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Name order effects in measuring adolescent social networks using rosters

Shuyin Liu [a], David A. Nolin [b], James A. Kitts [c,*]

[a] Department of Sociology, University of Massachusetts, 200 Hicks Way, Amherst, MA 01003-9277, USA
[b] Department of Sociology, University of Massachusetts, 200 Hicks Way, Amherst, MA 01003-9277, USA
[c] Department of Sociology, University of Massachusetts, 200 Hicks Way, Amherst, MA 01003-9277, USA

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ABSTRACT

Recent studies have found order effects in social network data collection, where later names on a roster receive fewer nominations. Some thus argue for randomizing name orders or sampling peer names for survey rosters. We model order effects as biases in nomination choices and demonstrate observational and experimental methods for assessing these biases and illuminating their mechanisms. Employing these lenses, we find little evidence of order effects on eight sociometric questions in four middle school cohorts over six waves. To inform future work, we investigate aspects of the survey situation that may amplify or attenuate order effects. Analyzing these moderating forces offers guidance for detecting, understanding, and mitigating order effects in future research.

Research in social network analysis often employs data from sociometric surveys, such as where respondents nominate their friends or interaction partners (Cillessen and Marks, 2017; Neal, 2020). To limit burden on respondents and reduce bias in recall of partners (Adams et al., 2021; Brewer, 2000), researchers often supply a roster of names and ask respondents to select their network partners. For example, adolescents may be presented with a roster of members of their class (McFarland et al., 2014), their grade in school (Kreager et al., 2016; Leszczensky and Pink, 2015), or their entire school (Paluck et al., 2016) and asked to nominate their partners on a variety of relation types from that list of names.

Researchers generally assume that network data are measured without error, and that the roster eases the burden of answering the sociometric question but does not influence the data collected. However, two recent studies (Marks et al., 2016; Poulin and Dishion, 2008) have used long alphabetical rosters for several sociometric questions in multiple middle schools and found evidence of order effects: On some questions, names earlier on the roster received more nominations than names later on the roster, suggesting that respondents quit nominating or give less attention to the task as they scan down the roster.

To mitigate these biases, some researchers have proposed randomizing the order of names (van den Berg and Cillessen, 2013), randomly selecting a subset of 30–50 names to appear on each respondent's roster, or counterbalancing roster name order (Poulin and Dishion, 2008). Randomizing name order likely increases burden on respondents, especially for longer rosters, and randomly selecting a subset of alters makes complete network data collection impossible, so this issue demands further study. If the bias is important and cannot be avoided, recent work has advocated controlling for name order in statistical analyses (Marks et al., 2016).

We note that such negative order effects could be due to respondent *fatigue*, to greater *salience* of names early on the list, to respondents *satisficing* on search efforts as they fulfill the perceived expectations of the survey researcher, or to a researcher-imposed *cap* at a maximum number of nominations (such as up to five closest friends), also known as the limited nomination method. Future research could experimentally disentangle these distinct forces, but our modest aim here is to replicate the recent finding of negative order effects, explore some potential mechanisms, and develop methods for elucidating order effects in future work.[1]

In this paper we build on the previous studies that showed name order effects for sixth graders (Poulin and Dishion, 2008) and eighth graders (Marks et al., 2016). While investigating this question of bias in a nearly ubiquitous social network data collection method, we aim to demonstrate a range of approaches for assessing this bias in future studies. Recasting this apparent bias in outcome (a correlation between a name's position on the roster and its *indegree*, or the count of its incoming nominations) as a bias in respondents' nomination behavior allows a more principled approach to modeling name order effects. It also allows future observational and experimental research on

* Corresponding author.
E-mail address: jkitts@umass.edu (J.A. Kitts).

[1] See Pustejovsky and Spillane (2009) for a discussion of many of the same psychological mechanisms, applied to question order effects.

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actor-level or structural features that may moderate name order effects.

We investigate such order effects in an empirical study of eight relations in four middle schools over six waves. Conducting an analysis similar to the earlier work, we find no evidence for systematic name order effects. There appear to be sporadic correlations between roster position and incoming nominations for particular relations, schools, and waves, but they are generally inconsistent (negative or positive), statistically non-significant, idiosyncratic, and trivial in magnitude. We also innovate by modeling order effects within rows and columns on the roster and find the same conclusions as when we model a simple linear order effect for the entire roster.

We go beyond documenting the extent of bias by exploring features of the survey context that may moderate order effects. For example, we hypothesize that exacerbating respondent burden in searching the roster will amplify order effects. To test this, we investigate rosters of varying length and also manipulate the alphabetization of the roster, by last name rather than first name. We similarly hypothesize that reducing respondent burden in searching the roster will attenuate order effects. To test this, we randomly assign half of subjects to rehearse the entire roster before responding to a sociometric question and we also assess whether negative order effects attenuate over successive waves of a longitudinal study due to increasing familiarity with the roster and sociometric questions. Despite our lack of support for the overall order effect, these extensions offer suggestions for research on order effects. Future observational and experimental studies using the proposed analytical lenses may aim to detect order effects (and distinguish authentic from spurious effects), identify their mechanisms, and ultimately help to mitigate any resulting biases.

