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## Arrested Friendships? Justice Involvement and Interpersonal Exclusion among Rural Youth

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### Abstract

**Objectives:** We examine the impacts of adolescent arrest on friendship networks. In particular, we extend labeling theory by testing hypotheses for three potential mechanisms of interpersonal exclusion related to the stigma of arrest: rejection, withdrawal, and homophily.

**Method:** We use longitudinal data on 48 peer networks from PROSPER, a study of rural youth followed through middle and high school. We test our hypotheses using stochastic actor-based models.

**Results:** Our findings suggest that arrested youth are less likely to receive friendship ties from school peers, and are also less likely to extend them. Moreover, these negative associations are attenuated by higher levels of risky behaviors among peers, suggesting that results are driven by exclusion from normative rather than non-normative friendships. We find evidence of homophily on arrest but it appears to be driven by other selection mechanisms rather than a direct preference for similarity on arrest.

**Conclusions:** Overall, our findings speak to how arrest may foster social exclusion in rural schools, thereby limiting social capital for already disadvantaged youth.

### Keywords

arrest; peer networks; rural schools; social exclusion

Millions of Americans pass through criminal justice systems each year, and this has consequences for their social bond, or integration into society. The rupturing of this bond, manifested through diminished participation in conventional institutions and blocked or broken normative relationships, is referred to as social exclusion (Silver 1994; Travis 2002). Social exclusion is harmful because it leaves individuals with less social capital (i.e., fewer resources upon which to draw from institutional ties and social networks; Coleman 1988; Daly and Silver 2008) and lower informal social control (i.e., less social pressure toward conformity and law abidance; Sampson and Laub 1993, 1997). Two lines of research on this topic have emerged in the wake of mass incarceration. The first examines associations between justice involvement and institutional participation, such as employment, education, and civic engagement (Brayne 2014; Burch 2011; Pager 2003). The second centers on interpersonal relationships, such as ties to family or others (Braman 2004; Turney 2015). Each attends to a different manifestation of social exclusion. We therefore follow Jacobsen (2020) by referring to the first as institutional exclusion and the second, which is the focus of this paper, as *interpersonal exclusion*. Our study centers on the mechanisms by which justice contact may foster interpersonal exclusion.

Labeling theory, particularly when considered from a life-course perspective (Elder 1998; Sampson and Laub 1997), is a useful framework for understanding the consequences of justice involvement for social exclusion. One of its key propositions is that legal sanctions that are stigmatizing increase the risk of exclusion from institutional participation and interpersonal ties (Goffman 1963; Paternoster and Iovanni 1989). In testing this proposition, research on institutional exclusion has made more headway than studies of interpersonal exclusion have in explaining associations with justice involvement. In particular, there seems to be broad consensus that stigma is a key mechanism in explaining why applicants with criminal histories are less likely to find stable employment or housing, or to be admitted to a college or university (Evans and Porter 2015; Stewart and Uggen 2020; Sugie, Zatz, and Augustine 2020). In contrast, the role of stigma in interpersonal exclusion is less clear. Some research suggests that criminal justice stigma may be transferred through social networks to the family or friends of individuals who are directly involved in the justice system (Braman 2004; Tinney 2020), but few studies suggest stigma is responsible for severing close social ties (Massoglia, Remster, and King 2011).

There are at least two reasons for the inconsistency between the proposition of interpersonal exclusion and the findings of prior research. First, previous studies have centered on intimate family relations rather than non-family ties like friendships (Geller 2013; Siennick, Stewart, and Staff 2014). Although they both play critical roles in human development, friendships may be more susceptible to breakup by criminal justice stigma because they are generally less bound by expectations of long-term devotion. Therefore, we examine friendship ties in this study and concentrate on adolescence, a stage in the life course when friends are central to one's social network (Warr 2002). Normative friendships, defined as close relationships with peers who are involved in conventional activities and who avoid developmentally risky behaviors, are a key source of social capital and control in adolescence. They offer socialization, companionship, and information that benefit youth and increase their stake in conformity as they near the transition to adulthood (Hirschi 1969; Stanton-Salazar and Dornbusch 1995). Thus, it is important to understand how youth form peer relationships

and to identify barriers to normative friendships. Criminal justice stigma is one factor that may affect friendship preferences. Specifically, we focus on arrest, which many US youth experience (between 16% and 27%; Brame et al. 2012) and which has been linked to such negative outcomes as diminished educational attainment and subsequent behavior problems (Kirk and Sampson 2013; Wiley, Slocum, and Esbensen 2013).

Second, prior studies use broad national samples that mask more specific contexts in which justice contact occurs (e.g., Massoglia et al. 2012), or they rely on disadvantaged urban samples where justice contact resembles a “normalized” rather than stigmatized experience (e.g., Turney 2015; regarding normalization, see Hirschfield 2008; Pettit and Western 2004). In contrast, rural communities also face problems of concentrated disadvantage (Elder and Conger 2000) but are often characterized by factors that would increase informal sanctions for arrested youth by reducing anonymity and reinforcing norms. Such factors include cultural homogeneity, connectedness among neighbors and kin, and high network closure (Marsden and Srivastava 2012; Weisheit, Falcone, and Wells 2006; Wuthnow 2018). In our study, we examine arrest and friendship networks among youth in rural contexts. In doing so, we add to a growing body of criminological research on rural peer relationships (e.g., Cotter and Smokowski 2016; Osgood, Feinberg, and Ragan 2015), in which the role of punishment and stigma deserves more attention.

Our objective in this paper is to extend prior research on justice involvement and social exclusion by examining consequences of arrest for normative school friendships. For this, we build upon a contemporary adaptation of labeling theory by Sampson and Laub (1997), in which they integrate a classic labeling model with their life-course theory of informal social control (Paternoster and Iovanni 1989; Sampson and Laub 1993). In particular, we consider the proposition of interpersonal exclusion by testing hypotheses for three stigma-related mechanisms of arrest and friendship selection: rejection, withdrawal, and homophily. In doing so, we extend the work of Jacobsen (2020) which assessed rejection and withdrawal as mechanisms of friendship selection after school discipline. We build on that study by investigating homophily as an additional mechanism and by accounting for the broader networks of school peers in which friendship ties emerge and deteriorate. We also shift from a focus on school sanctions to examine formal social control that occurs in the broader community. Our data are uniquely suited for these purposes. They follow 48 peer networks through middle and high school, allowing us to model changes in friendship preferences over time. Evidence of these mechanisms would imply that criminal justice systems amplify social exclusion among rural youth by stratifying access to friendships that would foster normative development and a stronger societal bond.

## Background

### Labeling Theory and Social Exclusion

A major aim of labeling theory is to explain why people who experience justice contact often continue to exhibit delinquent behavior (“secondary deviance” Lemert 1951). Briefly, it suggests that a stigmatizing sanction is criminogenic because it results in exclusionary reactions from conventional institutions and individuals, as well as greater acceptance by similarly-stigmatized peers (Paternoster and Iovanni 1989). Sanctioned youth respond to

these societal reactions by confirming their delinquent label (Tannenbaum 1938) and may eventually internalize it completely, resulting in a delinquent self-concept. After years of mixed empirical support, Sampson and Laub (1997) restated this model as a developmental control theory by integrating it with their age-graded theory of informal social control (Sampson and Laub 1993). In doing so, they place less emphasis on label internalization, making social exclusion the primary mechanism. In this way, justice involvement is seen as a turning point in the life course because it weakens the social bond, resulting in persistence in delinquency. A key mechanism of the impact of justice involvement on the social bond is stigma (Goffman 1963). In line with this proposition, many studies document the impacts of youth justice contact on weakened institutional attachment. On balance, these studies suggest that youth who are stigmatized by justice involvement may be *pushed* out of school through institutional responses (e.g., administrative labeling, juvenile detention) or *pull away* to avoid surveillance and further apprehension, and that these processes interfere with high school completion, as well as college or employment opportunities in young adulthood (Bernburg and Krohn 2003; Brayne 2014; Hjalmarsson 2008; Legewie and Fagan 2019; Widdowson, Siennick, and Hay 2016). Some also hypothesize that criminal justice stigma may “damage social relationships” *within* the school (Kirk and Sampson 2013:37), but this has not yet been adequately examined. We extend labeling research by moving beyond a focus on institutional exclusion or secondary deviance to test associations of arrest with mechanisms of interpersonal exclusion in rural schools.

