



Predictors of romantic partner nomination reciprocity in adolescent social networks[☆]

Kate Vinita Fitch^a, Molly Copeland^b, jimi adams^{c,*}

^a Department of Epidemiology, Gillings School of Global Public Health, University of North Carolina Chapel Hill. 135 Dauer Drive, 2101 McGavran-Greenberg Hall, CB #7435, Chapel Hill, NC 27599, USA

^b Department of Sociology, Michigan State University, Berkey Hall, 509 E. Circle Drive Room 316, East Lansing, MI 48824, USA

^c Department of Health and Behavioral Sciences, University of Colorado Denver, Campus Box 188 P.O. Box 173364, Denver, CO 80217, USA

ARTICLE INFO

Keywords:

Adolescents
Romantic networks
Reporting agreement

ABSTRACT

Network data uniquely allow relationships to be multiply reported, creating varying rates of relationship nomination reciprocation. However, what drives such variation is unclear. Variation in reciprocation may reflect substantive information about relationships (e.g., social salience or desirability) or study design (e.g., question wording or capped nominations). We examine predictors of nomination reciprocity in romantic network data from the PROSPER study to analyze individual and dyadic predictors of nomination reciprocity. Results show higher grades predict higher reciprocity, while same-sex relationships and behaviorally discordant dyads are less likely to be reciprocal.

Introduction

Adolescent romantic relationships mark an important life course event contributing to socio-emotional development (Cavanagh, 2007; Collins, 2003; Connolly et al., 2000; Feiring, 1999). Romantic relationships are common by late adolescence, with some research estimating more than 75 % of 18-year-olds reporting a recent romantic relationship (Carver et al., 2003). Romantic partnerships also uniquely shape adolescent behavior (Kreager and Haynie, 2011; Kreager et al., 2012). However, much research focuses on partnerships reported by both individuals, with reciprocity (the joint nomination of the relationship by both partners) serving as an inclusion criterion rather than an outcome of interest (e.g., Kreager et al., 2016; Kennedy et al., 2011).

Nomination reciprocation carries important substantive and methodological implications for romantic relationship data. As such, assessing rates of nomination reciprocity (or reporting agreement)—and any potential biases in those patterns—can provide important information about the nature and quality of the data (e.g., recognizing that different populations and datasets may exhibit substantial differences in estimates of similar measures), how best to make use of those data (e.g., what rules to use for counting ties as present in subsequent data

analyses), and the potential implications of those assessments and decisions for the subsequent uses of the data. In this section, we (1) elaborate why adolescent romantic partner nomination reciprocity may exhibit unique patterns, (2) describe how previous studies have demonstrated that study design should be accounted for in assessing those patterns, and (3) recognize that these can combine in potentially unique ways across studied populations.

First, reciprocity (or lack thereof, as evidenced in pair's discordant reports) can indicate adolescents' developing definitions of romantic relationships. In early adolescence, romantic relationships are often exploratory means of status attainment or simply “for fun”, generating relational churn in early adolescence (Meier and Allen, 2009). At a first level, this could lead to lower rates of agreement about relationship status among adolescents. Personal and relationship characteristics can also shape how readily partner reports should be expected to align. For example, research in older populations has shown that intimacy within relationships improves rates of nomination reciprocity (Helleringer et al., 2011; Adams and Moody, 2007), and as adolescents age, romantic partnerships become increasingly important sources of emotional support (Smetana et al., 2006). Therefore, as adolescents' conceptualizations of romantic partnerships mature, reciprocity may increase (Carver,

[☆] The analyses used data from PROSPER, funded by grant R01 DA013709 from the National Institute on Drug Abuse, co-funded by the National Institute on Alcohol Abuse and Alcoholism, and supported by grants from the W. T. Grant Foundation (8316) and National Institute on Drug Abuse (R01 DA018225). KVF was supported by NIGMS grant number 5T34GM096958. We gratefully acknowledge input from Chris Marcum and Adam Lippert.

* Corresponding author.

E-mail addresses: katev@email.unc.edu (K.V. Fitch), jimi.adams@ucdenver.edu (adams).

