Online Supplemental Information for

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

Table S1. Combined 54 Networks of PROSPER at Sixth-Grade, Compared to National Data

	PROSPER		United States Small Towns and Rural Areas	
	2002-03	2003-04	2002-03	2003-04
Students per school (median)	110.000	98.500	50.000	50.000
Male	0.505	0.488	0.471	0.472
Free or reduced-price lunch	0.402	0.398	0.402[a]	0.386[a]
White	0.854	0.834	0.751	0.744
Hispanic	0.054	0.071	0.071	0.079
Black	0.034	0.035	0.110	0.109
Native American	0.006	0.008	0.025	0.024
Asian	0.013	0.011	0.012	0.013

Notes. PROSPER; includes students who ever completed a survey between Waves 2 and 7. Descriptive statistics refer to Wave 2 (spring of sixth grade). National data come from the National Center for Education Statistics and include students in all regular public schools with at least two sixth-grade students (https://nces.ed.gov/ccd/elsi/tableGenerator.aspx).
[a] Refers to percent in the school overall. Lunch status not available for sixth-grade specifically.

Youth in these networks are similar to rural students nationally but also differ in important ways. Compared to sixth-grade students in rural and small-town public schools nationally, each cohort has a similar gender composition (51% and 49% male, compared to 47% male in each cohort nationally). In addition, about 40% of each cohort receives free or reduced-price lunch. However, PROSPER has disproportionately fewer nonwhites relative to rural and small-town sixth-grade students nationally (14% and 16% in each respective cohort, compared to 22% in each cohort nationally; National Center for Education Statistics 2019). This difference appears driven by the proportion black (3% and 4%, compared to 11% in each cohort nationally). Therefore, an important limitation of our data is that they under-represent black youth in rural and small-town schools, and these adolescents may be at greatest risk of arrest.

