaults on correctional staff (Sorensen et al. 2011); assault other inmates (Huebner 2003). This is also found among juvenile gang identified detainees engaging in institutionally dangerous conduct (Trulson, 2007). Prison gang affiliation is also associated with elevated rates of in-prison violence even when controlling for a number of measures associated with overall predisposition toward violence (Gaes et al. 2002; Griffin & Hepburn 2006).

The second body of work identifies a relationship between misbehavior while incarcerated and recidivism. Trulson et al. (2005) follow-up on more than 2,000 adjudicated delinquents released from the California Youth Authority (CYA) and find that those releases with more infractions while detained had significantly higher odds of rearrest for a felony offense. Another study of CYA releasees also finds a connection between gang-related activity, institutional violence, and overall rearrest postrelease (Lattimore et al. 2004; see also, Trulson et al. 2011). Lastly, Huebner and co-authors (2007) find that the timing of postrelease reconviction is also correlated with institutional misconduct; those with higher scores are reconvicted sooner.

Prior analysis of the dataset used in this study found that prison gang members do differ from non-gang members in the number of criminogenic traits they carry. When controlling for

age, race, education, and criminal history the effect of gang membership is reduced to insignificance in a logistic model (Olson, Dooley, & Kane, 2004).

Data, sample, and measures

The sample is comprised of all releasees from prisons operated by the Illinois Department of Corrections (IDOC) during the month of November 2000. (Further detail on the methodology can be found in Olson et al., 2004 and Olson & Dooley, 2006.) The IDOC research and analysis unit submitted all documentation relevant to each inmate to the research team. These data are enumerated below (Table 1). The data contains many of the key demographic and correctional items contained in the analysis: *Age* (in years), *sex* (0=female, 1=male), *race* (0=white, 1=black, 2=Hispanic), *marital status* (0=common law/married, 1=separated/divorced, 2=single/widowed, 3=missing), *number of children*, and *educational attainment* (0=high school diploma/GED, 1=no high school diploma/GED, 2=missing). These data were collected at reception and classification as the inmates were processed into the correctional system. The IDOC data also includes justice system data including the *commitment offense type* (0=missing, 1=person, 2=property, 3=drug, 4=sex, 5=DUI, 7=other), *time served* (years), and the *county type* (0=rural, 1=urban, 2=Cook i.e. Chicago) to which they were being released. Additionally, these data include a measure of *prison gang affiliation* (0=no, 1=yes) that was assigned by gang intelligence units operating in the correctional system. Twenty-four percent of the sample was determined to be prison gang affiliated via an objective assessment of intelligence gathered by specially trained officers within the facilities. Those scoring high enough in terms of the point system established by the IDOC were flagged as being gang affiliated. These items could include, but were not limited to, the following pieces of evidence: self-admission, gang tattoos, keeping gang paraphernalia, and

developing or maintaining ties to known prison gang members. This finding is consistent with published national estimates of prison gang prevalence (Knox, 2000) of 25 percent (among males) and Illinois estimates of 26 (Corrections Compendium, 2000). A unique identifier, the state ID (SID) number, allows us to link these dependent variables with our outcome measures.

Approximately two years after the cohort was released, recidivism information was collected. The Illinois State Police records were matched on the SID allowing for arrest records to be merged with the IDOC master file. On January 6, 2003 arrest histories giving both pre-incarceration arrest (*number of priors*) and *rearrest* were added. A few days prior, December 31, 2002 a search of the Offender Tracking System maintained by the IDOC indicated which of the November 2000 releasee cohort had been readmitted to prison (*return*). These returns resulted from either a technical violation of the terms of mandatory supervised release and/or rearrest for a new offense. The available evidence on recidivism finds that most of the failure that occurs is likely to occur well within the roughly 750 day follow-up period used here. For the overall sample two-thirds (66 percent) were rearrested and just under half (48 percent) reincarcerated post release. These numbers are consistent with past research on recidivism

Due to there being little, if any, reason to suspect seasonality of releasee type, the sample can be safely assumed to be representative, granting the work a reasonable claim to generalizability. There are two additional reasons why this data and its being set within Illinois are ideal conditions for examining the prison gang/recidivism relationship writ large. First, the state prison system is the seventh largest in the country (West et al., 2010). If the prison gang statistic is applied to the approximate population of the state prison system (42,000) this yields an estimate of slightly more than 10,000 inmates who are currently prison gang affiliated. This provides the researcher with a sufficiently large sample on which to conduct relatively

sophisticated statistical analyses. Secondly, the state has one of the most entrenched prison gang populations of any state penal system (Jacobs, 1977; Camp & Camp, 1985).