Lastly, for future research on name order effects, we demonstrate the substantial risk of artifactual order effects due to indegree outliers, or names with many incoming nominations, when they appear near the beginning or end of a roster. This risk is greatest for shorter rosters, or for subsets of rosters (divided by roster pages or columns, or by levels of categorical covariates such as gender or race). We demonstrate methods for detecting and mitigating spurious effects.

Evidence of name order effects

Two recent studies (Marks et al., 2016; Poulin and Dishion, 2008) found evidence of a negative correlation between the position of a student's name on an alphabetized roster and the number of nominations that name received from peers. If a student was listed earlier on the roster, they received more nominations on some sociometric questions.

Those two studies examined multiple relations in multiple middle schools, with relatively long rosters (216–334 students in each school). However, they did not find name order effects in all kinds of networks. Among networks that were examined in both studies, sentiment networks (like and dislike networks) showed apparent name order effects. Students whose names were listed later in the alphabetical order received significantly fewer nominations on like and dislike questions. In the friendship network and an interaction network ("hang around with"), Marks et al. (2016) found significant negative associations between the roster position of students' names and incoming nominations, while these order effects were not significant in Poulin and Dishion (2008). In summary, these two studies provided suggestive evidence of modest negative order effects, but these were not consistent across a variety of sociometric questions or across the two studies.

The authors point out that respondents can approach sociometric questions through two distinct nominating strategies, which they call *selecting* and *scanning* (Marks et al., 2016; Poulin and Dishion, 2008). In the selecting approach, respondents begin by considering the students they wish to nominate, then select those names on the roster. There is little reason to expect a name order effect if respondents follow this strategy, as the nomination choices occur prior to looking at the alphabetized roster. In the scanning approach, the respondents read the sociometric question and then search over the roster, recruiting names to nominate as they read down the list. They may stop scanning if they reach a cap on nominations (such as five closest friends) or they may stop if they feel they have nominated enough alters or otherwise lose interest in the task. This process could lead to a diminishing propensity to nominate peers as they read down the list, or negative name order effects (Poulin and Dishion, 2008).

Elucidating name order effects as biases in behavior

Previous studies have considered order effects as a correlation between a name's position on the roster and its indegree, or the number of nominations it receives. We move beyond this by reconceptualizing order effects as a bias in respondent behavior: A name order effect represents a biased propensity to nominate alters due to their positions on a roster. This leads to our first hypothesis:

H1. *The odds of a respondent nominating an alter will diminish as that alter's name is in a later position on the roster.*

Previous work has modeled this as an effect of absolute name order, regardless of how the names are arranged in rows, columns, and pages on the survey. By modeling order effects as a linear effect of name order on incoming nominations, researchers assume that respondents read down each column from top to bottom, switching columns from left to right, and read all pages in order, and their attentiveness is unaffected by the organization of names. We also consider how arrangement of names may affect biases due to name order. In our study rosters are limited to a single page, but names are listed in multiple rows, arranged in three columns. We consider a conventional name sequence effect, but also allow that positions in rows and columns may have independent effects. For example, names near the top of the page (in any column) may be more salient and names in the first column may receive more attention. This leads to the following hypotheses:

H1a. *The odds of a respondent nominating an alter will diminish as that alter's name is in a later row on the roster page*

H1b. *The odds of a respondent nominating an alter will diminish as that alter's name is in a later column on the roster page.*

We will also consider the possibility that row and column effects interact. For example, the row effect could be strongest in the first column, where respondents who are scanning the roster lose attention rapidly, but those who are still attending to later columns are less sensitive to rows (perhaps because they are selecting, not scanning).

Modeling order effects as a bias in the behavior of respondents in dyads instead of an aggregate outcome for nominees allows richer insight into the processes generating order effects. This bias may be heterogeneous across individuals or across dyads, and it may depend on aspects of the survey or the respondent's environment, all details that are obscured in analyzing aggregate indegrees. We will also demonstrate how to experimentally investigate these moderators of order effects.

All of the mechanisms described above reflect declining effort by respondents as they scan down the roster for alters to nominate. Plausibly, these effects may be moderated by any factors that affect the burden of scanning. This leads to a second hypothesis to guide our inquiry.

H2. *Interventions that increase the burden of searching the roster will amplify negative* name *order effects.*

We can consider two special cases of this general hypothesis. First, as a short roster grows longer, the respondent will have greater opportunity to become fatigued, lose attention, give up on the task, or reach a maximum cap on nominations before reaching the end of the roster. Thus, we argue that:

H2a. *A longer roster will amplify negative* name *order effects.*

Second, organizing the roster in a non-intuitive way (such that a respondent cannot quickly find the names they seek) will increase fatigue, decrease attention, or encourage abandoning the search for names. Sorting the names randomly would certainly be non-intuitive. This practice will also increase respondents' use of scanning the roster, as it will be harder to think of alters in advance and find them on an easily ordered list. For these reasons, we argue that:

H2b. *A roster sorted in a non-intuitive way will amplify negative* name *order effects.*

Next we expect a symmetric effect in the opposite direction.