### Friendship Networks in Rural Schools

Labeling theory from a life-course perspective calls attention to the interdependency of linked lives, including friendships, and the social forces that are expressed through these relationships (Elder 1998; Sampson and Laub 1997). Close friends are an important part of an adolescent’s societal bond because they offer social capital that fosters wellbeing through the life course (Crosnoe 2000; Narr et al. 2017). They provide support beyond family or mentors where youth may turn for information about health behaviors, study habits, and college or job preparation (e.g., Nelson 2016). In rural areas, such social capital may be especially important because other resources are generally limited. Out-migration to city centers leaves schools and families with less human capital and fewer economic means, and travel distances limit opportunities that are more abundant in larger cities (Roscigno, Tomaskovic-Devey, and Crowley 2006). Thus, it is important to understand how rural youth form close peer relationships and the obstacles they face in establishing and maintaining normative friendships.

Our perspective assumes that youth choose friendships within the opportunities and constraints their circumstances provide. In rural areas, friendship preferences are constrained by geographic distance and low population density. They are also structured by school and district boundaries. Schools bring rural youth together by busing in those who live farther out, and sorting students by grade. This organization assures that a relatively consistent body of peers—a student’s pool of potential friends—advances together from grade to grade through middle and high school. Inside this structure, friendship networks evolve according to student preferences. As networks evolve, patterns emerge that link friendship preferences to certain youth attributes. In rural schools and friendship networks more broadly, youth tend

to choose friends with whom they share similar demographic or behavioral characteristics (Hirschi 1969; Lazarsfeld and Merton 1954; McMillan, Felmlee, and Osgood 2018; Osgood et al. 2015). They may prefer friends whose behaviors are associated with status (e.g., drinking alcohol; Moody et al. 2011) and reject peers whose behaviors are aggressive or antisocial (particularly in childhood, as results are more mixed in adolescence; Dodge 1983; Kreager 2007; Rulison, Kreager, and Osgood 2014; Young 2014). Though past research has helped to advance knowledge of friendship selection processes, the role of punishment and stigma in disrupting the development of normative friendships has not yet been adequately examined.

Stigma is manifested through relationships between attributes and friendship preferences. Goffman (1963:3–5) defines stigma as “undesired differentness” marked by an attribute that is “deeply discrediting” in a given context. Stigma emerges when such an attribute has visibility or “known-about-ness” (Goffman 1963:49), and in the context of rural communities, this is fostered by dense, overlapping ties among youth and their families (neighbors, members of same religious congregation, same sports team, etc.), which limit a student’s anonymity. This structure, combined with shared norms, increases informal social control (Crockett, Shanahan, and Jackson-Newsom 2000; Freudenburg 1986; Osgood and Chambers 2000), giving rise to negative reactions toward youth with characteristics that are seen as undesirable or discrediting (Sherman 2006; Williams et al. 2008). An arrest may be one such characteristic.

### Stigma of Arrest in Rural Schools

There are more than 10 million arrests each year, including 700,000 of children and youth (Federal Bureau of Investigations 2020). Arrest rates have declined in recent decades, but some research suggests this reduction has been slower in less populated areas. One report finds that rural and small or mid-size counties have higher arrest rates today than urban or suburban counties (Rad, Yang, and Wunschel 2020). Despite this pattern and the fact that 8.2 million youth (grades 6 to 12) attend public school outside a city or suburb,<sup>1</sup> researchers know little about the consequences of arrest for rural youth. Indeed, prior research focuses mostly on urban or national samples rather than rural youth (Kirk and Sampson 2013; Widdowson et al. 2016).

One challenge in understanding how arrest impacts adolescent friendships is the variation in how arrests are defined. There are notable differences across jurisdictions in the conditions of police encounters that are recorded as arrests (Miller 2014; Sherman and Glick 1984). What may be recorded as an arrest in one area or situation may not be recorded as an arrest in another. Furthermore, there is ambiguity among youth who have been apprehended by police, and in some cases even handcuffed (Myers 2002), as to whether their encounter was an “official arrest” (Kirk 2006). Whereas studies of *institutional* exclusion capture official arrests through administrative records, self-reports, or audits (Grogger 1995; Hjalmarsson 2008; Uggren et al. 2014), our study of *interpersonal* exclusion aims to capture both official and unofficial (not formally recorded) arrests by defining an arrest as *being picked up by*

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<sup>1</sup>Based on urban-centric locale data from the National Center for Education Statistics (<https://nces.ed.gov/ccd/elsi/tableGenerator.aspx>), school year 2019–2020.

*police for suspected law violation.* Unofficial arrests, such as a trip to the police station or being apprehended and taken home to parents, are common in police-youth encounters (Schulenberg 2010), perhaps especially in rural areas where informal social controls are salient (Weisheit et al. 2006; Peterson 2019).

Regardless of whether it results in an official record, being picked up by police for suspected law violation may be stigmatizing among rural youth. This is because rural youth learn about arrests in ways other than a records check. Peers may witness the event at school (more than 60,000 students were arrested at school in school-year 2013–2014; Civil Rights Data Collection 2019), or they may see it happen elsewhere or learn about it through small-town gossip or social media (though internet access may be less prevalent in rural areas; Whitacre and Mills 2007). Negative reactions would then be fostered by dense social ties and attitudes. For example, rural youth may have positive views of police and know them in multiple roles (coach, parent of classmate; Adorjan, Ricciardelli, and Spencer 2017; Hurst 2007), making them less likely to sympathize with a peer whom the police have apprehended. In addition, relatives and friends may have negative attitudes toward people involved in the justice system (e.g., some research finds political conservatism associated with such attitudes; Hirschfield and Piquero 2010), and parents who hear of the arrest may discourage their child from socializing with the arrested peer. Thus, the stigma of arrest may impact adolescent friendship networks, and we seek to examine specific friendship selection mechanisms through which this may occur.

### **Mechanisms of Interpersonal Exclusion**

To advance knowledge of the impacts of justice involvement on the social bond (Sampson and Laub 1997), we examine processes through which the stigma of arrest may affect interpersonal ties. Relying on Goffman (1963), we anticipate that the stigma of arrest is manifested through three mechanisms of friendship selection: rejection, withdrawal, and homophily. We discuss rejection and withdrawal first. *Rejection* refers to the efforts of youth to distance themselves from arrested peers in response to their arrest. It would be evident if students were less likely to prefer arrested youth as friends.<sup>2</sup> *Withdrawal* refers to the actions of arrested youth when negative stereotypes of people who are involved in the justice system (Hirschfield and Piquero 2010) take on personal significance (Link and Phelan 2001). Goffman (1963:17) refers to this as anticipatory “defensive cowering,” avoiding peers to avoid rejection. It would be evident if arrested youth were less likely to prefer friendships with school peers after their arrest.

Rejection and withdrawal have not been adequately assessed in the context of youth justice contact, but some studies have examined broader associations between arrest and peer relations. For example, several city or school-based studies found that arrested youth are less likely to have friends involved in prosocial behaviors and more likely to have friends engaged in delinquency (Bernburg, Krohn, and Rivera 2006; Kirk and Sampson 2013; Wiley et al. 2013). These studies are in line with our overall argument that arrest may be associated with declines in normative friendships, but they lack two conditions required

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<sup>2</sup>Our definition of rejection differs from one that is based on how much a student is disliked by peers (Dodge 1983) but is consistent with Goffman’s (1963) emphasis on avoidance.

for testing rejection and withdrawal as we have defined them. The first is a network of students in which nominations received (being named as a friend) may be distinguished from nominations made (naming a friend). For example, prior research finds parental incarceration associated with fewer friendship nominations extended to school peers but weaker evidence of an association with fewer nominations received (Bryan 2017; Cochran, Siennick, and Mears 2018). Differentiating between rejection and withdrawal not only requires information about the existence of friendship ties but also the *direction* of the ties. The second requirement is information on *changes* in nominations over time. Rejection and withdrawal imply changes in a network from one point in time to the next. We examine whether these changes are associated with a recent change in arrest and are therefore not due to other sources of stigma that are potentially correlated with arrest but occur earlier in the life course or are stable over time (e.g., preexisting parent criminal history or family position in community).

Our third mechanism, *homophily*, occurs when arrested youth seek out close friendships with peers who have also been arrested. In particular, we are interested in homophily on arrest that is not due to structural characteristics of the network, similarity in behaviors, or other characteristics that are correlated with arrest. For arrested youth, peers who have been arrested may represent those whom Goffman (1963:19) calls “sympathetic others,” because they can offer acceptance and support as arrested youth navigate their justice system involvement. This is similar to ways in which incarcerated adults rely on peers in prison for mentorship and role modeling (Kreager et al. 2017). Homophily would be evident if arrested youth more often prefer friendships with peers who have also been arrested.