Methods

A large literature explores the issues that arise in establishing causal relationships with observational data (Imbens & Wooldridge, 2009). Regression analysis is typically performed in this context; however, in recent years researchers have increasingly begun to use matching algorithms to uncover causal relationships (Imbens & Wooldridge, 2009).

Matching is most commonly used in the program evaluation literature in which the researcher is concerned with estimating a "treatment effect." Estimation of program effects on outcomes is made difficult by the fact that program participants are not always selected at random, so unobserved variables can bias estimates of program effects. When evaluating a job-training program's effect on wages, for example, one must be concerned that unobservable variables, such as "work ethic," are correlated with participating in the program and wages. If "work ethic" positively affects wages and people with greater "work ethic" are more likely to participate in job training then the effect of job training on wages will be overstated in a standard regression analysis. Because individuals self-select (at least to some degree) into prison gangs, standard regression analysis will also generate biased estimates of the effect of prison-gang membership on recidivism.

The general idea of matching algorithms is to mitigate the effect of confounding, pre-treatment factors on estimates of treatment effects by balancing the covariate distributions of treatment and control groups. For example, exact matching—matching at least one treatment unit with one or more control units with the same covariate values—would perfectly balance the

data and the treatment effect on the treated can be calculated non-parametrically with a simple difference in means. However, when the data do not permit exact matching, such as when continuous variables are present, the researcher must resort to approximate matching techniques, such as propensity score matching, that are often cumbersome to implement (Iacus, King, & Porro, 2011). Additionally, the approximate matching techniques from the literature are designed to improve (and only report on) the mean imbalance between treatment and control groups, without addressing imbalance in higher-order moments. In this paper, we suggest that Coarsened Exact Matching (CEM), an easy to use and understand matching methodology developed by Iacus, King, and Porro (2011), can productively contribute to the criminology literature.

Identifying Assumptions for Estimating the Counter-Factual of Interest

We are interested in the impact of prison-gang membership (the treatment) on recidivism and re-arrest. Assume there are two potential outcomes for any post-release offender.[5] Let $T_i = 1$ for the i_{th} ex-convict who enters a prison gang and $T_i = 0$ for the person who does not join a gang. Let $Y_i(1)$ represent the outcome (i.e. recidivism or rearrest) when $T_i = 1$ and $Y_i(0)$ represent the outcome when $T_i = 0$. The true treatment effect for any individual is the difference between $Y_i(1)$ and $Y_i(0)$. However, we cannot observe an ex-convict who is both a gang and non-gang member simultaneously. What is actually observed in the data is $Y_i = Y_i(T_i) = Y_i(1) T_i + (1-T_i)Y_i(0)$. As a result, we must rely on assumptions concerning the counterfactual outcome—$Y_i(0)$ with treatment. The problem of establishing a credible counterfactual is a missing data problem. Our objective is to estimate (some parameter of) the unobserved joint distribution $F(Y(1),Y(0)|T=1)$. The most common parameter of interest is the mean effect of treatment on the treated: $ATT = E(Y(1)-Y(0)|T=1)$.

Matching algorithms typically assume covariates, X, can be drawn from the population to correct problems of pre-treatment differences across treatment and control groups. Two more precise assumptions of matching follow: 1) unconfoundedness—after conditioning on a proper set of covariates there is no unobserved variable that affects both the potential outcome and the treatment assignment probabilities—formally defined as $T \coprod (Y(1), Y(0))|X$, and 2) covariate overlap—for all values of the pre-treatment covariates there are treatment and control observations (Imbens & Wooldridge, 2009).[6] Assumption (2) implies that $0 < \Pr(T = 1|X) < 1$. In other words, the researcher's objective is to create a sample of treatment and control units matched on observables in which any statistically significant difference in observed outcomes is attributable to treatment.