H3. *Interventions that decrease the burden of searching the roster will attenuate negative* name *order effects.*

We can consider two special cases of this general hypothesis. First, in a longitudinal study, subjects will become more familiar with their peers over time and also more familiar with the sociometric questions. As a result, the names on the roster are more cognitively available to respondents in the latter waves of the study. Having the roster more available may make the respondent more likely to employ the *selecting* strategy (thinking of their partners and then choosing them on the roster, rather than scanning the whole roster) or at least will make it easier to search the roster, and discourage satisficing. Thus, we argue that:

H3a. *Participants' practicing the roster and the nominating task over successive waves can attenuate negative* name *order effects.*

Second, requiring a respondent to read over and consider the entire roster in advance, before responding to the sociometric question, will make the list more cognitively available to the respondent, which again will encourage selecting instead of scanning. Thus, we argue that:

H3b. *Rehearsing the names on the roster before responding can attenuate negative* name *order effects.*

Research context

The remaining part of this paper reports on an empirical study of students from four middle schools in the same small city in the northeastern United States (Sirard et al., 2023). We begin at the start of 6th grade and follow students for six waves in September and December of 2018 and then March, June, September, and December of 2019. Most of this paper focuses on the first wave of data, collected 10–16 days after the first day of 6th grade, when all of the students were new to the school.

All four of the schools are public schools in the same district, but there are notable differences. The smallest is a magnet school with a performing arts theme (ARTS), a student body that is primarily female (72% female), and a 6th grade cohort of 58 students in wave 1. One is a conventional school serving an area of the city (EAST), an even mix of genders (50% female), and a 6th grade cohort of 77 students in wave 1. One is a magnet school with a theme of science and technology (TECH), a narrow majority male students (43% female), and a 6th grade cohort of 96 students in wave 1. One is a magnet school with a theme of preparing students for college (PREP), where a narrow majority of students are female (54% female) in a 6th grade cohort of 107 students in wave 1. The student population in all four schools is economically disadvantaged and racially diverse. Nearly two thirds of the students identify as Latino or Hispanic, more than one quarter identify as Black or African American, and nearly one quarter identify as White. Students were allowed to identify with more than one racial-ethnic category.

Methods

Measuring adolescent networks

Recent work (Kitts and Quintane, 2020; Kitts and Leal, 2021) has developed theoretical distinctions between social ties as 1) *role relations* or socially constructed roles defined by norms for mutual behavior (such as friend or teammate), 2) social *interactions* such as eating or playing games together, 3) social *sentiments* such as liking or disliking, and 4) *access* or availability for exchange or support (even if does not actually occur). In this paper we focus on self-reported social interaction and role relations among middle school students, with traditional roster-based sociometric survey questions. The survey also included a measure of interpersonal sentiments, where each respondent rated all peers on a scale from Strongly Dislike to Strongly Like. The sentiment question does not include conventional sociometric nominations so it is not included in the analysis of nomination biases in this paper.[2] However, the sentiment question requires respondents to rehearse the entire roster by thinking about each peer individually, which sets up a natural experiment that we will shortly describe.

We employ three kinds of social network questions. *General Interaction* questions aim to measure the total density of social interaction (outside of school and organized activities) for each dyad. These general interaction questions ask respondents to nominate peers they spend free time with *face to face* or *online* at least once per week in the approximately three-month period of each wave. *Health Behavior Interaction* questions aim to measure interaction specifically related to three categories of health behavior (i.e., shared physical activity, screen time, eating), which are defined in the survey. Whereas the two general interaction questions define a tie as any interaction at least once per *week*, the specific health behavior interaction questions employ a more liberal threshold to identify a tie, asking respondents to identify peers they interact with at least once per *month*. A third kind of network question is not a frequency of interaction behavior over a time interval, but a contemporaneous report of perceptions around the time of the survey, including role relations (friends, club co-members) and interaction (regularly sitting together at school lunch). The key question wording for the eight sociometric questions is summarized in Table 1 below:

Students answer questions through electronic tablets using the survey software Qualtrics in sessions administered at school by project staff. For each sociometric question, the content of the question is at the top, followed by a roster containing the names of all students in the same grade. Names are presented in three columns for all sociometric questions and are sorted alphabetically by students' first names, with the exception of the first survey wave at TECH, where names were listed

Table 1
The wording for the eight network questions.