Labeling theory suggests that blocked or broken ties to normative friendships will constrain youth toward “deviant others,” peers who are involved in developmentally risky behaviors like substance use or illegal acts (Paternoster and Iovanni 1989:378). Sampson and Laub (1997) describe this as part of a process of cumulative disadvantage spurred by justice contact; not only are social bonds weakened but the potential for peer influence toward delinquency increases (Bernburg et al. 2006). Thus, in addition to testing propositions of rejection and withdrawal, we also assess whether the effects of these selection mechanisms are attenuated when peers are involved in risky behaviors. Notably, if arrested youth are obliged to form such relationships after rejection or withdrawal from normative friendships, then non-normative peer relationships may occur regardless of whether there is homophily on arrest.

## Current Study

We advance knowledge of the consequences of justice involvement for social exclusion by examining associations between arrest and friendships in rural schools, where we expect criminal justice stigma to be influential in shaping social networks. In a previous study, Jacobsen (2020) found that punishment in the form of school suspension among rural youth was associated with fewer school friendship nominations received and extended. The current study examines a more serious institutional response—arrest—that occurs within the broader community and may have significant implications for the development of social bonds and social capital among rural youth. Beyond our focus on arrest, this study also

builds upon Jacobsen (2020) in two other important ways. First, whereas that study defined rejection and withdrawal as discontinuity in already established friendships, we consider the larger network of peers in which ties emerge and deteriorate (i.e., likelihood that a tie to *any* student is present). For this, we rely on longitudinal analysis of sociocentric complete network data, and we do this using a longer span of adolescence than was possible for prior studies. A key benefit of this approach is the ability to account for interdependency among peers of the same schools, which standard regression methods are less equipped to address (Snijders, Van de Bunt, and Steglich 2010). Second, this approach allows for the examination of our three mechanisms of interpersonal exclusion simultaneously, similar to prior research on friendship selection and mental health stigma (Schaefer, Kornienko, and Fox 2011). This allows us to obtain estimates of each process while controlling for the others. Among rural youth, we expect that arrest is associated with a lower likelihood of friendship nominations made (withdrawal), a lower likelihood of nominations received (rejection), and a greater likelihood of nominations made to arrested peers (homophily). These hypotheses of interpersonal exclusion compete with predictions of other theories in which justice contact could be a symbol of status that resembles adulthood (Moffitt 1993) or opposition to mainstream culture (Cohen 1995). Indeed, such attitudes may be no less prevalent among rural youth than they are among disadvantaged urban youth because of concentrated poverty and geographic isolation (Keith and Griffiths 2014).

A benefit of our longitudinal network approach is the ability to test for patterns in friendship selection that are consistent with hypotheses of interpersonal exclusion (i.e., conceptualizing rejection, withdrawal, and homophily as changes in friendship ties that are associated with arrest, net of observed confounders), but a limitation is that it does not involve measuring stigma directly, such as through attitudes toward arrested youth (e.g., Zhang 1994). If associations between arrest and friendship selection are driven by stigma, as we assume, they should be robust to controls for other correlates of arrest and friendship selection. Potential confounders include risky behaviors, sensation-seeking tendencies (Gottfredson and Hirschi 1990; Osgood et al. 2015), missed school (and time with friends) due to case processing or suspension (Duxbury and Haynie 2020; Jacobsen 2020), diminished school attachment due to fewer perceived benefits of academic pursuits (Kirk and Sampson 2013), and relationships with peers who exhibit any of these characteristics. Our analyses account for these potential alternative explanations. Further, if declines in school friendships are due to stigma, we should expect them to be driven by weakened normative relative to non-normative friendships. Thus, we also examine whether negative associations between arrest and friendship ties (rejection and withdrawal) are weaker when the arrested youth's peers are involved in risky behaviors.

## Data and Methods

We rely on PROSPER, a longitudinal study of all students in 28 rural school districts of Iowa and Pennsylvania (Spoth et al. 2007). Rural districts are defined as those covering small populations; none have more than 45,000 people, and the mean is 19,100. Only four districts gain half or more of their populations from urbanized areas of over 50,000. To be eligible to participate, districts needed enrollments of 1,300 to 5,200 students with at least 15% eligible for free or reduced-price lunch. One district declined to participate



in the peer nomination part of data collection and is excluded from this study. These criteria resulted in schools being larger on average than rural and small-town schools nationally (online supplement Table S1). Using PROSPER allows us to extend beyond prior research by focusing on justice contact among youth in rural contexts and by following multiple peer networks over time, thus allowing for the examination of friendship selection mechanisms associated with arrest. Furthermore, by relying on school-based surveys, these data reduce concerns that results will be driven by more serious forms of justice involvement like confinement. All respondents are in school (not incarcerated) when completing the questionnaires, and we observe absences between waves of data collection.

Interviewers administered in-school baseline questionnaires to two sixth-grade cohorts (Fall of 2002, Fall of 2003). Students completed follow-up questionnaires in Spring of the same year and every year after to twelfth grade (8 waves of data collection). In each district, students began in multiple (usually two) smaller middle schools before merging, usually at ninth grade, into one larger high school. At each wave, all students of the same cohort in the district were invited to participate, permitting them to enter or exit the study at any wave. Participation rates are high (Table A1). We exclude baseline observations because the first two waves were collected in sixth grade, creating overlap between survey items that reference past-year, and we exclude twelfth-grade observations because some districts had low completion rates at this last wave (below 40%). Our initial 54 networks (two cohorts in each of the 27 districts) are each comprised of students who completed at least one questionnaire in Spring of grades 6 to 11 (92% of students on the school rosters). Across all networks (district-cohort combinations) and waves of data collection, we have more than 68,000 observations from nearly 15,000 students.

## Variables

**Friendships.**—At each wave, students were asked to list the names of their two closest friends and up to five other close friends in their same grade and school. Across school districts, 96% of all students made at least one nomination during the study, and 80% of nominations were matched successfully to class rosters (Table A2). Unsuccessful matches occurred when students nominated a name not included on the roster (18%) or because multiple names were plausible (2%). We refer to respondent nominations of peers as outgoing ties and peer nominations of the respondent as incoming ties. Across school districts, the average number of incoming or outgoing ties (i.e., degree) ranges from 6.02 in sixth grade to 4.31 in eleventh grade (Table A3; also see means of network and out-of-network ties by arrest in online supplement Figure S1).

**Arrest.**—We define arrest as being picked up by police for illegal behavior, whether it is officially recorded or not. The survey item reads, “During the past 12 months, how many times have you been picked up by the police for breaking the law?” This definition likely includes some apprehensions that are informal and may even be less known about among peers. If so, this would bias our results downward and result in conservative estimates for our three mechanisms. We consider an arrest to be a potential marker students carry with them as they move from grade to grade. Thus, we use two time-varying binary measures of arrest. The first indicates whether the respondent ever reported being arrested by a given

wave or grade in school (coded 1 at every wave beginning with the first wave a student reports an arrest; students who never report an arrest are coded 0 for all waves).<sup>3</sup> This allows us to examine friendship changes among youth who carry the stigma of arrest, relative to youth who do not. The second measure represents the youth's first-reported arrest (coded 1 at the wave a student first reports an arrest but 0 at all other waves; students who never report an arrest are coded 0 for all waves; see online supplement Table S2 for a discussion of how our arrest measures differ from those of other large surveys).

**Control Variables.**—To account for the possibility of a spurious relationship between arrest and friendship outcomes, we include four indicators of risky behavior: two types of substance use in the past month (smoking marijuana and drinking alcohol), delinquency in the past year, and sensation-seeking tendencies. We also account for school absence, the student's attachment to the school, family relations, gender, race (white/nonwhite), and free or reduced-price lunch, the latter as a proxy for socioeconomic disadvantage (detailed description of control variables in Table A4). In addition, we include dummy variables for wave (grade) and network (i.e., district-cohort combination). Ideally, we would also include an indicator of school discipline, which may precede or accompany an arrest (Mowen and Brent 2016), but suspension data are only available for a small subsample of students who participated in an in-home survey (Jacobsen, 2020). We describe this subsample and our attempts to control for suspension in the online supplement.