Assumption (1) is the most controversial and difficult to establish (Imbens & Wooldridge, 2009). When the data are not generated by a controlled experiment, controlling for pre-treatment covariates with random assignment of treatment satisfies the unconfoundedness assumption (see Angrist and Pischke 2009). However, given the non-random assignment of treatment in most cases in the literature, the reliance on assumption (1) is precarious and matching on observables is not a perfect substitute for an experimental design which controls for the effects of unobservables as they are related to treatment (see Angrist & Pischke, 2009). Nonetheless, unconfoundedness can be justified on theoretical grounds (Imbens, 2004; Heckman, LaLonde, & Smith, 2000). Unconfoundedness may not be violated if individuals select treatment for unobserved reasons independent of the researcher's outcome of interest (Heckman, LaLonde, & Smith, 2000). If the prisoner's expectation of post-release outcome(s) is independent of the decision to join a prison gang, then the selection-on-observables assumption is justified. For example, a prisoner may sort into a prison gang for safety concerns, which may be generated

randomly, while in prison. Such a situation could arise, if for example, prisoners with the same values for pre-treatment covariates are randomly assigned sets of strategies (e.g., because of initial contacts with other prisoners upon arrival) for dealing with prisoner-on-prisoner violence. Consequently, two prisoners could have the exact same pre-treatment covariates and objective (i.e. to improve their safety during a prison commitment) with one selecting into a prison gang for unobserved reasons (i.e. it was the best available survival strategy) and unconfoundedness not be violated. However, if those prisoners who choose prison gang membership base their choice on a previous decision to become a hardened criminal, then unconfoundedness is violated and the estimate of prison-gang membership on post-release outcomes will be biased. Additionally, if one or more of the variables used to match treatment and control units is *caused* by selection into prison gangs then not only will unconfoundedness fail but the estimation bias can actually increase (Heckman, LaLonde, & Smith , 2000). We avoid this last problem by exploiting the timing of the treatment with pre-treatment characteristics and then control for post-treatment observables in regression models. It is also important to point out that matching algorithms can also reduce bias by decreasing dependence on functional form assumptions because observations are matched non-parametrically (Iacus, King, & Porro).

Coarsened Exact Matching

We introduce Coarsened Exact Matching (CEM) to the criminology literature.[7] CEM provides a potentially superior alternative to the more often used propensity score matching (PSM) because the latter only addresses mean imbalances (Iaucs, King, & Porro, 2011). This can be a major problem if the data on covariates are drawn from distributions which are not well behaved across treatment and control units, in which case minimizing only mean-imbalance could be problematic (Iaucs, King, & Porro, 2011). Another common problem with propensity

score matching is that to improve the balance between treatment and control groups on one covariate, balance often decreases in others (Iacus, King, & Porro).

This last issue is a result of a methodological difference between CEM and PSM: with CEM the level of covariate balance is pre-determined by setting "tuning parameters" for each covariate which subsequently determines the sample size, whereas PSM sets the sample size ex ante by specifying the number of control units per treatment unit and balance is determined ex post (Iaucs, King, & Porro, 2011). In CEM, the "tuning parameters" bound the imbalance on any single covariate but do not affect the imbalance on any of the other covariates (Iaucs, King, & Porro, 2011). Because CEM limits each covariate by only one tuning parameter, balance in means and higher moments, as well as the full multivariate distribution can be improved (Iaucs, King, & Porro, 2011). In the case of CEM, the tuning parameters are used to determine the level of coarsening of the data. For example, one can think of "tuning parameters" as setting the tolerance level for grouping members by age. If the researcher is studying small children, the tolerance (age range) would not be as high as if the entire adult population was the object of study.

The general idea of CEM is to temporarily coarsen the covariates into bins, which can be defined by the user or generated automatically via the binning algorithms developed by (Blackwell, Iacus, King, & Porro, 2009). Exact matching is used to group treatment and control units. The covariates are then returned to their original values and any group without at least one treatment and control unit is dropped. As a result, both treatment and control units are pruned from the sample. Trimming treatment and control units also reduces model dependence in estimates of the treatment effect by not "forcing" unreasonable matches for treatment units (Iacus, King, & Porro, forthcoming). Additionally, CEM has a variety of other desirable

statistical properties. For example, CEM has also been shown to bound the estimation error on the average treatment effect and is computationally efficient (Iacus, King, & Porro).