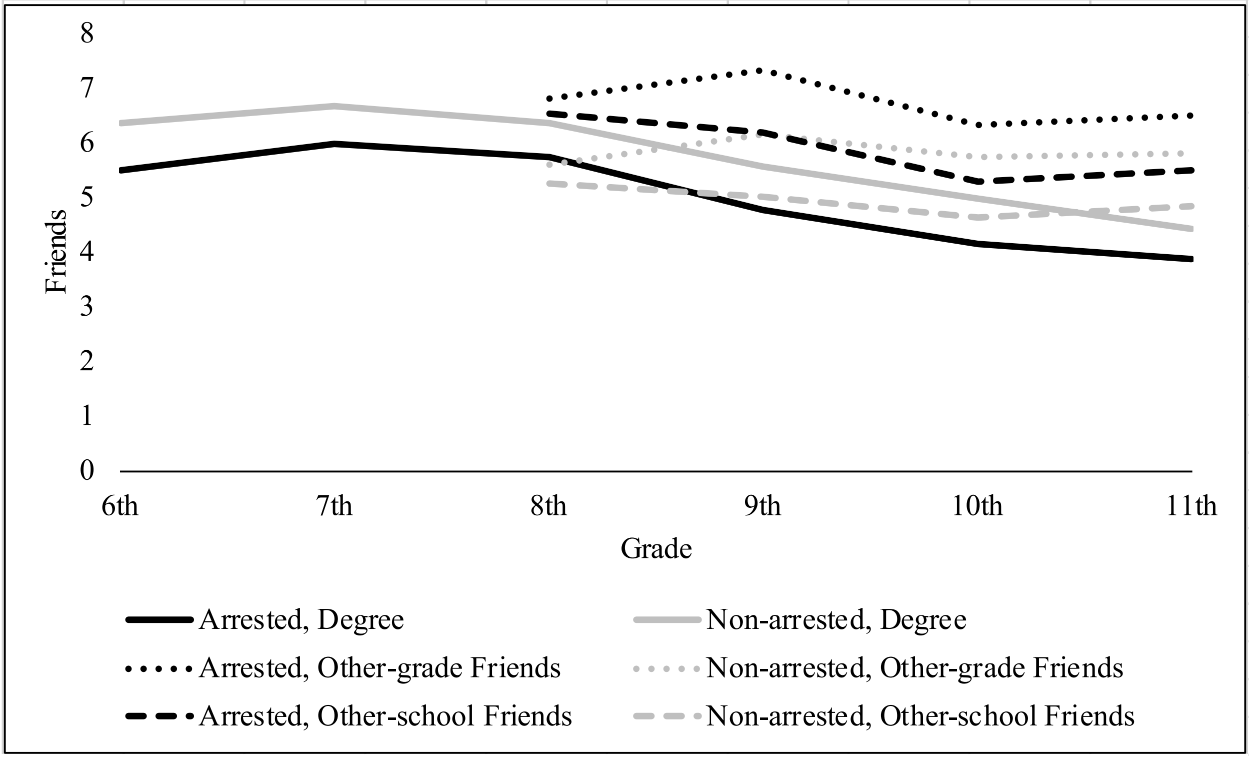

Figure S1. Average Degree and Out-of-Network Ties, by Grade
Notes. PROSPER. Results are based on observations from 47 networks, Waves 2 to 7. Arrests are self-reported; any arrests prior to Grade 6 are not captured. Independent samples t-tests at each wave indicate that all mean differences between arrested and non-arrested youth are statistically significant ($p<.001$). Data on friends in other schools was not collected prior to Wave 4 (Grade 8). N = 12,524 students.

Figure S1 shows a decline in same-grade friends over time, from a mean of 6.4 friends in sixth grade to 4.4 in eleventh grade for non-arrested youth, and from about 5.5 in sixth grade to 3.9 in eleventh grade for arrested youth. The difference between arrested and non-arrested youth is just under one friend in each grade (independent-samples t-tests; $p<.001$). Some of this difference could be due to the stigma of justice involvement, but some of it is likely also because arrested youth are already more marginalized in these networks. Indeed, youth who are at greater risk of arrest already have fewer friends. For example, in sixth grade, racial minority youth receive an average of 5.7 nominations compared to 6.4 for white youth (not shown in figure). Socioeconomically disadvantaged youth (the top 25% of the distribution of free or reduced-price lunch) receive an average of 5.2 nominations compared to 7.3 for other youth. Our focus here is on school-based friendship networks, but it is helpful to consider how these compare to out-of-network ties. For this, we examine the number of friends in other grades and schools, based on the survey items (beginning in grade 8), “How many friends do you have who go to other schools who are as close or closer to you than the friends you listed above?” and “How many friends do you have in other grades in your school who are as close or closer to you than the friends you listed above?” Arrested youth have more out-of-network friends than non-arrested youth have, a difference of more than one friend in other schools and in other grades that narrows to less than one by grade 11 (independent samples t-test each grade; $p<.001$).

Table S2. Comparing Our Measure of Arrest to Measures from Other Large Scale Surveys

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 item is more inclusive than that of the National Longitudinal Survey of Youth 1997 (NLSY97), which asks, "Have you ever been arrested by the police or taken into custody for an illegal or delinquent offense (do not include arrests for minor traffic violations)" (Brame et al. 2012). It also differs from Add Health, which reads, "Have you ever been arrested?" (Barnes et al. 2015). Unlike the NLSY97, our item does not exclude arrests for minor traffic offenses but still omits police stops that do not involve being "picked up" (e.g., being pulled over for speeding). Moreover, by avoiding words like "arrest" and "custody," we aim to minimize non-response on what is a sensitive topic for youth. A limitation of our definition is that it does not capture arrests prior to sixth grade, but arrests at early ages are very rare (0.6% of juvenile arrests are of youth younger than 10; Federal Bureau of Investigations 2020). The prevalence of arrest in our data (22% by Wave 7) is higher than a comparative estimate for rural youth in NLSY97, which puts the share at 12% (based on computing the proportion arrested before age 17 using all 17 rounds of the NLSY97; "rural" defined by residence at age 12; n=1,924 rural youth; results are weighted). It is also higher than other NLSY97 estimates not limited to rural youth (about 16% arrested by age 17), though still within upper and lower prevalence intervals (Brame et al. 2012). These discrepancies are most likely driven by differences in question wording previously mentioned; however, it is also important to note that this prevalence of 22% is a lower-bound estimate because it is does not account for the fact that students may enter or exit the study school district at any wave. Students who enter in a later grade or exit before twelfth grade are more likely to have an unknown arrest history, but unless they report an arrest during the study period, they are treated here as having no arrest history in our analyses.