### Modeling Strategy

We use stochastic actor-based (SAB) models, estimated with the Simulation Investigation for Empirical Network Analysis (SIENA) statistical program (Snijders 2001; Snijders et al. 2010). SIENA produces estimates of processes associated with change in network panel data. It decomposes wave-to-wave change into a series of simulated microsteps, each representing an opportunity for a single actor to add a friend, drop a friend, or make no change to outgoing ties. SIENA begins with the data at each wave and relies on these simulated microsteps to determine what changes would lead the observed data at next wave, and estimates are adjusted until patterns of change in the simulations are comparable to patterns in the observed data. Changes are modeled as outcomes of a Markov process, in that the probability of a change at any given point depends only on observed data at the current wave and is not informed by prior observations. Given that youth who are not arrested at a particular wave may experience stigma due to an earlier arrest, we use the two arrest measures described earlier: (1) the youth's first-reported arrest and (2) whether the youth has ever reported an arrest by a given wave.

Our SIENA SAB models include three parameters for each of the covariates. First, *alter* parameters indicate whether an attribute is associated with receiving friend nominations. Positive estimates for this parameter suggest that youth with higher values of the attribute *receive* more nominations relative to those with lower values. In our study, we use the alter parameter for arrest (*alter arrest*) to test for peer rejection, as we expect youth who report an

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<sup>3</sup>We keep youth who were arrested at baseline (3% of baseline respondents) in our analytic sample, but in supplemental analyses we removed them and regression results were the same.

arrest to receive fewer nominations relative to their peers. Second, *ego* parameters indicate whether an attribute is associated with making nominations. Positive estimates suggest that youth who have higher values of the attribute *make* more nominations relative to those with lower values. We use the ego parameter for arrest (*ego arrest*) to examine withdrawal, which would occur if students who report an arrest make fewer nominations relative to their peers. Third, *similarity* parameters indicate whether an attribute is associated with nominating peers who are more similar to (or the same as, if the attribute is binary) the actor on a given attribute. Positive estimates reflect a tendency to be friends with others who share similar values. We use the similarity parameter for arrest (*arrest similarity*) to examine arrest homophily, as we expect arrested youth to be more likely than their peers to nominate other arrested youth. For each variable, these three estimates are produced simultaneously so that each controls for the others. By including alter, ego, and similarity parameters for each of our covariates, our models incorporate information about respondent and peer characteristics at each wave, including risky behaviors, when estimating the change in friendship ties that occurs between waves. Additionally, SIENA allows us to control for factors that are endogenous to the networks, in that they emerge regardless of student attributes (network processes and structural parameters described in Table A4).

The current study relies on 47 networks (24 districts), comprised of 48,747 completed surveys of 12,524 students (online supplement Table S3).<sup>4</sup> Of the 54 original networks, we excluded three (965 students) because of issues during data collection that affected the networks' structures (school fire, missed wave) and four more (1,490 students) because our SAB models did not achieve recommended standards of convergence (Steglich, Snijders and Pearson 2010).<sup>5</sup> SIENA produces a separate estimate and corresponding standard error for each of the 47 networks. We then used three-level hierarchical linear models (HLM) to combine estimates of the separate analyses, resulting in an aggregate estimate for each parameter. Each school district was the level-three unit of analysis, and grade-cohorts within school districts were the level-two unit of analysis. At level one, the precision of the SIENA SAB model estimates (i.e., their squared standard errors) served as a known variance. This procedure, in addition to accounting for the nesting within the study design, also weights each set of parameter estimates inversely by its corresponding standard error, as is common in meta-analysis (Raudenbush and Bryk 2002). To ensure consistency with prior research, we examine the association between arrest and friendship ties (extended, received, and extended to arrested youth) in a set of regression analyses before turning to our SAB models. These supplemental models are presented and described in Tables S4 of the online supplement. Additionally, we control for students' school suspension histories in a subsample analysis described in Table S5.

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<sup>4</sup>Of completed surveys across networks, all but three covariates were missing less than 1% of observations: sensation-seeking (2%), race (2%), and absence (5%). SIENA imputes these values with the global mean (average across waves) of each network for the purpose of the simulations, but only non-missing data are used to produce parameter estimates (Ripley et al. 2019).

<sup>5</sup>Convergence *t* values, which represent the extent to which the simulated data vary from the actual data, are less than  $\pm .10$  across all networks for all parameters, and the overall maximum *t*-ratio for convergence is below .25 for each model. All estimates are from models with five phase-2 sub-phases and 4,000 iterations during phase 3.

## Results

Table 1 presents means, standard deviations, and ranges of our measures for completed survey observations across all 47 networks. Fourteen percent of completed observations (pooled across waves) pertain to youth who reported an arrest by a given grade (8% arrested in past year; 5% first-reported arrest); however, this does not reflect a student's risk over time. By spring of junior year (Wave 7), when most students are about 17 years old, 22% have reported at least one arrest (more information in Table S2).<sup>6</sup> Among the few nonwhite youth in these networks (6% Hispanic, 3% Non-Hispanic black), about 1 in 4 have experienced an arrest by their junior year (32% for boys and 18% for girls), compared to just over 1 in 5 among whites (26% for boys, 17% among girls; network-level variation in arrest presented in Figure S2 of online supplement).

Table 2 presents the results of our main models, with estimates corresponding to the log odds of a friendship nomination. Models 1 and 2 present results for the association between friendship and having ever reported an arrest, and Models 3 and 4 present results for the first-reported arrest. Models 1 and 3 are reduced and include the three parameters related to arrest. These models provide initial estimates of rejection, withdrawal, and homophily within a social network framework, but do not control for the larger set of potential alternative explanations to stigma, such as those related to deviant behaviors or school absences. These models also include a selection parameter for *outdegree (density)*, which has an estimate that corresponds to the likelihood of any student in the network being selected at random and is comparable to an intercept in a traditional linear regression model. In regards to rejection (alter effects), results in Model 1 suggest that students are less likely to extend ties to school peers who have been arrested than they are to school peers who never reported an arrest ( $b=-0.118$ ,  $SE=0.023$ ,  $p<0.001$ ). For clarity, this can be interpreted in terms of odds ratios and from the perspective of youth on the receiving end of the nominations: among previously arrested youth, the odds of receiving friendship nominations are 11% lower compared to non-arrested youth  $[(e^{-0.118} - 1) \cdot 100]$ . Regarding withdrawal (ego effects), results suggest that the odds of nominating friends in school are 21% lower for previously arrested youth compared to non-arrested youth ( $b=-0.241$ ,  $SE=0.025$ ,  $p<0.001$ ). Next, results for homophily (similarity effects) suggest that arrested youth are more likely to nominate other arrested youth as friends. The odds of nominating arrested peers are 12% greater for youth who previously reported an arrest themselves relative to students who never reported an arrest ( $b=0.117$ ,  $SE=0.020$ ,  $p<0.001$ ).

Model 2 assesses whether the results presented in Model 1 are maintained when we account for our control variables and their respective parameters. The estimates for rejection and withdrawal are reduced, but overall these results are robust to the addition of controls variables. For rejection, arrested youth have odds of receiving friendship ties that are 8% lower than the odds among non-arrested youth ( $b=-0.080$ ,  $SE=0.022$ ,  $p<0.01$ ); for withdrawal, arrested youth have odds of extending ties that are 11% lower than the odds for

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<sup>6</sup>This should be considered a lower-bound estimate because those with unknown arrest histories are assumed to not have experienced an arrest. If the prevalence of having ever been arrested is actually higher among youth in our study, it means that results of our hypotheses tests that are based on the ever-reported arrest measure are biased downward.

other youth ( $b=-0.111$ ,  $SE=0.018$ ,  $p<.001$ ). However, the coefficient for homophily reverses direction when controls are added, suggesting that arrested youth are less likely—not more likely—to nominate arrested peers as friends when other predictors of friendship selection are included in the model. Indeed, the odds of nominating arrested peers are 9% lower for arrested youth than they are for non-arrested youth ( $b=-0.093$ ,  $SE=0.016$ ,  $p<.001$ ) when we account for controls.

Results for the parameters associated with first-reported arrest in Models 3 and 4 are consistent in terms of direction and statistical significance with those of Models 1 and 2. Like Model 1, Model 3 does not include control variables. Results of this model suggest that in the year following a first-reported arrest, the odds of receiving friendship nominations are 8% lower than the odds for youth who have not indicated a first-reported arrest ( $b=-0.078$ ,  $SE=0.025$ ,  $p<.01$ ), and the odds of extending friendship ties are also lower, by about 17% ( $b=-0.187$ ,  $SE=0.029$ ,  $p<.001$ ). When control variables are added in Model 4, results are consistent in terms of direction and statistical significance. Arrested youth have odds of receiving ties that are 15% lower than the odds for non-arrested youth ( $b=-0.157$ ,  $SE=0.024$ ,  $p<.001$ ). Their odds of extending ties are also lower, by about the same amount ( $b=-0.163$ ,  $SE=0.026$ ,  $p<.001$ ). In contrast, the coefficient for homophily is positive in Model 3 ( $b=0.085$ ,  $SE=0.020$ ,  $p<.001$ ) and then negative in Model 4, where results suggest that the odds of extending ties to an arrested peer are about 20% lower than the odds for those with no arrest ( $b=-0.227$ ,  $SE=0.017$ ,  $p<.001$ ).