Results

In Table 1, the variable definitions, means (standard deviations), and logit estimates for re-arrest and recidivism. According to the logit models, prison-gang membership increases the probability of both re-arrest and recidivism by about five-percentage points, but it is barely statistically significant at conventional levels.[8]

Table 2 shows the multivariate and univariate imbalance between the control and treatment group with a statistic developed by Iaucs, King, & Porro (2011). See the technical appendix for an explanation of how this statistic is formed. The multivariate L_1 statistic reported at the top of Table 2 indicates approximately 38 percent of the density for the treatment and control groups overlap. Given the relatively low level of overlap, the estimates from Table 1 of *gang* are heavily model dependent. An important point about the multivariate L_1 statistic is that it is of use primarily for comparing across different models, just as R-square is used in regular regression analysis (Iacus, King, & Porro, forthcoming). Table 2 also reports univariate balance for the joint distribution of each covariate, as well as for the means and quantiles of the univariate distributions. We can see for example that the variable *priors* has better balance in the full joint distribution than in the means and also is not well balanced in the quantiles. Table 3 shows the imbalance after applying CEM to the data set.[9] Multivariate balance improved substantially as 58 percent of the density for the treatment and control group overlap in the matched sample. Also, univariate balance improved for each covariate. Mean imbalance actually increased for *priors*, however, balance in the quantiles improved significantly.

Table 4 shows the results from logit models for the estimate of prison-gang membership on recidivism and re-arrest. For the matched samples, the regressions are weighted with importance weights generated from the CEM procedure (Blackwell, Iacus, King, & Porro, 2009).[10] Without adjusting for covariates, the positive effect of *gang* on recidivism and re-arrest for the unmatched sample is quite large—approximately 13 and 10 percentage points, respectively—and statistically significant. However, after balancing, the effect diminishes to approximately 7 and 6 percentage points, respectively. We also adjust the estimates by including the post-treatment covariates left out of the first-stage CEM procedure. The effects are similar to those found for *gang* in Table 1, approximately 6 percentage point increase for both recidivism and re-arrest; however, the coefficients are more precisely estimated.

We also estimated the same models using propensity score matching along with regression analysis[11]. Using propensity score matching we found that the Average Treatment Effect (ATE) of gang membership on recidivism was 0.09 (nine percentage point increase in the probability of recidivism) and the (ATE) of gang membership on re-arrest was 0.10 (ten percentage point increase in the probability of recidivism) and both estimates are statistically significant at the five and one percent confidence levels, respectively. Additionally, we estimated the Average Treatment Effect on the Treated (ATT), which is the goal of CEM, and prison gang membership had statistically significant increase in the probability of both recidivism and re-arrest of eight percentage points.

Discussion and conclusion

We examine the effect of prison gang membership on the risk of recidivism from a sample of released inmates from the Illinois penal system. The prison gang selection problem is

analyzed in a potential outcomes framework which allows us to postulate the conditions under which an average treatment effect of gang membership on recidivism can be estimated. We then use Coarsened Exact Matching, a matching algorithm that has several advantages over the more popular propensity score matching, to balance the gang and non-gang groups on observables. Our results suggest prison gang membership increases recidivism by approximately six percentage points, which when evaluated at the sample means is a quantitatively large effect.

The literature addressing street gangs has been a mainstay of research since Thrasher's classic 1927 work. Alternatively, the discussion on how prison gangs relate to their street counterparts/compatriots has only recently been undertaken (Fleisher & Decker, 2001a; Decker, 2007; Griffin, 2007). One recent study seeks to dissaggregate the relationship between gangs, guns, and drugs and recidivism (Huebner et al., 2007). The analysis identified street gang membership of a cohort of prison releasees as a significant contributor to reconviction. Echoing recommendations offered by others (Fleisher & Decker, 2001b), the authors advocate for additional resources to be dedicated to prison programming and improving the prison-community transition after release (Huebner et al., 2007). These recommendations should be augmented with additional research to further specify how prison gang membership interacts with both street gang membership as well as the traditional hallmarks of post-release failure.