Table S3. Addressing Uncompleted Surveys

In addition to the 48,747 observations with completed surveys, there were also 8,757 cases in which the student was on the school rosters and could have been nominated by a peer but was absent, refused, or had incomplete data (about one-third due to each reason). SIENA imputes these missing cases for the purpose of the analysis but excludes them from the calculation of the target statistics, so their effects on the parameter estimates are minimized (Huisman and Steglich 2008). A guideline is to have fewer than 20% of observations missing per wave when estimating models with SIENA (Ripley et al. 2019). Of our 282 network-wave observations (47 networks over six waves of data collection), 73 (26%) had more than 20% missing on at least one variable, and 53 of these had more than 20% missing on out-degree. Fourteen (5%) had more than 30% missing on at least one variable, and none had 40% or more missing. We conducted a sensitivity analysis by first creating a binary measure of whether the network was missing more than 20% of cases on at least one variable at any wave. We then included this measure as a grand mean-centered covariate in level-2 of our HLM meta-analyses that produced the aggregate estimates of alter arrest, ego arrest, and arrest similarity (main results in Table 2 of main text). None of the estimates associated with the missing data covariate achieved statistical significance at conventional alpha levels. However, the estimate for this covariate did achieve marginal significance ($b=0.102$, $SE=0.054$; $p<.10$) in the model aggregating the alter arrest estimates employing the measure of ever reporting an arrest.

Table S4. Random-Effects Linear Regression Coefficients for Between- and Within-Person Associations between Arrest by a Given Wave and Number of Friendship Nominations (Standard Errors in Parentheses)

Explanatory Variable	Nominations Made (Outdegree)		Nominations Received (Indegree)		Nominated an Arrested Peer	
	Model 1 Arrest	Model 2 Add Controls	Model 3 Arrest	Model 4 Add Controls	Model 5 Arrest	Model 6 Add Controls
Ever reported arrest (between)	-0.810 ***	-0.328 ***	-0.386 ***	0.086	0.179 ***	0.095 ***
	(0.057)	(0.048)	(0.067)	(0.060)	(0.010)	(0.010)
Ever reported arrest (within)	-1.192 ***	-0.205 ***	-1.258 ***	-0.324 ***	0.203 ***	0.059 ***
	(0.045)	(0.045)	(0.051)	(0.053)	(0.011)	(0.011)
Wave						
2 (Grade 6)		-----		-----		-----
		-----		-----		-----
3 (Grade 7)		0.087 **		0.134 ***		0.070 ***
		(0.027)		(0.032)		(0.006)
4 (Grade 8)		-0.059 *		0.103 **		0.135 ***
		(0.028)		(0.033)		(0.006)
5 (Grade 9)		-0.577 ***		-0.261 ***		0.203 ***
		(0.029)		(0.034)		(0.007)
6 (Grade 10)		-0.877 ***		-0.604 ***		0.236 ***
		(0.030)		(0.036)		(0.007)
7 (Grade 11)		-1.149 ***		-0.876 ***		0.273 ***
		(0.031)		(0.039)		(0.007)
Risky Behaviors						
Marijuana use in past month		-0.318 ***		-0.135 **		0.048 ***
		(0.036)		(0.044)		(0.008)
Drinking in past month		0.029		0.158 ***		0.046 ***
		(0.023)		(0.028)		(0.005)
Delinquency in past year		-0.016 **		0.015 *		0.006 ***
		(0.005)		(0.006)		(0.001)
Sensation-seeking behavior		0.003		0.121 ***		0.018 ***
		(0.011)		(0.013)		(0.002)
School and Family						
Missed school 7+ days past year		-0.098 ***		-0.224 ***		0.026 ***
		(0.022)		(0.026)		(0.005)
School attachment		0.314 ***		0.053 **		-0.034 ***
		(0.016)		(0.020)		(0.004)
Family relations		0.030		-0.024		-0.010
		(0.027)		(0.033)		(0.006)
Student Demographics						
Male		-0.678 ***		-0.451 ***		0.116 ***
		(0.026)		(0.034)		(0.005)
White		0.288 ***		0.152 **		-0.016 *
		(0.038)		(0.049)		(0.008)
Free or reduced lunch		-0.144 ***		-0.279 ***		0.019 ***
		(0.008)		(0.011)		(0.002)
Constant	4.567 ***	2.371 ***	4.116 ***	2.466 ***	0.358 ***	-0.043
	(0.172)	(0.153)	(0.201)	(0.196)	(0.027)	(0.031)
N Students	11,946		11,946		11,946	
N Observations	43,788		43,788		43,788	