The results in Table 2 are consistent with what we expect if arrest is followed by two of the three mechanisms we have described: rejection and withdrawal. However, they do not offer evidence for whether these associations vary depending on the level of conformity or deviance among peers. To answer this question, we turn to models that include interactions between our measure of first-reported arrest and three measures of risky behaviors—marijuana, drinking, and delinquency—and we test two types of interactions for each behavior. First, we assess the extent to which the negative association between the first-reported arrest and the likelihood of *extending* ties (ego arrest parameter in Model 4 of Table 2) varies with the risky behavior of a student's peers; in other words, whether an arrested youth's withdrawal from peers is more or less likely when those peers are engaged in a particular risky behavior. For this, we examine an interaction between the ego arrest parameter and the risky behavior alter parameter. Second, we test whether the negative association between the first-reported arrest and the likelihood of *receiving* ties (alter arrest parameter in Model 4 of Table 2) differs according to the risky behavior of a student's peers; in other words, whether an arrested youth's rejection by peers is more or less likely when those peers are engaged in a particular risky behavior. Given that this interaction focuses on ties being received, the ego parameters refer to characteristics of a student's peers, so the interaction is between the alter arrest and risky behavior ego parameters.

Table 3 presents results for these interactions, across marijuana use (Models 1 and 2), drinking (Models 3 and 4), and delinquency (Models 5 and 6). Model 1 reveals a positive and statistically significant interaction between the ego arrest parameter and the marijuana use alter parameter ( $b=0.282$ ,  $SE=0.054$ ,  $p<.001$ ). In conjunction with the coefficient for the main effect of withdrawal after the first-reported arrest ( $b=-0.169$ ,  $SE=0.026$ ,  $p<.001$ ),

this suggests the lower likelihood of extending ties to peers after an arrest is attenuated when those peers use marijuana. Similarly, the interaction in Model 2 between the alter arrest and marijuana use ego parameters ( $b=0.322$ ,  $SE=0.045$ ,  $p<.001$ ) indicates that the lower likelihood of receiving ties from peers after an arrest ( $b=-0.160$ ;  $SE=0.024$ ,  $p<.001$ ) is attenuated when those peers use marijuana. Models 3, 4, 5, and 6 present similar results for drinking and delinquency with one exception: the coefficient for the interaction between alter arrest and ego drinking in Model 4 is not statistically significant. Taken together, these results suggest that the negative associations between arrest and friendship are weaker among youth whose peers are involved in risky behaviors. Supplementary analyses that test for associations between network-level characteristics and our parameter estimates are presented and described in online supplement Table S6. In addition, supplementary analyses that examine associations between arrest and out-of-network ties (friends in other grades and schools) are presented and described in Table S7.

## Discussion

The current study extends research on justice involvement and social exclusion by shedding light on the consequences of arrest for adolescent friendships. Whereas prior research is most focused on the impacts of justice involvement on diminished institutional participation (e.g., school, employment; Bernburg and Krohn 2003) or weakened family relationships (Massoglia et al. 2011; Turney 2015), we have built upon contemporary labeling theory (Sampson and Laub 1997) to more carefully consider consequences for normative school friendships. In doing so, we advance an understanding of punishment and stigma in rural areas, which prior research has not often considered. We extend prior research on justice involvement and peer relationships (Cochran et al. 2018; Wiley et al. 2013) by applying a longitudinal social network approach to examine friendship change and account for interdependent relations among peers. In particular, we have drawn from Goffman (1963) to define three mechanisms of friend selection as potential manifestations of criminal justice stigma: withdrawal, rejection, and homophily. Overall, our findings are consistent with our labeling framework in suggesting that justice involvement may foster interpersonal exclusion from adolescent peers, but only withdrawal and rejection seem to be operating in the ways that we expected.

In regards to withdrawal, we found arrest associated with declines in the likelihood of extending friendship ties. The odds of extending ties were about 11% lower for youth who ever reported an arrest than they were for non-arrested youth, and about 15% lower after the first-reported arrest. However, this lower likelihood for arrested youth was attenuated when their peers were more involved in marijuana use, drinking, or delinquency. This suggests arrested youth more often pull away from normative friendships—close relationships with peers who are not involved in risky behaviors and who are most likely to foster academic success and wellbeing (Crosnoe 2000; Wentzel 2017). This is consistent with prior research on arrest and friendship, primarily among urban youth. This prior research, which did not distinguish the friendship preferences of youth from those of their peers, found arrested youth less likely to feel they had friends engaged in prosocial behaviors and more likely to have friends engaged in delinquency (Bernburg et al. 2006; Kirk and Sampson 2013; Wiley

et al. 2013). We argue this decline in friendships, at least among these rural youth, may be driven by criminal justice stigma.

In regards to rejection, we found arrest associated with declines in the likelihood of receiving friendship ties from same-grade peers. The odds of receiving ties were about 8% lower for youth who ever reported an arrest than they were for non-arrested youth, and about 15% lower following the first-reported arrest. This association, like that for withdrawal, was weaker when peers were more involved in marijuana use and delinquency, but not when peers were engaged in drinking. This is probably because among the three behaviors, drinking was most common (and perhaps least deviant) among youth in these rural schools. These findings suggest arrested youth not only pull away from normative friendships, but that their peers pull away from them as well. Thus, arrested youth may be constrained toward relationships with peers who are more often involved in risky behaviors, and these relationships may be harmful for their development (Bernburg et al. 2006). This is consistent with research describing rejection as an antecedent of involvement with antisocial peers (Dishion et al. 1991), but it is somewhat inconsistent with studies of parental incarceration, a vicarious form of justice involvement. In these studies, evidence that parental incarceration is associated with fewer friendship ties *received* appears less consistent than evidence for fewer ties *extended* (Bryan 2017; Cochran et al. 2018). In contrast, we found evidence for rejection and withdrawal. The difference in findings may be due to our sociocentric network approach, or it may have to do with perceptibility. Youth are more likely to know about a peer's recent arrest than they are to know about the criminal history of a peer's parent. This makes arrest more stigmatizing and more likely to result in rejection. Furthermore, our results are consistent with those of Jacobsen (2020) who found evidence of rejection and withdrawal following school punishment. They also move beyond those earlier results by offering limited evidence from supplementary analyses that arrest is associated with friendship ties even when adjusting for school suspension. Unlike that earlier work, we define rejection and withdrawal in terms of a lower likelihood of any friendship ties forming rather than discontinuity in already-established ties. A next step would be to identify the extent to which declines in friendship following arrest are driven by a deterioration of ties versus a lower likelihood of new ties, as well as the roles of potential drivers, such as a parent's perceptions of their child's peers, the youth's formal and informal supervision, and stereotypes.

In regards to homophily, we found an initial association between similarity in arrest status and extending friendship ties. The direction of this association, however, reversed from positive (tie more likely when both peers are arrested) to negative (tie less likely when both peers are arrested) when we accounted for network processes and other covariates. Results suggested that the odds of nominating an arrested peer were about 9% lower for students who ever reported an arrest and 20% lower following the first-reported arrest. Thus, the tendency for arrested individuals to name others who have been arrested as friends appears to be explained by the other friendship selection mechanisms included in the model rather than a direct preference for similarity on arrest status—a finding that seems inconsistent with Goffman's (1963) concept of "sympathetic others." Recent qualitative research offers insight into the social processes that may explain a lower likelihood of extending ties to arrested peers following arrest. In particular, Fader (2021) finds that men

in neighborhoods that are heavily impacted by mass incarceration engage in “network avoidance,” or withdrawal from certain individuals who may increase their risk of “drama” or trouble with the legal system. Thus, one possibility is that even in rural communities, many of which have not escaped the reach of mass incarceration (Eason, Zucker, and Wildeman 2017), an arrest may increase a person’s perceived risk of future encounters with the police, and in an effort to avoid this, arrested youth avoid each other. Such avoidance efforts may be further compounded by probation restrictions for youth whose arrest leads to a probation (something we were not able to measure). And yet despite their preferences to evade arrested peers, other selection mechanisms such as preferences for similarity on demographic or behavioral characteristics “win out” in bringing arrested youth together. In-depth qualitative research among rural youth would help to better understand the specific social processes at play in these relationships. Another possible explanation for the negative homophily result is that even though arrest is associated with a lower likelihood of receiving ties, a disproportionate number of ties that arrested youth receive come from non-arrested peers, for whom an arrest may be a sign of excitement or maturity (Moffitt 1993). An additional finding of note was that arrest and delinquency had associations with friendship that were in opposite directions. Whereas arrest was associated with declines in friendship ties, delinquency was associated with more ties (outgoing and incoming) when controlling for arrest and other covariates. Research on the association between delinquency and friendship or peer status has produced mixed results (Rulison et al. 2014), and our findings raise questions for future research about the role of justice involvement and stigma in this relationship.