All of those in our sample who were deemed by IDOC officials as participating in prison gang activity were identified at reception and classification as street gang members. Therefore, our results have clear implications for the importation model (Irwin & Cressey, 1962). The literature establishing the connection between street gang involvment and recidivism is estabilshed enough not to require further elaboration, and the paper cannot speak directly to this point. However, the connection between in-prison misconduct and subsequent misconduct being

linked to prison gang identification supports an emerging thesis merging life-course criminology (Laub & Sampson, 2003) with the importation model. The "life-course importation" model (DeLisi et al. 2011) invites reflection on how prison authorities should attempt to develop the prison experience as a turning point toward confomity, as opposed to repeat offending.

Our test is less than definitive however, given the limitations of the data. One of the strengths of the data—its origins in a chronic gang state—may simultaneously cause estimation bias because of unobserved heterogenity in the prison gang members. There may be substantial differences among those classified as gang members. Consequently, these findings should be interpreted with caution by those interested in crafting effective policy solutions. Yet, following this line of reasoning, we would argue that an experimental design, if it were possible to implement some sort of random assignment mechanism, would not deliver the information to help us understand the relationship between post-release outcomes and prison-gang membership (see Heckman, 2005).   That is, the mechanisms which assign treatment are often crude constructions that do not allow the researcher to identify the channels through which "causes" determine "effects." Additionally, random assignment by itself can only aid us in estimating the local treatment effect and cannot provide information on what might happen if treatment were assigned to agents in a different environment (see Heckman, 2005).

However, we do see an opportunity for ethnographic data to bridge the gap in our understanding of gang-member selection and later outcomes. This recommendation affirms that of at least one gang scholar (McGloin, 2007) who promotes the idea that processual (e.g. Short & Strotdbeck, 1965) issues are integral to deepening our understanding at the micro level. Ethnographers uncover first-order correlations which would not necessarily be observed in survey or experimental data because participant-observers are privy to the data generating

process as it happens in real time. However, the unique, restrictive nature of prison gangs makes ethnographic research more difficult than in other instances, such as street gangs (Venkatesh, 1997; 2006). Past qualitative studies of prison life and prison social organization come from former inmates (Irwin 1970; 1980), academics serving as correctional officers and staff (Clemmer, 1940; Sykes, 1958; Carroll 1974; DiIulio, 1987; Fleisher, 1989), and even from non-academic correctional officers (Morrill, 2005; Morales, 2008). One sociologist has recently lamented the fact that detailed studies of "the everyday world of inmates in America have gone into eclipse just when they were most needed on both scientific and political grounds" (Wacquant, 2002: 371; also Simon, 2000). In this context, ethnographic data are crucial because the process of gang-member selection likely has important subtelties which could be captured and incorporated into an improved survey design. As our potential outcomes framework makes clear, a rich set of pre-treatment covariates which accurately describe the selection process would allow us to obtain credible estimates of gang-membership on recidivism. Hence, closer cooperation between ethnographers and analytical researchers could greatly improve our understanding of the prison experience and its later effects.

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Notes

[1]The potential outcomes framework is attributed Donald Rubin and is often referred to as the Rubin Causal Model in the statistics and econometrics literature. See Holland (1986).

[2] On the definition of prison gangs, see also Lyman (1989) and Pyrooz et al (2011).

[3] There has also been must less work and progress on identifying the key aspects of prison gangs (Pyrooz et al 2011, 16).

[4] This relates to the development and importation of inmate norms, see Hunt, Riegel, Morales, and Waldorf (1993).

[5] The potential outcomes approach is common to the program evaluation literature. In the following discussion of the potential outcomes framework, we rely on previous discussions by Holland (1986), Todd (2008), Imbens and Wooldridge (2009), and Angrist & Pischke (2009).

[6] Other terms for unconfoundedness include the following: selection on observables, exogeneity, and conditional independence. We use these terms interchangeably with unconfoundedness in the text. Covariate overlap is also synonymous with common support in the literature.