General Interaction (at least once per week during the time interval)	
Face to Face	*you spent free time with (in person, but outside of clubs, teams, or classes)*
Online	*you communicated with electronically*
Health Behavior Interaction (at least once per month during time interval)	
Eat Out	*you got food with at restaurants or fast food places*
Physical Activity	*you did physical activity with*
Screen	*you did screen time together with (in the same space)*
Other (contemporaneous)	
Club	*you regularly participated with in any school or community clubs and teams*
Lunch	*you regularly sit and talk with at school lunch*
Friendship	*your friends*

[2] There may be order effects in peer evaluations, such as our sentiment scores. For example, responses to evaluation questions may grow more positive, more negative, or more neutral as respondents read down a roster. This is outside the focus of this paper as it may require different methods and different mechanisms than we consider.

alphabetically by students' last names (which sets up an experiment to be described). Students are asked to check the boxes next to the names of their grademates who fit the question description. All nominations are dichotomous (present/absent). Unlimited nominations are allowed and self-nominations are not allowed.

Analytical approach

Replicating simple name order effects of previous studies

Previous studies (Marks et al., 2016; Poulin and Dishion, 2008) have tested for name order bias by modeling the indegree of nominees as a linear function of their roster position. For simplicity, we refer to this as the linear indegree model. We use the first wave of our study data to replicate the previous analytical approach: Like the earlier work (Marks et al., 2016), we estimate a separate linear regression model for each kind of relation (Face to Face, Online, Lunch, etc.) using data pooled across multiple schools, giving 8 models and thus 8 coefficients. Following previous work, dummy variables for individual schools are used as control variables in regression models whenever data are pooled across schools. This specification allows indegree to differ across schools, but assumes that any order effect is shared by all subjects, regardless of which school they are in. We go beyond previous work in also reporting separate regression models for individual schools, giving 32 more coefficients (32 models from 8 relations x 4 schools). Thus, our basic analysis includes a total of 40 estimated coefficients for order effects. In all cases, the dependent variable is indegree, the number of nominations each student received in each relation, and the independent variable is the rank order of the name's position on the roster. The name at the top of the roster was assigned the number 1.

Modeling order effects as biases in nomination behavior

By aggregating across respondents, the linear indegree model puts the focus on the nominee. We offer an alternative approach, informed by social network analysis, that models respondent nomination choices in dyads as a function of the nominee's roster position. This shifts the focus from nominees to respondents, where we directly model the bias in nomination behavior as respondents read down the roster. For simplicity and tractability, we employ a logistic model of nomination events, modeling the log odds of a nomination as a function of roster position and other predictors of interest. We refer to this as the logistic nomination model. This method is functionally equivalent to a dyadic-independent exponential random graph model (Lubbers and Tom, 2007; Lusher et al., 2013, p. 59; Robins et al., 2007a).[3]

Our general model is as follows:

$$\ln\left(\frac{pr(nomination)}{1-pr(nomination)}\right) = \alpha + \beta P + \gamma M + \delta R + \zeta C + \eta S, \quad (1)$$

where α is a constant term, β is a vector of coefficients for a matrix P of nominees' roster positions (possibly including row and column numbers), γ is a vector of coefficients for a matrix M of nominee covariate values, δ is a vector of coefficients for a matrix R of respondent covariate values, and ξ is a vector of coefficients for a matrix C of covariate values about the nomination context (e.g., roster length, experimental manipulations of the survey design). The term ηS represents coefficients and covariate values for control variables, such as dummy variables for subgroups in pooled data (e.g., school).

This approach offers a method for modeling characteristics of respondents (R) that may affect response behavior. If a characteristic of respondents is expected to affect not just their overall nomination rate, but also their sensitivity to name order bias, then this too can be modeled with the inclusion of RxP interaction terms. Similarly, the moderating effect of an experimental treatment (for example, a manipulation of how roster names are presented or selected for individual respondents) can be modeled as a CxP interaction term (i.e., the interaction between nomination contexts and roster positions). This kind of heterogeneity among respondents, including subject-level experimental manipulations, cannot be considered when nominees' indegrees are used to assess bias, as the behavior of multiple respondents is aggregated for each nominee.

We assess statistical significance for coefficients of the logistic regression models using permutation tests rather than the standard Wald or Likelihood Ratio tests. The assumptions of parametric approaches to statistical significance are clearly violated by our data, and we anticipate that inference would be particularly problematic here due to non-independence of observations, imbalanced data (sparsity of nominations), small sample size (in this case, the number of roster positions), and influential outliers. Permutational approaches provide a principled way to assess statistical significance in the face of such challenges (LaFleur and Greevy, 2009) and have a history of fruitful application to social network data (Fredrickson and Chen, 2019; Qu et al., 2020).

In this application of permutation tests, we are most substantively concerned with addressing the problem of *indegree outliers*, 'popular' students who have many incoming nominations on a given relation. To wit, when such popular students are randomly positioned toward one end of the roster, this may lead to artifactual name order effects. This risk is greatest when rosters are short and when indegree distributions are highly skewed, both conditions that are often true for sociometric data collected by surveys.

The permutation method is premised on the assumption that names can be exchanged randomly under the null hypothesis of no name order effect. By reassigning roster positions at random and rerunning the analysis on these permuted data 100,000 times for each model, we find each p-value as the frequency that random data yield a coefficient at least as extreme (two-tailed) as the coefficient estimated for the observed data. If the coefficient based on the empirical data is more extreme than 95% of the coefficients based on permuted data, we can be more confident that it is not a fluke such as an artifact of indegree outliers appearing randomly near either end of the roster.