It is important to consider our findings in relation to prior research on justice involvement and social exclusion, including studies of interpersonal family ties and institutional participation. In regards to family relationships, some research has suggested that weakened ties are due to lengthy periods of separation during confinement, rather than to stigma (Massoglia et al. 2011) and that family members are more resilient than friends are to periods of incarceration (Volker et al. 2016; Whichard 2018). Our findings suggest that for rural youth, close friendships may be more susceptible to the influences of stigma than this prior research has suggested family ties are for adults. In regards to institutional exclusion, our findings raise the question as to whether changes in interpersonal ties may help explain lower institutional participation. Kirk and Sampson (2013) find little evidence that prosocial friendships explain the impacts of arrest on school dropout, but future longitudinal network analyses should assess whether peer rejection and withdrawal help explain the impacts of arrest on low educational attainment (Bernburg and Krohn 2003; Widdowson et al. 2016). Furthermore, as prior research has implied that social ties may mitigate the impacts of justice involvement on institutional exclusion in rural areas (e.g., housing, employment; Huebner, Kras, and Pleggenkuhle 2019; Sherman 2006), criminal justice stigma could weaken these supports. This would result in a “piling up” of disadvantages, further marginalizing rural youth who have been involved in the justice system, with criminogenic consequences (Sampson and Laub 1997). Such processes could explain why justice contact among rural youth is associated with criminal persistence (Johnson, Simons, and Conger 2004).



In our study, we have focused on youth in rural, racially homogenous schools, but similar processes may take place among youth in other contexts. A recent meta-analysis suggests that estimates of selection and influence on risky behaviors in PROSPER fall in the middle (in terms of size) of a distribution of estimates from peer network studies in the US and Europe (Gallupe, McLevey, and Brown 2018). Thus, it is possible that the specific processes we have described (rejection, withdrawal, homophily) are also comparable in other contexts, including in urban schools. Examining friendship preferences in urban schools would allow for the assessment of variation by key characteristics like race and geographic concentration of justice involvement. Stigma may have a weaker influence in disadvantaged urban minority communities where arrests are more prevalent (Hirschfield 2008) and where criminal justice institutions are looked upon with mistrust. Or, stigma may be amplified among youth in these areas by an accumulation of other disadvantages (Sampson and Laub 1997; Sugie and Turney 2017). In supplemental analyses, we did not find stronger effects of arrest in schools where arrest is less prevalent, but this may be because of high cultural and racial homogeneity in these schools. Indeed, an important limitation of our data is that they are under-representative of rural racial minority youth in the US. Even so, we find disparities in arrest prevalence between whites and nonwhites, consistent with prior research (Brame et al. 2014). Given that disadvantaged and racial minority youth are already more marginalized in these rural schools, our findings imply that the criminal justice system perpetuates patterns of inequality in these communities. An important next step is to test for racial heterogeneity in effects among urban and racially diverse samples of rural youth.

Our unique dataset and methodological approach provide important advancements to prior research but several limitations should be reiterated. Importantly, our findings are based on observational data; we cannot rule out the possibility that our results are upwardly biased by unobserved differences between youth who get arrested and those who do not, or that our findings are due to something other than criminal justice stigma as we have assumed (e.g., mental health stigma; Schaefer et al. 2011; Sugie and Turney 2017). Although our findings are consistent with what we would expect if arrested youth are rejected by peers or withdraw from them on the basis of the arrest, we have not measured rejection and withdrawal directly. Future research should assess whether measures of perceived rejection or intentional withdrawal (Moore and Tangney 2017) explain our findings for friendship selection. Additionally, we have only captured arrest (broadly defined) and not incarceration or other forms of justice contact. In doing so, we have focused on average estimates rather than heterogeneity in justice contact as an important first step in this area of research. Future research should use similar methods to advance understanding of other forms of justice contact not observed in PROSPER (e.g., confinement, probation) and whether effects vary with greater perceptibility among peers. This research should also assess the extent to which effects of criminal justice sanctions are driven by separation from friendships rather than stigma (Jacobsen 2020; Massoglia et al. 2011). Finally, although our reliance on self-reports of arrest was appropriate for our study because we needed to capture “unofficial arrests” as well as those that were recorded by the police (Kirk 2006), we could not distinguish between the two in our data. Future research should link administrative records with survey data in order to examine variation across arrest experiences.

Our findings are overall consistent with labeling theory (Paternoster and Iovanni 1989; Sampson and Laub 1997) and imply that police apprehension of youth should be minimized. Additionally, schools may offer evidence-based interventions for helping youth who have been involved with the justice system to access needed services (e.g., substance-use treatment, counseling; Siennick et al. 2019); however, targeting youth who have been involved in the justice system may not result in the intended effects if they continue to be stigmatized in school. Such programs might be improved if combined with efforts to reduce stigma. This could be done by promoting person-centered language when students and teachers refer to youth involved in the justice system (Denver, Pickett, and Bushway 2017) and educator-led classroom discussions centered on reducing prejudice (Killen 2019). An important challenge will be making such programs available in rural schools where resources are often scarce.

## Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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## Appendix

### Appendix

**Table A1.**

PROSPER Questionnaire Completion Rates and Attrition by Wave (Percentages)

Wave	Grade	Semester	Cohort 1 2002–2003		Cohort 2 2003–2004	
			Of Students on Roster	Of Baseline Participants	Of Students on Roster	Of Baseline Participants
1	6	Fall	80	100	69	100
2	6	Spring	80	81	81	88
3	7	Spring	83	80	84	81
4	8	Spring	84	76	87	79
5	9	Spring	86	73	86	72
6	10	Spring	82	65	82	65
7	11	Spring	78	58	76	57
8	12	Spring	74	47	85	53

Note. "Baseline" refers to Wave 1. "Participants" refers to students who completed a questionnaire.

**Table A2.**  
PROSPER Friendship Nomination Match Rates and School Transition Rates

Wave	Grade	Percent of Nominations that Matched to Name on Roster	Percent of Students who Transitioned Schools since Last Wave
2	6	84	---
3	7	86	29
4	8	83	2
5	9	77	92
6	10	73	8
7	11	71	1

Note. PROSPER. Results based on observations from 47 networks (Waves 1 and 8 excluded).

**Table A3.**  
Descriptive Statistics for Change in Friendship Networks

	Wave 2 Grade 6	Wave 3 Grade 7	Wave 4 Grade 8	Wave 5 Grade 9	Wave 6 Grade 10	Wave 7 Grade 11
Wave-specific means						
Network size						
Mean	174.750	184.813	190.813	188.208	170.208	152.583
Minimum	73.000	82.000	84.000	75.000	81.000	63.000
Maximum	352.000	407.000	406.000	415.000	353.000	320.000
Friendship ties						
Average degree	6.018	5.889	5.684	4.883	4.622	4.312
Density (%)	2.756	2.675	2.592	2.215	2.092	1.956
	Wave 2-3	Wave 3-4	Wave 4-5	Wave 5-6	Wave 6-7	
Between-wave changes						
Change in friendship ties						
Distance ( <i>n</i> of changed ties)	716.292	778.479	668.500	554.042	418.688	
Jaccard index (%)	24.238	27.050	28.069	30.963	32.504	

Note. PROSPER Waves 2 to 7. Results are based on observations from 12,524 students in 47 networks.