[7]CEM is a member of the Monotonic Imbalance Bounding class of matching algorithms (Iaucs, King, & Porro, 2011). Monotonic Imbalance Bounding differs from the Equal Percent Bias Reducing class of matching methods, which includes propensity-score matching (PSM), as Equal Percent Bias Reducing only addresses mean imbalance (Iaucs, King, & Porro, 2011). For the purpose of exposition, we compare CEM to PSM in this discussion in lieu of repeatedly referencing the class of estimators each belongs to, MIB and ECB, respectively.

[8] We also ran χ^2 tests of independence for gang membership and recidivism/rearrest and were able to reject the null hypothesis at the 1 percent level in each case.

[9] Because most of the variables are binary and there is no obvious theoretical reasoning to manually coarsen *priors*, we use the automatic binning algorithm from (Blackwell, Iacus, King, & Porro, 2009).

[10] Importance weights are generated in STATA from the CEM procedure and they reflect the quality of the match between observations. For an intuitive explanation of the weights, see https://docs.google.com/document/d/1xQwyLt_6EXdNpA685LjmhjO20y5pZDZYwe2qeNoI5dE/edit

[11] An anonymous reviewer posed an important question regarding the preference for Coarsened Exact Matching over Propensity Score Matching. The query assumes more importance in light of the similar results produced. In reaction we offer the following reply in justifying a preference for the former technique: We agree with this critique in general and we do not advocate for the abolition of propensity score matching. However, there are no behavioral assumptions associated with these matching algorithms—they are all at some level black-box routines that improve the efficiency of our estimates. Coarsened Exact Matching is as close to exact matching as one can get when dealing with continuous variables. It also has nice statistical features that we highlight in the comment on CEM above.

Technical appendix

The imbalance statistic is calculated by first cross-tabulating pre-treatment covariates, *X*,

from the treatment and control groups and sorting them into bins according to pre-determined

cutpoints (Iacus, King, & Porro, forthcoming). Let $H(X_1)$ be the set of values generated from the coarsening of continuous variable X_1, with binary and categorical variables retaining their same values (Iacus, King, & Porro, forthcoming). A multi-dimensional histogram is created from the Cartesian product $H(X)=H(X_1)\times...\times H(X_k)$ (Iaucs, King, & Porro, 2011). Let $t_{l_1}...t_{l_k}$ and $c_{l_1}...c_{l_k}$ represent the relative frequency distributions for treatment and control groups, respectively, in k-dimensional space. Multivariate imbalance is then measured by the L_1-distance: $L_1(t,c) = \frac{1}{2}\sum_{l_1...l_k \in H(X)}|t_{l_1...l_k} - c_{l_1...l_k}|$ (Iaucs, King, & Porro, 2011). Conditional on the level of coarsening, $L_1 \in (0,1)$ gives the fraction of the covariate overlap for the treatment and control groups. $L_1 = 0$ represents perfect (up to the level coarsening) multivariate balance and $L_1 = 1$ would indicate perfect multivariate imbalance.

TABLE 1: VARIABLE DEFINITIONS, MEANS (STDEV), LOGIT ESTIMATES

Variable	Definition	Mean	Logit re-arrest	Logit return
re-arrest	=1 if re-arrested	0.660 (0.474)	X	X
return	=1 if return to prison	0.493 (0.500)	X	X
gang	=1 if in a prison gang	0.282 (0.450)	0.053* (0.031)	0.054* (0.031)
priors	# of prior arrests before incarceration	14.540 (14.106)	0.011*** (0.001)	0.005*** (0.001)
children	=1 if has children	0.673 (0.469)	0.024 (0.028)	0.053* (0.030)
male	=1 if sex is male	0.844 (0.363)	0.086*** (0.034)	0.121*** (0.038)
dropout	=1 if no H.S. diploma	0.566 (0.495)	0.059** (0.025)	0.110*** (0.027)
black	=1 if race is black	0.685 (0.465)	0.074** (0.034)	-0.012 (0.037)
hisp	=1 if Hispanic	0.058 (0.234)	0.030 (0.060)	-0.118* (0.065)
married	=1 if married	0.173 (0.378)	-0.009 (0.034)	-0.015 (0.037)
divorce	=1 if divorced	0.118	-0.034	-0.055