Joyner, and Udry, 2003).

Second, nomination reciprocity rates vary due to study design. For example, Kreager and Haynie (2011) suggested that the low reciprocity rate for romantic nominations in the Add Health data (30 %) was likely substantially shaped by unclear question design. However, Carlson and Rose (2007) found that up to 90 % of romantic nominations were reciprocated among the oldest adolescents in their sample. Moreover, greater trust between researchers and subjects increases reciprocation rates, even for sensitive relationships (Adams and Moody, 2007). Researchers also make different choices in how to manage romantic tie data in ways that impact observed reciprocity, such as weakly or strongly symmetrizing romantic ties (e.g., Bearman et al., 2004). These patterns signify the importance of considering how study design, such as limits on the number of nominations, restrictions to nominations within grade or school, or nominating partners outside of the study, might shape reciprocity and its predictors (Card and Giuliano, 2013).

Third, these substantive and methodological elements can also combine to shape reciprocation. For example, patterns of reciprocity in platonic friendships are highly shaped by differences in social status and identities (Ball and Newman, 2013; Vaquera and Kao, 2008), and partnerships that violate social mores tended to be underreported by the more socially vulnerable partner (Helleringer et al., 2011; Nnko et al., 2004). Hesitancy to disclose potentially stigmatized relationships may decrease reciprocity of same-sex romantic nominations. Similarly, the social (un)desirability of substance use (deviance), or school adjustment (pro-sociality), or differential social status associated with these behaviors may affect reciprocity, as has been found in platonic ties (Vaquera and Kao, 2008). As a result, high individual and dyadic deviance may predict higher reciprocity, if some deviance is desirable in romantic partners and willingly reported. Alternatively, dyadic discordance on these behaviors may predict lower reciprocity, suggesting a hesitancy to report relationships with less- or more-deviant partners. Moreover, nomination caps may skew in the direction of under- (or over-) representing stigmatized (or status-biased) relationships among reports, and therefore in reciprocated nominations.

These considerations lead to our set of research questions here regarding partner nomination reciprocation by separately asking: (1) how readily adolescents agree upon romantic partner nominations, and (2) how that agreement is predicted by a set of individual and dyadic factors. While levels and predictors of partner nomination reciprocity are our research focus, we secondarily: (a) examine any alterations to these patterns when accounting for a set of controls that are known to contribute to network measurement and agreement patterns, and (b) consider how robust these patterns may be to a number of study design factors.

Primary research questions & hypotheses

In combination, the above lead us to a set of primary research questions and hypotheses, along with several suggested controls and robustness checks that should be accounted for in our assessments. As noted above, our first research question is to examine:

RQ1. How readily do adolescents agree about romantic partner nominations?

Here, we have no specific hypotheses, both because of limited studies that would have directly comparable expectations, but also because in previous work, nomination reciprocity rates vary widely. As noted above, some landmark research observes what some have considered low rates of reporting agreement (e.g., Killworth and Bernard, 1976) though little of this work actually focuses on adolescents, while others have demonstrated that these improve with the intimacy of the relationship type (Adams and Moody, 2007). As such, this question primarily serves to provide a baseline for understanding these data, and provides the context for our subsequent analyses.

This baseline question also requires multiple possible strategies for defining the set of relationships for which to assess nomination

reciprocity. Primarily, our focus in examining RQ1 is to assess nomination reciprocity at the level targeted by data collection—within

Table 1
Descriptive statistics.