**Table A4.**  
Description of Control Variables and Network Processes Accounted for in the Stochastic Actor-Based Models

Control Variables	
Risky Behaviors	
Marijuana use in past month	“During the past month, how many times have you smoked marijuana (pot, reefer, weed, blunts)?” We dichotomize the responses as follows: 0=not at all, 1=at least once.
Drinking in past month	“During the past month, how many times have you had beer, wine, wine coolers, or other liquor?” We dichotomize the responses as follows: 0=not at all, 1=at least once.
Delinquency in past year	“During the past 12 months, how many times have you ...?” We dichotomize the responses (0=none or 1=at least once) and then sum across the full set of items (0 to 11 delinquent behaviors).
	<b>1</b> “taken something worth less than \$25 that didn’t belong to you”

	2	“taken something worth \$25 or more that didn’t belong to you”
	3	“beat up someone or physically fought with someone because they made you angry (other than just playing around)”
	4	“purposely damaged or destroyed property that did not belong to you”
	5	“broken into or tried to break into a building just for fun or to look around”
	6	“thrown objects such as rocks or bottles at people to hurt or scare them”
	7	“run away from home”
	8	“skipped school or classes without an excuse”
	9	“carried a hidden weapon”
	10	“avoided paying for things such as movies, rides, food, or computer services”
	11	“taken something from a store that you did not pay for?”
Sensation-seeking behavior		“How often do you do the following things?” Original responses range from “never” (1) to “always” (5), and our measure is the mean of these responses (alpha=0.78).
	1	“do what feels good, regardless of the consequences”
	2	“do something dangerous because someone dared you to do it”
	3	“do crazy things just to see the effects on others”
School		
Missed school 7+ days past year		“How many days were you absent from school last year?” Options were “None,” “1–2 days,” “3–6 days,” “7–15 days,” and “16 or more days.” We combined the upper two categories so that 0=6 days or less and 1=7 days or more, because most students (70% of observations) missed fewer than seven days.
School attachment Family		“How true is each of the following statements for you?” Options ranged from “Never true” (1) to “Always true” (5), and the final measure is the mean of these items (alpha=0.80).
	1	“I like school a lot.”
	2	“I try hard in school.”
	3	“Grades are very important to me.”
	4	“School bores me.” (reverse coded)
	5	“I don’t feel like I really belong at school.” (reverse coded)
	6	“I feel very close to at least one of my teachers.
	7	“I get along well with my teachers.”
	8	“I feel that teachers are picking on me.” (reverse coded)
Family		
Family relations		Mean composite of three standardized measures: affective qualities, parent-child activities, inductive reasoning. <i>Affective qualities</i> are measured with the item, “During the past month, when you and your [parent] have spent time talking or doing things together, how often...?” Response options range from (1) “always or almost always” to (5) “never or almost never.” Items are repeated and rephrased to refer to each parent’s behaviors toward the child and the child’s behaviors toward each parent (four separate subscales total, each given one-quarter weight). Items include:
	1	“did [she/he/you] let [you/her/him] know [she/he/you] really [care/cares] about [you/her/him]”
	2	“did [she/he/you] act loving and affectionate towards [you/her/him]”
	3	“did [she/he/you] let [you/her/him] know that [she/he/you] [appreciate/appreciates] [you/her/him], [your/her/his] ideas, or the things [you/she/he] [do/does]”
		<i>Parent-child activities</i> is mean of items from the question, “During the past month, how often did you do these things with your mom or dad?” Response options range from (1) “every day” to (5) “once during the past month.”
	1	“work on homework or a school project together”

- 2 “do something active together, like playing sports, bike riding, exercising, or going for a walk”
- 3 “talk about what’s going on at school”
- 4 “work on something together around the house”
- 5 “discuss what you want to do in the future”
- 6 “do some other fun activity that you both enjoy”

*Inductive reasoning* is a mean composite. Response options for each item range from (1) “always” to (5) “never.”

- 1 “My parents give me reasons for their decisions”
  - 2 “My parents ask me what I think before making a decision that affects me”
- “When I don’t understand why my parents make a rule for me, they explain the reason”

**Student Demographics**

Male	1=male, 0=female
White	1=white, 0=nonwhite
Free or reduced lunch	“What do you usually do for lunch on school days?” Two of the options included “I receive free lunch from school” and “I buy my lunch at school at a reduced price.” We constructed a single binary variable in which 0=no free or reduced-price lunch and 1=free or reduced-price lunch. Prior research has recommended free or reduced-price lunch only be used as an indicator of disadvantage when alternative measures are not available, because youth who are eligible for subsidized meals do not always apply for them, especially as they get older and it may be more stigmatizing (Entwisle and Astone,1994; Hauser 1994). Therefore, to minimize concerns with inconsistency in responses in later waves, we follow Osgood, Baals, and Ramirez (2018; see also Baals 2018) by relying on a cross-wave measure of the frequency of free or reduced-price lunch (using our binary indicator), based on empirical Bayes shrinkage estimates (Raudenbush and Bryk 2002; Morris 1983). This approach gives greater weight to students who reported receiving subsidized lunch at multiple waves and in later grades (when it is less common). The resulting measure represents the log odds for receiving subsidized lunch across waves, a time-stable proxy of socioeconomic disadvantage.

**Network Processes**

**School Changes**

Transition into higher-level school	Accounts for changes in overall rate of friendship choice due to transition from middle school into high school. Estimated with ego parameters but reported with structural parameters because applies to all respondents in a network at a given wave and does not vary between individuals within a network.
Smaller schools merge into larger	Accounts for changes in overall rate of friendship choice due to merging of multiple schools into single school. Estimated with ego parameters but reported with structural parameters because applies to all respondents in a network at a given wave and does not vary between individuals within a network.

**Structural Parameters**

Reciprocity	Tendency for friendship ties to be reciprocated
Transitive triplets	Nomination of friends of the respondent’s friends
Transitive reciprocated triplets	Interaction between transitive triplets and reciprocity
Indegree popularity (square root)	Tendency for students who are nominated as friends to continue receiving more nominations
Outdegree truncated (1)	Naming of at least one friend
In-in degree^(1/2) assortativity	Tendency for students who are frequently nominated to name others who are also frequently nominated
Outdegree (density)	Overall rate of friendship choice

Friendship rate parameters	Account for the number of opportunities for ties to change in the simulations between each observation. Estimates remain consistent across different model specifications, but are not of substantive interest in our study and are omitted from the results tables.
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*Notes.* PROSPER. The structural parameters derive from a larger effort to assess and improve goodness-of-fit tests for a series of less complex models. This involved comparing goodness-of-fit tests for models with different sets of structural network parameters, including those of prior studies using these data and several parameters recommended by one of the SIENA developers. We determined which configuration produced, on average, the best goodness-of-fit statistics. Overall, different configurations seem to have relatively little effect on estimates of non-structural parameters.

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**Table 1.**

## Descriptive Statistics across Friendship Networks

	Mean	SD	Min	Max
Number of Friendship Nominations				
Made (outdegree)	3.923	2.327	0.000	7.000
Received (indegree)	3.521	2.729	0.000	20.000
Arrest				
Ever reported arrest	0.135	-----	0.000	1.000
First-reported arrest	0.052	-----	0.000	1.000
Deviant Behaviors				
Marijuana use in past month	0.089	-----	0.000	1.000
Drinking in past month	0.289	-----	0.000	1.000
Delinquency in past year	1.446	2.322	0.000	11.000
Sensation-seeking behavior	2.222	1.014	1.000	5.000
School				
Missed school 7+ days in past year	0.296	-----	0.000	1.000
School attachment	3.683	0.755	1.000	5.000
Family				
Family relations	-0.082	0.429	-1.398	0.826
Student Demographics				
Male	0.487	-----	0.000	1.000
White	0.868	-----	0.000	1.000
Free or reduced lunch (Bayes)	-0.209	1.643	-1.758	3.435
N Complete student-wave observations			48,747	
N Students			12,524	

*Notes.* PROSPER Waves 2 to 7 (pooled). 47 networks. SD = standard deviation. Of completed surveys, all variables missing less than 1% of observations except for school absence (4.5%), race (2.4%) and sensation-seeking (1.6%). Free or reduced-price lunch represents the Empirical Bayes estimates of the log odds of this status. Higher values represent greater consistency across waves in having free or reduced-price lunch. Arrest is based on self-reports in past year and does not include arrests prior to sixth grade.