Notes. PROSPER Waves 2 to 7. Of 48,747 completed survey observations, we drop 86 cases in which students were retained and another 4,873 with missing data on any variable. All models include dummy variables for 47 networks. Model 2 controls for indegree and whether the youth nominated an arrested peer. Model 4 controls for outdegree and whether the youth nominated an arrested peer. Model 6 controls for indegree and outdegree.
*p<.05; **p<.01; ***p<.001 (two-tailed)

In order to ensure consistency with prior research relying on standard regression approaches, we used conventional regression methods to examine the association between arrest and the number of friends in school before examining our stochastic actor-based (SAB) models. We use individual random-effects models in which the arrest variable (here, we use the ever-arrested measure) is centered within students, meaning it represents the wave-specific deviations from the student-level means of arrest, and the student-level means are included as an additional control. We also include grade as a covariate to control for changes in arrest and friendship nominations that occur as students get older, and a network indicator (dummy variables) to account for variation across combinations of school districts and cohorts. This between-within approach is similar to using individual fixed-effects regression models because it allows for the examination of within-individual associations (comparing the time when youth had been arrested to the time before their first-reported arrest) and reduces omitted-variables bias by accounting for observed and unobserved time-stable differences between arrested and non-arrested youth (Allison 2009). It also has the added benefit of allowing for the examination of between-person associations as well but is limited because it does not account for interdependency among actors within a network or the structural processes that shape the evolution of each network over time.

In Table S4, we use the ever-reported arrest measure to predict three outcomes: number of outgoing ties (outdegree) in Models 1 and 2, incoming ties (indegree) in Models 3 and 4, and a binary indicator of whether the youth nominated an arrested peer in Models 5 and 6. All estimates in Table S4 are linear regression coefficients. Models 5 and 6 are linear probability models because the outcome (nomination of arrested peer) is binary. Logit models produced coefficients that were similar in direction and statistical significance. Beginning with outdegree, the coefficients in Model 1 suggest that arrested youth nominate 0.8 fewer friends than non-arrested youth (b=-0.810, SE=0.057, p<.001). They also nominate 1.2 fewer friends in years following their arrest than they did before they reported an arrest (b=-1.192, SE=0.045, p<.001). These effect sizes are reduced considerably when control variables are added in Model 2 (between-person b=-0.328, SE=0.028, p<.001; within-person b=-0.205, SE=0.045, p<.001), but they remain moderate in size and statistically significant. The pattern for indegree is similar. Results in Model 3 suggest that arrested youth receive about 0.4 fewer nominations than non-arrested youth (b=-0.386, SE=0.067, p<.001) and that in years following their arrest, they receive 1.2 fewer nominations than they did before their arrest (b=-1.258, SE=0.051, p<.001). However, only the within-person association is robust to the addition of controls in Model 4 [b=0.086 for between-person (not significant); within-person b=-0.324, SE=0.053, p<.001]. Next, we turn to the probability of nominating an arrested peer. Results in Model 5 suggest that arrested youth are more likely than non-arrested youth to nominate an arrested peer (b=0.179, SE=0.010, p<.001). They are also more likely to nominate an arrested peer after their arrest than they were before they reported an arrest (b=0.203, SE=0.011, p<.001). These associations decline considerably with the addition of controls in Model 6, but they remain positive and significant (between-person b=0.095, SE=-0.010, p<.001; within-person b=0.059, SE=0.011, p<.001). In analyses not shown, we tested interactions between arrest and race (nonwhite) for each outcome. For indegree and outdegree, these interactions were not significant when controls were included, but results for nominating an arrested peer were larger for white youth. In another test, we limited regression analyses to youth who completed surveys at all six waves (no attrition; n=13,356, from 2,226 students). For each outcome, results were consistent in terms of direction and statistical significance to those presented in Table S4. In additional analyses, we attempted to control for school suspension. These analyses are presented in Table S5.