Measure	All Grades	Grade 8	Grade 9	Grade 10	Grade 11	Grade 12
Romantic Partner Nominations	6683	2109	1603	1057	1042	872
			N			
			Mean (SD)			
Proportion of Nominations Reciprocated in Wave	0.51 (0.50)	0.46 (0.50)	0.44 (0.50)	0.55 (0.50)	0.56 (0.50)	0.63 (0.48)
Proportion of Nominations Reciprocated in Wave, Only One Nomination Received	0.56 (0.50)	0.52 (0.50)	0.49 (0.50)	0.59 (0.49)	0.59 (0.49)	0.66 (0.47)
Proportion of Nominations Ever Reciprocated	0.56 (0.50)	0.48 (0.50)	0.52 (0.50)	0.62 (0.49)	0.64 (0.48)	0.69 (0.46)
Proportion of Nominations Ever Reciprocated, Only One Nomination Received	0.61 (0.49)	0.53 (0.50)	0.56 (0.50)	0.65 (0.48)	0.67 (0.47)	0.72 (0.45)
Receiver Male	0.50 (0.50)	0.49 (0.50)	0.47 (0.50)	0.51 (0.50)	0.51 (0.50)	0.51 (0.50)
Receiver White	0.87 (0.34)	0.88 (0.33)	0.85 (0.36)	0.87 (0.34)	0.86 (0.35)	0.88 (0.33)
Receiver Free Lunch	0.19 (0.40)	0.22 (0.42)	0.21 (0.41)	0.18 (0.38)	0.18 (0.38)	0.14 (0.34)
Receiver Substance Use	1.46 (0.81)	1.31 (0.64)	1.44 (0.77)	1.46 (0.82)	1.59 (0.94)	1.69 (0.95)
Receiver School Adjustment & Bonding	3.64 (0.73)	3.65 (0.76)	3.59 (0.74)	3.63 (0.71)	3.64 (0.69)	3.70 (0.68)
Dyad Sex Agreement	0.04 (0.19)	0.01 (0.11)	0.03 (0.17)	0.05 (0.22)	0.08 (0.27)	0.05 (0.22)
Dyad White Agreement	0.83 (0.38)	0.83 (0.37)	0.82 (0.38)	0.85 (0.36)	0.82 (0.39)	0.83 (0.38)
Dyad Free Lunch Agreement	0.73 (0.45)	0.68 (0.47)	0.72 (0.45)	0.74 (0.44)	0.76 (0.43)	0.79 (0.41)
Dyad Substance Use Disagreement	0.56 (0.79)	0.43 (0.70)	0.55 (0.74)	0.59 (0.80)	0.71 (0.89)	0.75 (0.88)
Dyad School Adjustment & Bonding Disagreement	0.76 (0.60)	0.77 (0.61)	0.77 (0.60)	0.76 (0.60)	0.72 (0.57)	0.77 (0.59)
School-Cohort Size	207 (105)	203 (105)	248 (118)	210 (107)	201 (100)	172 (80)
School-Cohort Proportion Male	0.50 (0.04)	0.50 (0.04)	0.50 (0.03)	0.50 (0.04)	0.50 (0.04)	0.50 (0.04)
School-Cohort Proportion White	0.85 (0.10)	0.84 (0.11)	0.85 (0.09)	0.86 (0.10)	0.86 (0.10)	0.86 (0.10)
School-Cohort Treatment Condition	0.48 (0.50)	0.48 (0.50)	0.48 (0.50)	0.48 (0.50)	0.48 (0.50)	0.48 (0.50)

wave—and therefore compares reports within each wave separately, then

Table 2
Nomination reciprocation rate (Kappa).

Year	All Dyads	Reciprocation Possible	Reciprocation Possible, Single Nomination Received	Current Relationship Given Reciprocated
Grade 8	0.26	0.19 (n = 3073)	0.24 (n = 2617)	0.93 (n = 496)
Grade 9	0.29	0.26 (n = 3108)	0.32 (n = 2684)	0.92 (n = 363)
Grade 10	0.44	0.46 (n = 2686)	0.50 (n = 2508)	0.95 (n = 305)
Grade 11	0.45	0.46 (n = 2339)	0.47 (n = 2252)	0.92 (n = 304)
Grade 12	0.54	0.53 (n = 1813)	0.57 (n = 1711)	0.87 (n = 283)

aggregates those estimates across the full study period (Table 1, row 2; and Table 2, column 1).¹ However, we also sub-set these assessments to account for how two different potential biases may shape those estimates. First, since addressing our question at the level for which data were targeted (*within* wave nominations) potentially includes the same relationships multiple times across waves, we also provide an estimate of agreement across all waves at a dyad (not wave-specific) level—labeled “ever reciprocated” (see rows 5 and 6 of Table 1).² Second, since respondents were limited to only reporting one partner, but could potentially be nominated by multiple others, we also report results that limit analyses to alters receiving only one nomination to recognize the potential impact this design constraint could impose on nomination reciprocity possibilities.