Results

Replicating and reconsidering simple name order effects

Following Marks et al. (2016), we run regression models to examine the effect of a name's roster position on its indegree or the total number of incoming nominations in the first wave. These results are shown in Fig. 1. The entire table summarizes 40 different regression models. Each cell represents the coefficient of the name order effect for a distinct model applied to a given type of relation (Face to Face, Online, Club, etc.) using either the pooled data (ALL) or the data from each school (ARTS, EAST, TECH, and PREP). We expect a negative association between roster position and indegree, so H1 is supported if this table is filled with significant negative coefficients (bold font). In fact, only 20 of 40 coefficients are negative (50%, the same as we would expect on an analysis of random data) and only 1 of 40 coefficients is statistically significant at the $p < 0.05$ level (where we would expect 2 coefficients to be significant even in random data).[4] Analysis of the data pooled across

[3] The assumption of dyadic independence allows these models to be estimated with maximum likelihood methods using a common logistic regression routine (Moody, 2001), whereas more complex forms of dependence typically require Markov Chain Monte Carlo methods (Robins et al., 2007b; Snijders, 2011).

[4] Rather than adjust p-values for multiple tests (e.g., with a Tukey or Bonferroni correction), we take a meta-analytic approach, comparing the number of significant coefficients identified at a fixed alpha against the number we would expect to find by chance (i.e., at $\alpha = 0.05$ we should expect about 5% of tests to be significant by chance alone under the null hypothesis).

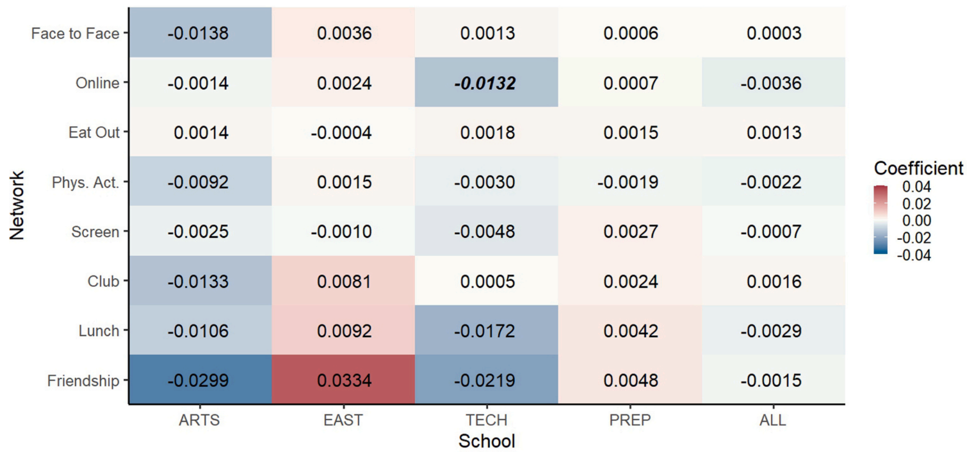

Fig. 1. Results of Linear Indegree Models. Unstandardized regression coefficients are presented in the figure. Significant coefficients (p < 0.05; two-tailed) are indicated with boldface text. Refer to Table S1 in the Appendix for additional details.

schools (ALL) further suggests that there is no discernible order effect on any relation. Beyond our failing to find an overall bias toward negative order effects, we further note that any negative or positive order effects for particular relations in particular schools are rare, weak, and idiosyncratic (i.e., do not show a systematic or intelligible pattern for any relation or within schools). Overall, following the analytical approach of previous work, we see no support for H1.

The only significant coefficient, in the Online network of TECH, is scarcely evidence for the name order effect. In any given network there is often a skewed distribution of indegrees, and it would be unrealistic to expect popular actors to evenly scatter across an alphabetical roster. Purely by chance, high-indegree nodes will occasionally fall far enough toward either end of the roster, and the resulting distribution of indegrees will produce what looks like a positive or negative order effect. Indeed, close inspection of the significant case in TECH for wave 1 suggests that it is indeed an artifact of an indegree outlier: There is a very popular student in the online relation (i.e., who has many incoming nominations for online interaction) at the beginning of the roster in that month, when the list of names is alphabetized by last name. A few other students early on the TECH roster in wave 1 also have high indegree. Notably, the apparent negative name order effect disappears in wave 2 (see Fig. 4) as the roster is alphabetized by first name and such indegree outliers are no longer at the top of the roster.

Logistic nomination models

We next reconsider name order effects as a bias in nomination behavior rather than simply a bias in the aggregated outcome and examine how respondents' nomination behavior is affected by the position of names on the roster. As described above, we employ logistic nomination models with statistical significance indicated by permutation tests.