		(0.323)	(0.042)	(0.046)
drugcrime	=1 if conviction is for drug crime.	0.393 (0.489)	-0.016 (0.031)	-0.021 (0.032)
sexcrime	=1 if conviction is for sex crime.	0.055 (0.227)	-0.002 (0.056)	0.024 (0.059)
personcrime	=1 if conviction is for crime against person(s)	0.212 (0.409)	-0.086** (0.035)	-0.133*** (0.038)
exage	age exits prison	33.935 (8.588)	-0.009*** (0.002)	-0.005*** (0.002)
act_time_sv	Actual time served in prison in years	1.467 (2.326)	-0.010* (0.005)	-0.005 (0.006)
city	=1 if exit from prison in a urban area	0.278 (0.448)	0.062 (0.042)	0.055 (0.048)
chicago	=1 if exit from prison in Chicago	0.609 (0.488)	0.103** (0.046)	0.067 (0.051)

Notes: For conviction variables, the omitted category is a composite of D.U.I., Property Crime, and Other Crime. Reported coefficients for logit models are marginal effects calculated at sample means for the covariates. Standard errors are calculated using the delta method. Statistical significance at the 10%, 5%, 1% levels indicated by *, **, ***, respectively.

TABLE 2: MULTIVARIATE AND UNIVARIATE IMBALANCE OF PRETREATMENT COVARIATES

Multivariate L_1 Distance: 0.617

Univariate L_1 Distances

Variable	L_1	mean	min	25%	50%	75%	max
priors	0.148	0.294	0	2	2	1	-12
children	0.012	-0.012	0	0	0	0	0
male	0.177	0.177	0	0	0	0	0
dropout	0.134	0.134	0	0	0	0	0
black	0.124	0.124	0	1	0	0	0
hisp	0.076	0.076	0	0	0	0	0
drugcrime	0.068	0.068	0	0	0	0	0
sexcrime	0.030	-0.030	0	0	0	0	0
personcrime	0.056	0.056	0	0	0	1	0
married	0.038	-0.038	0	0	0	0	0
divorce	0.107	-0.107	0	0	0	0	0

Notes: Treatment and control groups are separated on the basis of prison-gang membership. 1630 observations are used to make the calculations.

TABLE 3: MULTIVARIATE AND UNIVARIATE IMBALANCE WITH THE MATCHED SAMPLE

Multivariate L_1 Distance: 0.420

Univariate L_1 Distances

Variable	L1	mean	min	25%	50%	75%	max
priors	0.11913	0.72425	0	1	1	1	-1
childre	0.001	0.001	0	0	0	0	0
male	0.001	0.001	0	0	0	0	0
dropout	0.001	0.001	0	0	0	0	0
black	0.001	0.001	0	0	0	0	0
hisp	0.001	0.001	0	0	0	0	0
drugcrime	0.001	0.001	0	0	0	0	0
sexcrime	0.001	0.001	0	0	0	0	0
personcrime	0.001	0.001	0	0	0	0	0
married	0.001	0.001	0	0	0	0	0
divorce	0.001	0.001	0	0	0	0	0

Notes: Treatment and control groups are separated on the basis of prison-gang membership. 1131 observations are used to make the calculations.

Table 4: Estimates of Prison-Gang Affiliation on Recidivism and Rearrest

Variable	Logit re-arrest			Logit return		
	unmatched	matched		unmatched	matched	
gang	0.134***	0.074***	0.064**	0.100***	0.065**	0.060*
	(0.027)	(0.029)	(0.030)	(0.028)	(0.031)	(0.032)
exage			-0.003**			-0.002
			(0.002)			(0.002)
act_time_sv			-0.020***			-0.009
			(0.005)			(0.006)
city			0.094*			0.106
			(0.057)			(0.066)
chicago			0.133***			0.097
			(0.053)			(0.062)
observations	1630	1131	1124	1630	1131	1124
Treatment	459	411	410	459	411	410
Control	1171	720	714	1171	720	714
Pseudo R-sq	0.0118	0.0047	0.0230	0.0058	0.0028	0.0068

Notes: *For conviction variables, the omitted category is a composite of D.U.I., Property Crime, and Other Crime. Reported coefficients for logit models are marginal effects calculated at sample means for the covariates. Standard errors are calculated using the delta method. Statistical significance at the 10%, 5%, 1% levels indicated by *, **, ***, respectively.*