Our second primary set of research questions arising from the literature noted above concerns whether there are particular factors at the individual or dyadic level that are predictors of nomination reciprocity. Primary among those we consider are:

Hypothesis 2a. : Nomination reciprocity increases with respondent age.

Hypothesis 2b. : Nomination reciprocity is higher for homophilous relationships (e.g., nodal attributes like race, and behavioral patterns like substance use or school adjustment), compared to heterophilous relationships.

Hypothesis 2c. : Nomination reciprocity is lower for same-sex relationships.

As with the caveats raised to RQ1 above, these questions are considered individually, but also collectively in a single model, and controlling for necessary design considerations in the analyses that follow.

Methods

Participants

We used data from the PROSPER (PROmoting School-community-university Partnerships to Enhance Resilience) Project. PROSPER was

¹ While the text focuses on these as potential “design effects” this follows the recognition that romantic relationships are temporally specific. So, this is not merely a question of assessing the research design’s targeted relationships, but reflecting that those reflect the theorized processes of romantic tie formation (and dissolution).

² Kreager et al found that being “friends first” did not predict later romantic relationships; this perhaps also speaks to a lack of “generalized” reporting patterns among these adolescents, suggesting the within-wave comparisons are the more salient for assessing nomination reciprocity, even given these design constraints.

a randomized controlled trial in 28 school districts in Iowa and Pennsylvania, evaluating a partnership-based delivery system for evidence-based prevention interventions. The project used a cohort sequential design with two cohorts of sixth graders in 2002. Eligible communities had 1300 to 5200 enrolled students with at least 15 % eligible for free or reduced-cost school lunch. 19,772 participating students completed surveys in school in the fall and spring of grade six, then annually in the Spring in grades seven through 12. Further details about the PROSPER data are available in prior work (Spath et al., 2004, 2011). Romantic tie nominations were elicited from students in 27 schools from grades eight through twelve, yielding 17,384 students with potential romantic partner nomination data.

Students were asked to report their “current or most recent boyfriend or girlfriend, if [they] have had any within the past year.” Names of reported romantic ties were matched to school rosters and only in-wave, in-cohort ties were considered valid nominations. Of 17,384 students, 5171 (29.7 %) ever sent and 4810 (27.7 %) ever received a valid romantic nomination *in any wave*, representing 8359 unique nominations within 5139 unique dyads. After list-wise deletion of romantic partner nominations for which recipient- or dyad-level variables were missing, our analytic sample included 6683 romantic partner nominations at any wave, (1676 lost due to missing data³) within 4094 unique dyads.⁴

Variables

Romantic partner nomination reciprocation

Our outcome was same-wave reciprocation of a received romantic partner nomination. Individuals could *receive* multiple romantic partner nominations from multiple current or previous romantic partners, but could each only *send* one nomination per wave. As such, unreciprocated ties among former partners can be valid responses and need not be respondent error. For example, consider students A and B who were in a previous relationship, where student A is now un-partnered, and student B is now romantically tied to student C. In this case, A may nominate B as their most recent partner, while students B and C validly nominate each other as their current partner. This leaves student A’s nomination unreciprocated by study design. Therefore, our analyses began by assessing possible reciprocation, then restricted these computations to students receiving only one nomination in a given wave—to determine whether findings were robust to this design.