**Table 2.**

Stochastic Actor-Based Models: Log-Odds Coefficients (Standard Errors in Parentheses) for Associations between Arrest and Friendship Nomination (47 networks; 12,524 students)

	Ever Reported Arrest		First-Reported Arrest	
	Model 1 Arrest Parameters	Model 2 Add Controls	Model 3 Arrest Parameters	Model 4 Add Controls
Arrest				
Alter arrest (rejection)	-0.118 *** (0.023)	-0.080 ** (0.022)	-0.078 ** (0.025)	-0.157 *** (0.024)
Ego arrest (withdrawal)	-0.241 *** (0.025)	-0.111 *** (0.018)	-0.187 *** (0.029)	-0.163 *** (0.026)
Arrest similarity (homophily)	0.117 *** (0.020)	-0.093 *** (0.016)	0.085 *** (0.020)	-0.227 *** (0.017)
Risky Behaviors				
Marijuana use				
Alter marijuana use		-0.099 ** (0.026)		-0.028 (0.022)
Ego marijuana use		-0.196 *** (0.021)		-0.146 *** (0.021)
Marijuana use similarity		-0.165 *** (0.023)		-0.060 ** (0.020)
Drinking				
Alter drinking		-0.006 (0.007)		<0.001 (0.007)
Ego drinking		-0.035 ** (0.010)		-0.032 ** (0.010)
Drinking similarity		0.031 ** (0.011)		0.042 ** (0.011)
Delinquency				
Alter delinquency		0.016 *** (0.002)		0.013 *** (0.002)
Ego delinquency		-0.035 ** (0.012)		0.014 *** (0.003)
Delinquency similarity		0.266 *** (0.033)		0.245 *** (0.034)
Sensation-seeking behavior				
Alter sensation seeking		0.021 *** (0.004)		0.021 *** (0.004)
Ego sensation seeking		0.007 (0.005)		0.006 (0.005)

	Ever Reported Arrest		First-Reported Arrest	
	Model 1 Arrest Parameters	Model 2 Add Controls	Model 3 Arrest Parameters	Model 4 Add Controls
Sensation-seeking similarity		0.104 *** (0.019)		0.102 *** (0.019)
School				
Missed school 7+ days in past year				
Alter missed school		-0.026 ** (0.007)		-0.022 ** (0.007)
Ego missed school		-0.007 (0.011)		-0.001 (0.011)
Missed school similarity		0.015 ** (0.005)		0.024 *** (0.005)
School attachment				
Alter school attachment		-0.051 *** (0.005)		-0.050 *** (0.005)
Ego school attachment		0.047 *** (0.007)		0.049 *** (0.006)
School attachment similarity		0.203 *** (0.030)		0.202 *** (0.029)
Family				
Alter family relations		0.008 (0.011)		0.006 (0.011)
Ego family relations		0.061 ** (0.016)		0.059 ** (0.015)
Family relations similarity		0.041 (0.021)		0.040 (0.021)
Student Demographics				
Gender				
Alter male		0.089 *** (0.012)		0.090 *** (0.011)
Ego male		-0.152 *** (0.015)		-0.154 *** (0.015)
Male similarity		0.588 *** (0.024)		0.587 *** (0.024)
Race				
Alter white		-0.061 ** (0.005)		-0.063 ** (0.019)
Ego white		0.013 (0.022)		0.016 (0.021)
White similarity		0.073 * (0.021)		0.073 * (0.021)

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	Ever Reported Arrest		First-Reported Arrest	
	Model 1 Arrest Parameters	Model 2 Add Controls	Model 3 Arrest Parameters	Model 4 Add Controls
		(0.032)		(0.032)
Socioeconomic status				
Alter free or reduced-price lunch		0.004 (0.003)		0.003 (0.003)
Ego free or reduced-price lunch		0.005 (0.003)		0.005 (0.003)
Free or reduced-price lunch similarity		0.345 *** (0.019)		0.342 *** (0.019)
Network Processes				
School changes				
Transition to higher-level school		-0.043 * (0.017)		-0.043 * (0.017)
Smaller schools merge into larger one		-0.264 *** (0.044)		-0.263 *** (0.044)
Structural Parameters				
Reciprocity		2.487 *** (0.042)		2.487 *** (0.042)
Transitive triplets		0.747 *** (0.017)		0.746 *** (0.017)
Transitive reciprocated triplets		-0.536 *** (0.020)		-0.535 *** (0.021)
Indegree popularity (square root)		0.361 *** (0.023)		0.353 *** (0.024)
Outdegree truncated (1)		-2.888 *** (0.065)		-2.863 *** (0.065)
In-in degree^(1/2) assortativity		-0.181 *** (0.011)		-0.177 *** (0.012)
Outdegree (density)	-1.859 *** (0.058)	-2.888 *** (0.065)	-1.834 *** (0.057)	-2.863 *** (0.065)

Note. PROSPER Waves 2 to 7. Results combined across 47 networks using meta-analysis. Rate parameters not shown for parsimony.

\* p<.05;

\*\* p<.01;

\*\*\* p<.001 (two-tailed)

**Table 3.**

Stochastic Actor-Based Models: Log-Odds Coefficients (Standard Errors in Parentheses) for Associations between First-Reported Arrest and Friendship Nomination, Including Interactions with Risky Behaviors of Peers (47 networks; 12,524 students)

	Model 1 Interaction with Alter Marijuana	Model 2 Interaction with Ego Marijuana	Model 3 Interaction with Alter Drinking	Model 4 Interaction with Ego Drinking	Model 5 Interaction with Alter Delinquency	Model 6 Interaction with Ego Delinquency
Arrest						
Alter arrest (rejection)	-0.155 *** (0.023)	-0.160 *** (0.024)	-0.157 *** (0.023)	-0.164 *** (0.023)	-0.158 *** (0.024)	-0.174 *** (0.024)
Ego arrest (withdrawal)	-0.169 *** (0.026)	-0.162 *** (0.026)	-0.175 *** (0.027)	-0.165 *** (0.026)	-0.175 *** (0.027)	-0.167 *** (0.026)
Arrest similarity (homophily)	-0.227 *** (0.016)	-0.225 *** (0.017)	-0.229 *** (0.017)	-0.229 *** (0.016)	-0.232 *** (0.017)	-0.234 *** (0.017)
Risky Behaviors						
Marijuana use						
Alter marijuana use	-0.038 (0.023)	-0.034 (0.022)	-0.028 (0.022)	-0.028 (0.022)	-0.029 (0.023)	-0.027 (0.022)
Ego marijuana use	-0.149 *** (0.021)	-0.160 *** (0.020)	-0.145 *** (0.021)	-0.144 *** (0.021)	-0.145 *** (0.022)	-0.146 *** (0.021)
Marijuana use similarity	-0.064 ** (0.020)	-0.065 ** (0.020)	-0.057 ** (0.020)	-0.059 ** (0.020)	-0.059 ** (0.020)	-0.058 ** (0.020)
Drinking						
Alter drinking	-0.001 (0.007)	0.000 (0.007)	-0.003 (0.007)	-0.001 (0.007)	0.001 (0.007)	0.001 (0.007)
Ego drinking	-0.033 ** (0.010)	-0.033 ** (0.010)	-0.034 ** (0.010)	-0.033 ** (0.010)	-0.033 ** (0.010)	-0.032 ** (0.010)
Drinking similarity	0.041 ** (0.011)	0.041 ** (0.011)	0.037 ** (0.011)	0.039 ** (0.011)	0.041 ** (0.011)	0.041 ** (0.011)
Delinquency						
Alter delinquency	0.013 *** (0.002)	0.013 *** (0.002)	0.013 *** (0.002)	0.013 *** (0.002)	0.012 *** (0.002)	0.011 *** (0.002)



	Model 1 Interaction with Alter Marijuana	Model 2 Interaction with Ego Marijuana	Model 3 Interaction with Alter Drinking	Model 4 Interaction with Ego Drinking	Model 5 Interaction with Alter Delinquency	Model 6 Interaction with Ego Delinquency
Ego delinquency	0.013 *** (0.003)	0.013 *** (0.003)	0.013 *** (0.003)	0.014 *** (0.003)	0.013 *** (0.003)	0.011 ** (0.003)
Delinquency similarity	0.234 *** (0.034)	0.234 *** (0.034)	0.236 *** (0.034)	0.241 *** (0.033)	0.224 *** (0.037)	0.211 *** (0.037)
Arrest-Behavior Interactions						
Arrest-Marijuana use						
Ego arrest × Alter marijuana	0.282 *** (0.054)					
Alter arrest × Ego marijuana		0.322 *** (0.045)				
Arrest-Drinking						
Ego arrest × Alter drinking			0.144 *** (0.026)			
Alter arrest × Ego drinking				0.063 (0.036)		
Arrest-Delinquency						
Ego arrest × Alter delinquency					0.022 * (0.010)	
Alter arrest × Ego delinquency						0.034 ** (0.009)

Note. PROSPER Waves 2 to 7. Results are combined across 47 networks using meta-analysis. Rate parameters and estimates for school attendance and attachment, family relations, sensation-seeking, demographics, and network processes are not shown for parsimony.

\* p<.05;

\*\* p<.01;

\*\*\* p<.001 (two-tailed)