Table S5. Random-Effects Linear Regressions Showing Between- and Within-Person Associations between Arrest by a Given Wave and Friendship Nominations, Controlling for Suspension (Standard Errors in Parentheses)

	Nominations Made (Outdegree)		Nominations Received (Indegree)		Nominated an Arrested Peer	
Explanatory Variable	Model 1 Arrest and Controls	Model 2 Add Suspension	Model 3 Arrest and Controls	Model 4 Add Suspension	Model 5 Arrest and Controls	Model 6 Add Suspension
Ever reported arrest (between)	-0.271	-0.122	-0.585	-0.299	0.076	0.087
	(0.314)	(0.320)	(0.440)	(0.447)	(0.054)	(0.055)
Ever reported arrest (within)	-0.737 *	-0.729 *	-0.121	-0.182	0.066	0.049
	(0.293)	(0.294)	(0.363)	(0.364)	(0.069)	(0.069)
Ever reported suspension (between)	-----	-0.519 *	-----	-0.869 **	-----	-0.020
	-----	(0.202)	-----	(0.283)	-----	(0.035)
Ever reported suspension (within)	-----	-0.385	-----	0.123	-----	0.136 *
	-----	(0.234)	-----	(0.290)	-----	(0.055)
Constant	3.422 ***	3.516 ***	3.460 ***	3.640 ***	-0.107	-0.093
	(0.484)	(0.485)	(0.657)	(0.657)	(0.092)	(0.093)
N Students	689		689		689	
N Observations	2,004		2,004		2,004	

Notes. PROSPER Waves 2 to 5. Sample limited to in-home survey participants with valid data on suspension. Controls include school district, wave, risky behaviors, absence, school attachment, family relations, and student demographics. Model 2 controls for indegree and whether the youth nominated an arrested peer. Model 4 controls for outdegree and whether the youth nominated an arrested peer. Model 6 controls for indegree and outdegree.
*p<.05; **p<.01; ***p<.001 (two-tailed)

Given that school punishment may also affect friendship ties (Jacobsen, 2020), we repeat the analyses in Table S4, controlling for suspension. In PROSPER, suspension data were only collected from the subsample of youth who participated in an in-home survey portion of data collection. This in-home survey was adminstered concurrently with the first five waves of the in-school survey to a random subset of the 2003 cohort. Of the 2,267 who were invited, 979 participated (for more information on the in-home survey, see Lippold, Greenberg, & Collins, 2013). To be consistent with our larger analysis in this paper, we limit our supplemental analyses that control for suspension to in-home survey observations that are part of the larger analytic data described in the main text and for which youth provided valid suspension data (n = 2,004 observations from 689 students). To minimize underreporting, our measure of suspension is based on youth self-reports as well as mother and father reports about the number of times the child was suspended in the past year. We combine these reports into a single, binary, time-varying variable; at each wave, it is coded 0 for youth who had never reported a suspension by that wave and 1 for youth who had reported at least one suspension by that wave. Therefore, similar to our ever-arrest measure, values can only change from 0 to 1 from one wave to the next.

Results in Table S5 should be interpretted with caution when compared to those of the Table S4, which are from a much larger sample with more variation in arrest. This is because the in-home subsample is limited to early grades when arrest is less common and in-home participants are more advantaged than youth in the larger study (Jacobsen, 2020). Readers should also use caution when comparing results to those of Jacobsen (2020), which modeled discontinuity in friendship ties (likelihood of losing a tie) rather than change in the number of friends, as is presented here. Results here are consistent with those of Table S4 in terms of the direction of coefficients, but only coefficinets for the association of arrest with outdegree are statistically signficant after the inclusion of control variables. Suspension appears to explain very little of the within-person association of arrest with outdegree when other controls are included.