Recipient- and dyad- level controls

Our analyses included nomination recipient-level demographic measures of: sex (female is reference), binomial white or non-white race, and socioeconomic status proxied by receipt of free or reduced-cost school lunch. We include measures of mean substance use and school adjustment, each shown in prior research to affect social life in ways that may relate to patterns of romantic partnerships (Oelsner et al., 2011; Osgood et al., 2014, 2015). Mean substance use scores aggregate past-month drinking, drunkenness, cigarette smoking, and marijuana use, with higher scores indicating greater past-month substance use (Osgood et al., 2011). School adjustment and bonding reflects the mean across items addressing belonging, enjoyment, hard work, and closeness with teachers at school, with higher scores indicating better adjustment at school (Oelsner et al., 2011).

We also included agreement on the above items at the sender-

³ The majority of cases were dropped due to missing socioeconomic data. Retention of these cases through exclusion of this variable did not change our conclusions. Chi-square tests comparing the complete (N = 8359) and analytic (6683) samples showed that the analytic sample had a slightly larger proportion of white students than the complete sample, but no other differences.

⁴ Corresponding wave-specific versions of these numbers are available in Table 1 row 1, and analytic-subsample versions are presented in Table 2.

receiver dyad level (nested within individuals). Sex, white/non-white race, and SES agreement were dichotomous, with 1 indicating agreement and 0 indicating disagreement between sender and receiver. Mean substance use and school adjustment/bonding score (dis-)agreement were continuous, calculated as the absolute difference between responses, with higher scores indicating greater difference between sender and receiver.

Analysis

We calculated kappa statistics (Cohen, 1960) for dyads to measure same-wave nomination reciprocity on the romantic relationship nominations relative to the hypothetical probability of chance reciprocity.⁵ The denominators in these calculations only include dyads for which a romantic partner nomination was sent at some point. Therefore, these comparisons represent the probability of dyad members mutually reporting a relationship with one another in the same wave given that the relationship was ever reported by at least one member.

To account for design effects that constrain reciprocation, we present three sets of kappa statistics: (1) for all valid 4094 dyads, including those not included in the analytic data set for missingness, (2) the dyads for whom reciprocation was possible because both members were present with complete data in a wave, and (3) dyads for whom reciprocation was possible *and* neither member received more than one nomination in that wave.⁶

We used multilevel generalized estimating equations, clustered at the school-cohort and nomination-recipient levels, to estimate predictors of romantic partner nomination reciprocity. While school-cohort level characteristics could be associated with reciprocation rates, none achieved standard levels of significance in ancillary analyses,⁷ so we relied on school-level clustering to account for heterogeneity across school-cohorts. Furthermore, while clustering of multi-level estimation equations are often clustered within *respondents*, here that does not make sense (since respondents could only nominate one romantic partner per wave), and therefore the second level in our GEE is *alter*-specific (and controls are thus correspondingly included for nomination *receivers*, rather than nomination senders).

Limitations

The study population and restriction of nominations to a single in-school, in-grade romantic partner present some limitations. PROSPER precludes matching out-of-school and cross-grade romantic

⁵ Kappa is a standard measure of inter-rater reliability that is appropriate for these comparisons, because rather than just reporting the percentage of agreeing reports (p_o), it accounts for the hypothetical possibility of chance agreements as well (p_e); computed as $(p_o - p_e) / (1 - p_e)$.

⁶ We also estimated kappa statistics for agreement on the “is this your current boyfriend / girlfriend?” survey item, given that the dyad members already mutually reported a relationship. Agreement was very high, indicating that when both members of a dyad report a romantic tie, it is very likely to be a current relationship.

⁷ Given the design of PROSPER, we initially estimated models that also accounted for some of its design effects’ potential contributions to patterns examined here - particularly for (1) school-cohort level differences, (2) intervention status, and (3) state contexts in various stages of preliminary models. However, we (a) did not have reasons (conceptually or from prior work) for expecting these controls to affect key associations of interest, and (b) that none of these measures demonstrated significant effects, nor (c) altered the main effects presented in the final version of models in the manuscript. Therefore, to avoid creating small cell sizes that would prohibit meaningful conclusions, presented models do not include these controls. There is one exception to this: the racial homophily estimated effect varied by the racial heterogeneity of the schools, and we therefore present a sub-analysis limiting this comparison to racially heterogeneous schools in the Appendix table, and discuss this difference briefly in the results below.

partnerships, which may have different patterns than those observed, and reciprocity may differ for such relationships. We tested our findings’ robustness to these limitations through post-hoc analyses of single-nomination recipients and a racially heterogeneous subsample, discussed below. Further, analyses cannot determine the reason for non-reciprocity, which could indicate forgetting short-term relationships, different definitions of a relationship, inability to nominate multiple concurrent relationships, or lying about a relationship. Despite these limitations, current analyses provide a unique opportunity to examine romantic partner nomination reciprocity and its predictors.