The results of the logistic nomination models are shown in Fig. 2, and prove to be similar to the results of the linear indegree models in our replication of Marks et al. (2016) (Fig. 1). The coefficients are evenly split between positive (20) and negative (20), just as we would expect if the data were random. Only 2 of 40 coefficients (5%) are statistically significant, and they are also evenly split between positive and negative, both as we would expect by chance. When pooling the data from all four schools (ALL), we do not find any statistically significant coefficients despite the higher power afforded by the larger sample, which is additional evidence for no name order effects. Overall, it does not appear that there is evidence for H1 using the logistic nomination model as well as the linear indegree model.

Identifying row and column effect

Previous work and results shown so far have modeled order effects as a function of simple name sequence on the roster, regardless of how the names appear on the survey. This assumes that respondents scan the list from top to bottom and any marginal decrement to the log odds of nomination for each name on the list is constant from top to bottom. Going beyond previous research, we consider how the organization of names on the roster may influence any order effect. In our study, the names of students are listed in three equal columns on a single page roster for each of eight networks. The logistic nomination model is used and the roster position term (P in Eq. 1) now includes row numbers, column numbers, and their product to estimate the interaction between row and column effects. Separate models are run for each of the eight relations in each school.

In fact, we again find no evidence of order effects when roster position is modeled by row, by column, or an interaction of row and column. Despite the lack of support for order effects in our data, here we focus on these results to demonstrate a method for scrutinizing row and column effects, which may inform past and future studies that do find order effects in general. It helps to think of this question diagrammatically: If we specify a model with row and column order effects, then plot the predicted log odds of nomination on the y-axis with roster position on the x-axis, a null result would be a flat horizontal line, whereas a result with constant negative name order effects would be a straight line with a negative slope. If respondents pay more attention to the top of the survey than the bottom (regardless of column) then the line will be broken into a set of downward sloping lines, where each segment starts high with a negative slope. If there are row x column interactions, then the slopes will change from one segment to the next. For example, the slope may become less steep on segments to the right if the row order effect is strongest on the first column of names and diminishes on later columns.

Fig. 3 summarizes the predicted log-odds of being nominated based on roster positions for eight relations and four schools in wave 1, as an interactive function of row and column position, while the full set of coefficients and p-values is given in Table S3 in the Appendix. First and most importantly, note that there are no statistically significant coefficients for roster row, again revealing no evidence of significant name order effects. Even disregarding statistical significance, Fig. 3 does not show a systematic pattern of row effects across different relations or different schools. Whereas H1a (row effect) predicted that line segments should have a negative slope, Fig. 3 reveals that positive slopes are as common as negative slopes. Second, we do not see evidence for column effects. Whereas H1b (column effect) predicted that line segments in later columns should start lower than the previous segment, this pattern does not appear. Only 2 out of 80 column effect terms are significant at

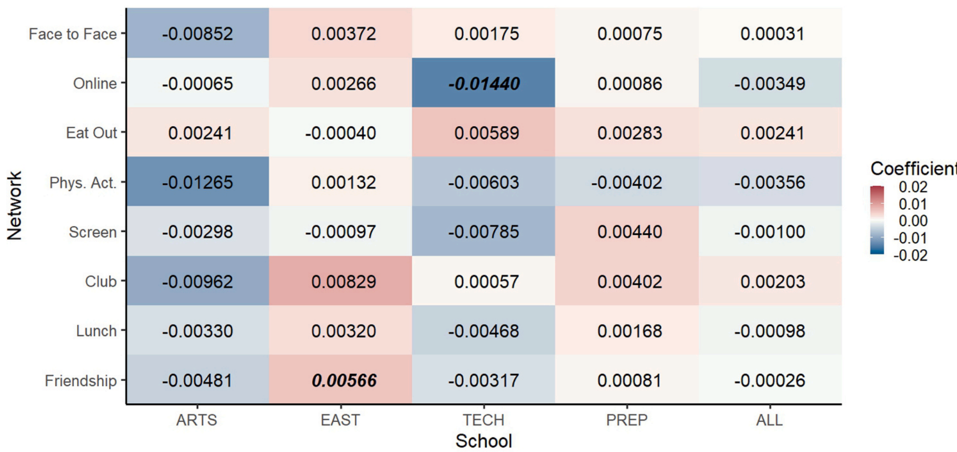

Fig. 2. Results of Logistic Nomination Models. *Note*: Coefficients are in log odds. Significant coefficients ($p < 0.05$; two-tailed) are indicated with boldface text. Refer to Table S2 in the Appendix for additional details.