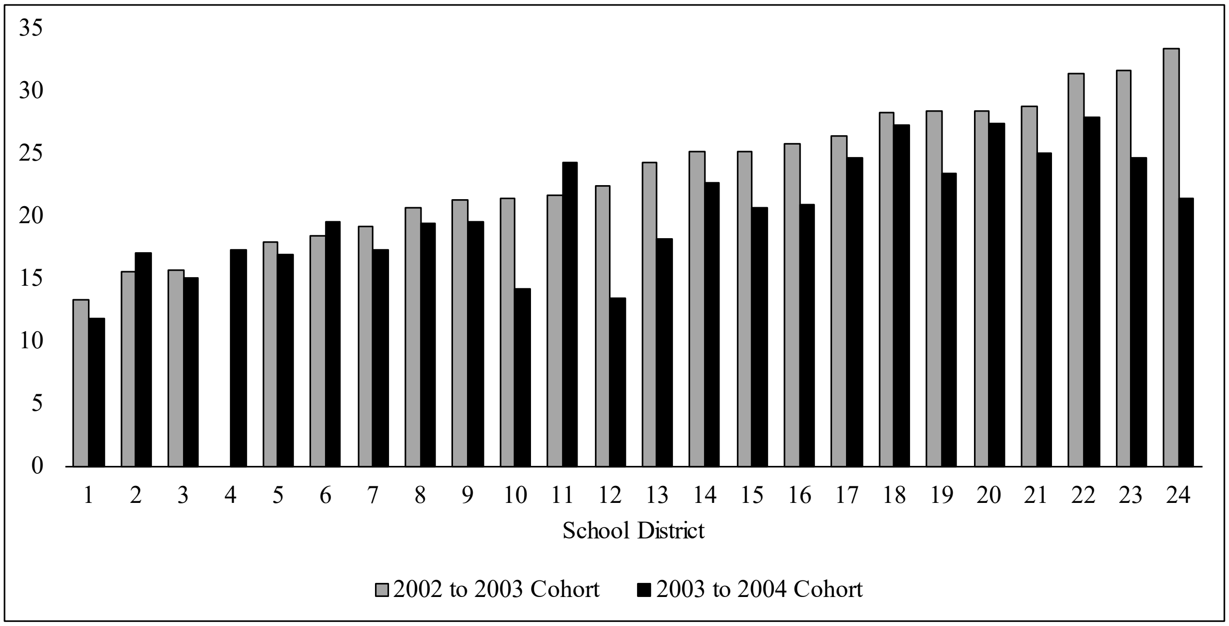

Figure S2. Percent of Students Ever Arrested between Grades 6 and 11, by Network (School District and Cohort Combination)
Notes. PROSPER. Results are based on observations from Waves 2 to 7 used in the main analyses of 47 networks (2002-2003 cohort of Network 4 is among those excluded). Arrests are self-reported (any arrests in past 12 months at each wave); arrests prior to Grade 6 are not captured.

Table S6. Stochastic Actor-Based Models with Network Covariates: Log-Odds Coefficients of Friendship Nomination (Standard Errors in Parentheses)

	Ever Reported Arrest	First-Reported Arrest
	Model 1	Model 2
Alter arrest (rejection)	-0.087***	-0.157***
	(0.021)	(0.023)
Network size	0.115*	0.047
	(0.050)	(0.055)
Network arrest rate	0.006†	-0.002
	(0.003)	(0.004)
PROSPER intervention	0.088*	0.065
	(0.034)	(0.041)
	Model 3	Model 4
Ego arrest (withdrawal)	-0.118***	-0.169***
	(0.020)	(0.024)
Network size	0.069	0.090
	(0.046)	(0.077)
Network arrest rate	0.000	-0.007
	(0.003)	(0.005)
PROSPER intervention	0.003	-0.060
	(0.039)	(0.059)
	Model 5	Model 6
Arrest similarity (homophily)	-0.100***	-0.227***
	(0.015)	(0.016)
Network size	0.077*	0.011
	(0.032)	(0.042)
Network arrest rate	0.003	-0.001
	(0.003)	(0.004)
PROSPER intervention	0.005	0.036
	(0.026)	(0.035)

Notes. PROSPER Waves 2 to 7. SE = standard error. Results are combined across 47 networks (comprised of 12,524 students) using meta-analysis. Arrest is based on self-reports in past year and does not include arrests prior to sixth grade. Parameter estimates for deviant behaviors, school attendance, school attachment, family relations, student demographics, and network processes are not shown for parsimony.