Results

Nomination reciprocation

Across all waves, 51 % of romantic partner nominations were reciprocated in the same wave as the nomination and 56 % were ever reciprocated. In general, this increased by grade, with same-wave reciprocation at 46 % in grade 8, rising to 63 % by grade 12. Roughly half of the total 6683 nominations were sent to individuals who received at least one other nomination in that wave due to multiple romantic partnerships occurring within the past year. Of the 3293 nominations sent to individuals who received no other nominations, 56 % of nominations were reciprocated in the same wave. Few (3.7 %) nominations were sent within same-sex dyads, though this increased with grade (1.1–7.6 %).⁸

Before discussing our model results, we provide individual and dyadic descriptive overviews of the nodal attributes that we then use in Table 3 to predict nomination reciprocation patterns: About 83 % of dyads matched on white/non-white. Over time, dyads became more similar in SES, with 68.4 % and 79.2 % of grades eight and 12 dyads, respectively, matching on free/reduced lunch receipt. In contrast, dyads became less similar on substance use over time. Mean absolute difference of substance use scores was 0.42 in grade eight dyads, increasing each grade to 0.75 in grade 12 dyads. School adjustment differences consistently averaged 0.76 across waves.

Factors associated with nomination reciprocity

As expected, agreement (measured via kappa statistics) increased across grades for all dyads ($n = 4094$) and when restricting to only those dyads where neither member received more than one romantic nomination in a wave (Table 2). In comparison to other data that have estimated similar nomination reciprocity rates, these are generally comparable to or higher than those previous estimates.

Table 3 presents GEE model results. Model 1 provides the base rate of nomination reciprocity (across all grades, from Table 2). Model 2 adds effects for grade-level to assess how nomination reciprocity changes across grades. Compared to nomination recipients in grade eight, recipients in grades nine through 12 were significantly more likely to reciprocate their nominations.⁹ This relationship persisted in subsequent models. Model 3 adds individual-level (receiver) predictors.

⁸ Prior work (Savin-Williams and Joyner, 2014) in other data suggest ‘jokester’ nominations of fake same-sex dyads. We have no means to judge the (differential) accuracy of reported cross- or same-sex ties. However, given changes in normative expectations over the intervening years likely reduce this possibility, and their frequency in these data seem in alignment with estimates of such relationships more broadly. As such, while we interpret these effects with caution (because of their small numbers), we have no reason to suspect they are especially subject to data inaccuracies.

⁹ Pairwise comparison of least-squares means for a linear model using grade alone as a predictor for reciprocity demonstrated that reciprocation was significantly different between all grades except between grades 8 and 9 and between grades 10 and 11.

Table 3
Models.

	Base Model	Model 1	Model 2	Model 3
Intercept	0.03 (0.04)	-0.19* (0.08)	0.36* (0.18)	0.63** (0.21)
Grade Effects [†]				
Grade 9		-0.01 (0.12)	-0.06 (0.11)	-0.03 (0.11)
Grade 10		0.46*** (0.10)	0.33** (0.11)	0.37*** (0.11)
Grade 11		0.49*** (0.12)	0.37** (0.12)	0.42*** (0.12)
Grade 12		0.79*** (0.14)	0.68*** (0.14)	0.74*** (0.14)
Receiver Controls				
Nominations Received in Wave			-0.83*** (0.07)	-0.81*** (0.07)
Sex			0.15*** (0.04)	0.12** (0.04)
Race [‡]			0.27*** (0.07)	0.19* (0.08)
SES			-0.12 (0.06)	-0.12 (0.07)
Substance Use			-0.18*** (0.04)	-0.02 (0.04)
Adjustment/Bonding			0.13*** (0.04)	0.07 (0.04)
Dyad Controls				
Sex Agreement				-0.93*** (0.18)
Race Agreement				0.12 (0.11)
SES Agreement				-0.03 (0.08)
Substance Use Disagreement				-0.27*** (0.05)
Adjustment/Bonding Disagreement				-0.23** (0.07)

†Grade Eight as reference group. ‡Non-White compared to White as reference group. *p < 0.05; **p < 0.01; ***p < 0.001.