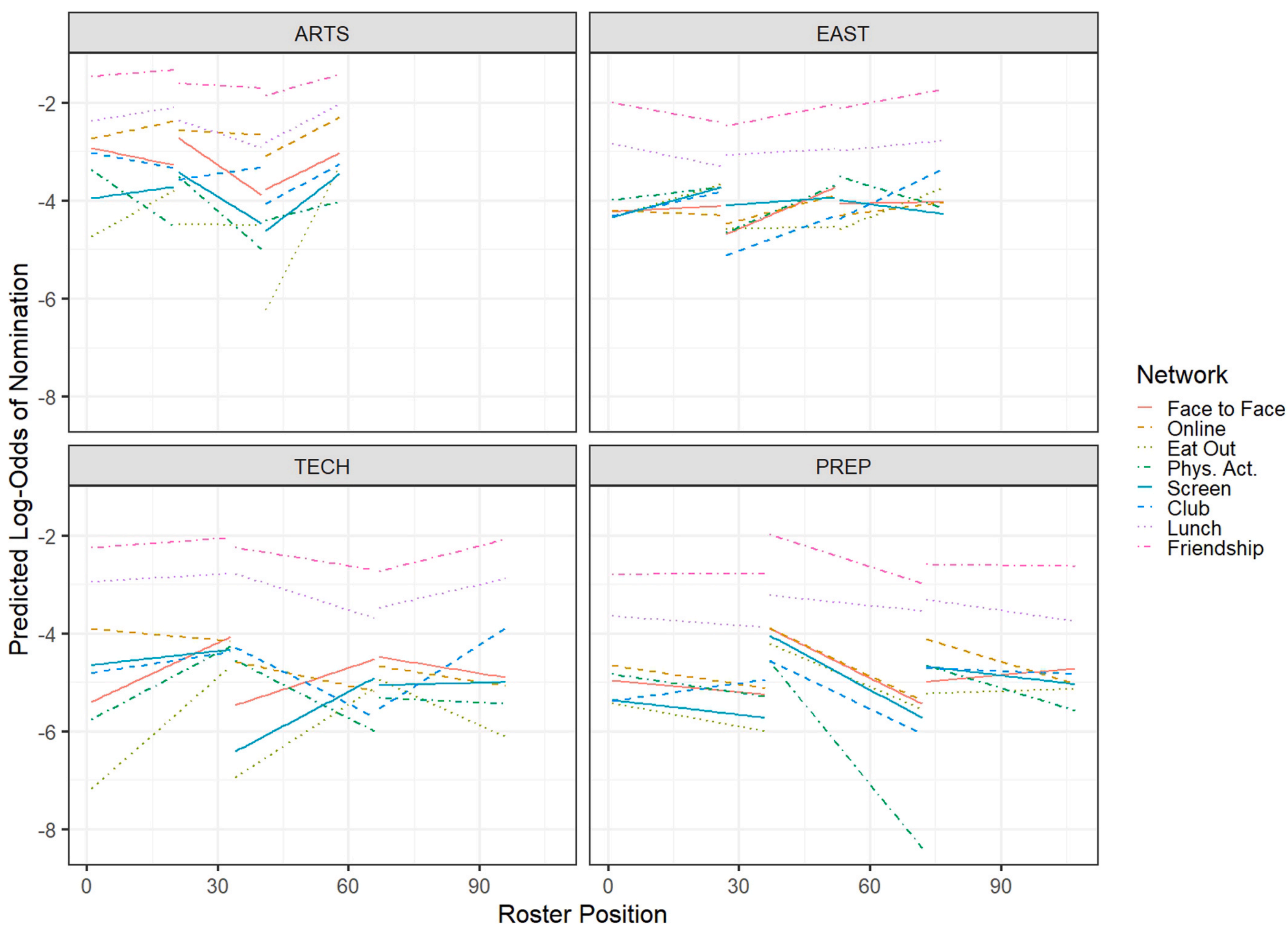

Fig. 3. Logistic Nomination Models of Row and Column Effects. Refer to Table S3 in the Appendix for coefficients and p-values.

the $p < 0.05$ level and those significant coefficients are positive as well as negative. Finally, even though line segments seem to have different slopes across columns in Fig. 3, only 1 of 80 row x column interaction terms reaches statistical significance. Overall, the results of the row and column analysis tell the same story as the results from the previous analyses based on the simple name sequence on the roster: We did not find a general tendency toward negative name order effects by row or column and also did not find any coherent pattern across relations or schools. Thus, there is no support for hypotheses H1a and H1b.

One thing we should be cautious about when interpreting Fig. 3 is that the row and column analysis is more vulnerable to artifactual order effects due to indegree outliers, compared to the analysis based on the simple name sequence. The length of each column is much shorter (about 19–35 names) compared to the entire roster and thus popular students at the top or bottom of a column will have a bigger effect on the slope of the line segment for that column. For example, one might be tempted to infer that there are negative order effects in the 2nd column in PREP when they see downward slopes on all eight relations for that column. In fact, the seemingly negative order effects in this case appear to be driven by a few popular students whose names are at the beginning of the 2nd column of the PREP roster, illustrating a problem that is particularly acute for short rosters or subsets of a roster.

Identifying mechanisms for name order effects: What amplifies or attenuates order effects?