†p<.10; *p<.05; ***p<.001 (two-tailed)

If arrest is stigmatizing in rural schools, it may have a greater impact on friendship selection in smaller networks where students have less anonymity, making the arrest more perceptible to peers. It may also have stronger associations with friendship choice in networks where arrest is less prevalent. Further, approximately half of the school districts participated in the PROSPER intervention, which could potentially affect network dynamics within those schools. We tested these propositions by adding three covariates to the HLM meta-analyses that we used to combine SIENA SAB estimates from our 47 networks. The results of these models reveal whether the covariates predict differences among the network-specific SIENA estimates related to arrest. These three covariates were (1) *network size* (measured as the natural log of the number of students in the network at Wave 7), (2) *network arrest rate* (indicated by the proportion of students in the network who had ever reported an arrest by Wave 7), and (3) a binary indicator of whether the school district participated in the *PROSPER intervention*. The measures of network size and network arrest rate were both entered as level-2 (district-cohort) covariates, and the measure of intervention status was entered as a level-3 (school district) covariate. Each of these covariates was grand mean-centered within the analyses and together were entered into the meta-analyses that produced the aggregate estimates of alter arrest (rejection), ego arrest (withdrawal), and arrest similarity (homophily).

The only statistically significant estimates associated with the covariates are in the models predicting network dynamics with the measure of ever reporting an arrest; the covariates do not predict differences in network dynamics related to the first-reported arrest. Model 1 presents the aggregate estimate of alter arrest (rejection) using the measure of ever reporting an arrest. The results from this model suggest the negative association between having ever reported an arrest and being named as a friend is weaker in larger networks ($b=0.115$, $SE=0.050$; $p<.05$) and in districts that participated in the PROSPER intervention ($b=0.088$, $SE=0.034$; $p<.05$). Results in Model 5 also indicate that the negative association between arrest and extending ties to other arrested youth (arrest similarity) is weaker in larger networks ($b=0.077$, $SE=0.032$; $p<.05$). The negative association between arrest and friendship ties to arrested peers was attenuated in larger networks (but not in the year after the first-reported arrest), and we found similar results for rejection but not for withdrawal. Thus, the greater anonymity in larger schools may offer some protection from actual peer rejection, but arrested youth appear to still pull away, perhaps out of fear of being rejected.

Table S7. Random-Effects Linear Regression Coefficients for Between- and Within-Person Associations between Arrest by a Given Wave and Number of Out-of-Network Ties (Standard Errors in Parentheses)

Explanatory Variable	Friends in Other Grades		Friends in Other Schools	
	Model 1 Arrest	Model 2 Add Controls	Model 3 Arrest	Model 4 Add Controls
Ever reported arrest (between)	1.110 ***	0.581 ***	1.189 ***	0.427 ***
	(0.108)	(0.115)	(0.112)	(0.118)
Ever reported arrest (within)	0.260 *	-0.058	0.021	0.010
	(0.111)	(0.117)	(0.108)	(0.114)
Constant	5.095 ***	3.767 ***	4.313 ***	5.024 ***
	(0.304)	(0.361)	(0.319)	(0.372)
N Students	10,109		10,073	
N Observations	25,696		25,221	

Notes. PROSPER Waves 4 to 7. Samples limited to cases in which survey participants provided non-missing values on each outcome variable. Control variables not shown adjust for wave, risky behaviors, school absence and attachment, family relations, student demographics, same-grade friendship nominations, and whether the youth nominated an arrested peer. All models include dummy variables for 47 networks.
*p<.05; **p<.01; ***p<.001 (two-tailed)

In Table S7, we examine associations of arrest with out-of-network ties to examine whether arrested youth exhibit increases in friendship with youth in other grades or schools (measures for out-of-network ties are explained in the description of Figure S1). Results for friends in other grades are presented in Models 1 and 2. Results for friends in other schools are presented in Models 3 and 4. Beginning with friends in other grades, results suggest that arrested youth report having more friends in other grades than non-arrested youth have (b=1.110, p<.001 in Model 1; b=0.581, p<.001 in Model 2), but the within-person association (b=0.260, p<.05 in Model 1; b=-0.058 in Model 2) is not statistically significant. Results for friends in other schools are similar. Arrested youth report having more friends in other schools than non-arrested youth have (b=1.189, p<.001 in Model 3; b=0.427, p<.001 in Model 4), but the within-person association (b=0.021 in Model 3; b=0.010 in Model 4) is not significant.

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