Respondents who were male, white, scored higher on school adjustment, or scored lower on substance use were significantly more likely to reciprocate nominations. Model 4 adds dyadic agreement covariates. Nominations sent within same-sex dyads were significantly less likely to be reciprocated. Greater similarity in substance use and school adjustment significantly predicted reciprocation, while the receiver-level measures of these behaviors became nonsignificant. This pattern suggests that reciprocity is driven by behavioral similarity between dyad members rather than one member’s individual level of substance use or school adjustment.

To test the robustness of results to study design permitting only one sent nomination, ancillary analyses restricted the study sample to only those individuals who received a single nomination in a given wave. Results were consistent with Table 3 (Supplementary Table).

Given the racial homogeneity in most of the sample, we also examined robustness to individual-level and dyadic agreement on race among a subsample of the more racially heterogenous school-cohorts (range 46–73 % white), containing 1008 of the romantic partner nominations. Some effects were different within this subsample. While dyadic agreement on white/non-white race was nonsignificant in the whole sample, matching on this variable significantly increased nomination reciprocation within the more racially heterogeneous subsample. That is, the non-significance in the larger sample likely arose from the racial homogeneity in many schools, rather than no association between racial homophily and agreement. Additionally, lower SES was associated with lower reciprocation in the subsample, while receiver- and dyad-level substance use and school adjustment were nonsignificant (Supplementary Table). These results suggest that while agreement on behavior and belonging were more salient factors in romantic nomination reciprocity for the racially homogeneous sample, factors like socioeconomic status and race may have been more important for more racially diverse

subgroups.

Discussion

We sought to describe romantic partner nomination reciprocity, identify individual and dyadic predictors of reciprocity, and test robustness of these to study design limitations. Romantic partner nomination reciprocity rates in PROSPER were just above 50 % across all waves, speaking to RQ1. As expected from previous studies, grade was a significant predictor of reciprocity, with higher reciprocity in later grades than in earlier grades, in line with expectations of Hypothesis 2a. Individual-level male sex, white race, and higher school adjustment were positively associated with romantic partner nomination reciprocity, while individual-level past-month substance use was negatively associated with reciprocity. However, after including dyadic agreement on school adjustment and substance use, individual-level effects were no longer significant. Dyadic similarity on substance use and school adjustment, however, significantly predicted higher reciprocity, supporting Hypothesis 2b. This finding may suggest that dissimilarly deviant adolescents have differing definitions of romantic partnerships, shorter relationships, or face social desirability bias (not wanting to disclose relationships with differentially deviant partners). These patterns suggest that future work interested in social processes related to factors like school adjustment or deviance, such as diffusion of substance use, should consider how dyadic discordance in these behaviors or the behaviors themselves may be associated with tie reciprocity, especially when considering whether or not to include non-reciprocated nominations. For example, diffusion potential from those who use substances to those who do not may be underestimated if romantic ties in discordant romantic dyads are excluded. Additionally, same-sex dyads had significantly lower reciprocity rates, in line with Hypothesis 2c, which may also reflect social desirability bias. Future work should further explore these patterns, for example, by examining the direction of status differentials in relationships affecting reported reciprocity, as statistical power here limits further teasing apart details of stigmatized or discordant relationships.

Results were robust to the major design limitation of PROSPER permitting only a single within-school, within-grade nomination for a past-year romantic partner. Limiting our analytic models on a subset of individuals who received only one romantic partner nomination in each wave did not change conclusions, suggesting that survey constraints did not substantially drive predictors of reciprocity. However, results did differ in a sub-sample of racially heterogenous schools, with dyadic agreement on race and SES predicting reciprocity while substance use and school adjustment became nonsignificant. However, data here cannot speak to romantic relationships across grades, schools, or other social settings. Results here may underestimate non-reciprocity if out-of-grade romantic ties are generally more fragile or discordant in ways that increase non-reciprocity, which may be likely given the persistent role of schools as social foci. However, if ties that are more stigmatized or deviant are more likely to flourish outside of the social control of in-grade peers, then results here may overestimate the extent to which such factors drive non-reciprocity, especially if that could lead the more stable relationships of this nature to be outside those captured by the study’s design. Future work that is not limited to within-grade school ties should further examine reciprocity in romantic ties that reach beyond an adolescent’s grade in school, and future studies should continue to consider the role of study design, as with nomination caps or boundary restrictions, in shaping patterns of reciprocity.

Conclusions

Results suggest several developmental and methodological contributors to romantic partner nomination reciprocity, providing a foundation for more substantively in-depth future investigations. As adolescents age, reciprocity increased, in line with expected maturing

conceptualizations of romantic relationships and increased relationship duration. This suggests romantic partner nominations in older adolescents may more reliably represent enduring, mutually agreed upon romantic relationships.

However, even after controlling for grade, we observe lower nomination reciprocity between same-sex and behaviorally discordant adolescents. This pattern may indicate unwillingness to disclose potentially stigmatized relationships, suggesting that omitting non-reciprocated romantic partner nominations in adolescent social network studies may systematically exclude stigmatized relationships in ways that further marginalize youth already facing detriments to well-being, such as adolescents in same-sex romantic partnerships. This exclusion also means that omitting behaviorally discordant dyads where one partner engages in riskier behaviors may underestimate the influence of romantic partners in the diffusion of deviant behaviors, such as substance use.

Consequently, methodological decisions regarding inclusion of non-reciprocated romantic partner nominations in social network studies have important implications for the relationships captured. Studying only reciprocated nominations restricts analysis to more mature relationships between behaviorally similar adolescents in socially desirable relationships. In contrast, including non-reciprocated nominations may be noisier, but will capture relationships that are stigmatized, among younger adolescents, or behaviorally discordant. Depending on research goals, inclusion of these non-reciprocated ties may therefore be justifiable.

Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at [doi:10.1016/j.socnet.2023.10.001](https://doi.org/10.1016/j.socnet.2023.10.001).

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1 **Supplementary Table: GEE Models in Population Subsets**

	Single-Nomination Subset	Racially Heterogeneous Subset
Intercept	-0.15 (0.23)	0.03 (0.39)
Grade Effects [†]		
Grade 9	0.02 (0.14)	0.21 (0.25)
Grade 10	0.45*** (0.12)	0.76*** (0.20)
Grade 11	0.48*** (0.13)	0.55 (0.38)
Grade 12	0.81*** (0.15)	0.92** (0.33)
Receiver Controls		
Nominations Received in Wave		-0.55*** (0.12)
Sex [‡]	0.11* (0.05)	0.08 (0.13)
Race [§]	0.17* (0.09)	0.08 (0.08)
SES	-0.14 (0.08)	-0.60*** (0.14)
Substance Use	-0.04 (0.04)	-0.03 (0.20)
Adjustment/Bonding	0.07 (0.05)	0.07 (0.17)
Dyad Controls		
Sex Agreement	-1.05*** (0.20)	-1.20** (0.45)
Race Agreement	0.14 (0.11)	0.47** (0.18)
SES Agreement	-0.05 (0.09)	-0.35 (0.25)
Substance Use Disagreement	-0.25*** (0.05)	-0.03 (0.17)
Adjustment/Bonding Disagreement	-0.21** (0.07)	-0.15 (0.17)

2 [†]Grade Eight as reference group

3 [‡]Female as reference group

4 [§]Non-White compared to White as reference group

5 *p < 0.05; **p < 0.01; ***p < 0.001