In this section, we aim to demonstrate some novel methods for identifying causal mechanisms of negative name order effects by examining moderators of those effects. Indeed, reconsidering name order effects as biases in nomination behavior allows us to design observational and experimental studies to identify intelligible mechanisms that might exacerbate or mitigate name order effects. The test of H2 illustrates ways to examine factors that might increase the burden of searching the roster: 1) longer rosters (H2a) and 2) alphabetization by last name rather than first name on all of the rosters (H2b) in TECH at wave 1. The test of H3 demonstrates two ways to examine factors that might decrease the burden of searching the roster: 1) observing attenuation of the name order effect over successive waves, as subjects become more familiar with the sociometric questions and the roster (H3a) and 2) randomly assigning half of respondents to rehearse the entire roster just before responding to a sociometric question (H3b).

For starters, H2a implies that as respondents scan over a longer roster there should be a greater force for fatigue, salience, or satisficing and thus larger negative order effects. We will compare name order effects in four school cohorts of varying sizes – ARTS (58) < EAST (77) < TECH (96) < PREP (107) – to look for the expected interaction with roster length. We hypothesized (H2a) that the negative name order effects should grow stronger as the roster grows longer. In fact, Fig. 2 above does not support the expected interaction with roster length (H2a) over the range of roster lengths we examined (i.e., 58–107 names). In the school with the longest roster (PREP) there are no significant name order effects and seven of eight coefficients are actually positive. In the school with the shortest roster (ARTS), there are no significant name order effects and seven of eight coefficients are negative. This is a null result for the hypothesized interaction with roster length as well as the overall name order effect.

Our study also included an experimental manipulation at the school level that allows us to address H2 another way: The wave 1 roster for TECH was alphabetized by last name instead of first name, which likely increased the burden that students faced in finding their network partners on the roster because they may not know many peers' last names near the start of middle school[5]. Thus comparing the order effects on the TECH roster to the other schools in wave 1 provides an opportunity to investigate the hypothesized moderating effect of search effort on order effects (H2b). To guard against the possibility that a name order effect might be driven by other characteristics of TECH instead of a different format of alphabetization, we also look at wave 2, where all rosters were alphabetized by first name. Hypothesis H2b would be supported by this experiment if we can find that TECH has a stronger negative order effect than the other schools in wave 1 but not in wave 2.

Results of the alphabetization natural experiment are presented in Fig. 4, which compares the coefficients for name order effects across eight relations and four schools in waves 1 and 2. The boxplot figure includes two boxes for each school (one for wave 1 and one for wave 2). Each point in each box represents the name order effect coefficient for a distinct model applied to a given type of relation (Face to Face, Online, Lunch, etc.) using the data from each school (ARTS, EAST, TECH, and PREP) in each wave.

We cannot find support for H2b in Fig. 4 primarily because only 2 of the name order coefficients out of 64 are statistically significant at $p < 0.05$ (fewer than the 3 we would expect by chance), and thus the whole family of coefficients cannot be interpreted as different from zero. Even if we ignore this fact and try to interpret a visual shift in TECH from mostly negative coefficients in wave 1 to mostly positive coefficients in wave 2, we see that the same visual pattern is even greater for ARTS, where there was no manipulation of the roster alphabetization. Thus, we do not find a specific negative name order effect that is unique to TECH in wave 1, as hypothesized.

We test H3 by considering mechanisms that might decrease the burden of searching the roster for partners. First, we examined how successive waves of data collection may attenuate order effects. Under H3a, we expect that the burden of responding – and any observed order effect – will attenuate over six successive waves. As subjects become more familiar with the sociometric questions and more familiar with the roster (and also form more stable cognitive representations of their personal networks), the burden of answering these questions should diminish. As they have less trouble selecting partners on the roster, in later waves they are less likely to slack off early due to fatigue or satisficing. Thus, H3a would be supported if coefficients for name order effects are initially negative and attenuate toward zero over time.

Fig. 5 plots the name order coefficients from 192 models, where each box includes 32 coefficients from distinct models (each of 8 relations at each of 4 schools) within each wave. In Fig. 5, the coefficients in each wave are randomly distributed on both the negative and positive sides, only 6 of 192 coefficients are statistically significant at $p < 0.05$ (fewer than the 9 we would expect by chance) and 4 of those 6 are actually positive. Our finding of no name order effect in wave 1 (see Fig. 2) clearly remained true for all later waves, and we did not see the hypothesized attenuation of a negative name order effect over time. Thus we did not find support for H3a. However, future longitudinal studies with apparent negative order effects could apply the method we develop here to see if those effects attenuate over successive waves.

Our final investigation of moderating forces was a split-ballot experiment that allows a direct test of H3b. As noted previously, near the start of the survey all subjects responded to a question that required them to go over the entire roster, rating each peer on a sentiment scale from Strongly Dislike to Strongly Like. Thinking about each peer listed on the roster in this way should make them cognitively accessible and ease the burden of nominating alters on subsequent sociometric questions. Half of the respondents were randomly assigned to respond to a conventional sociometric question (Online interaction) immediately after rehearsing the whole roster in this way, and the other half responded to the Online interaction question immediately before rehearsing the roster, allowing us to assess the effect of rehearsing the roster on any order effects for the Online question. If H3b were supported, we would see a weaker name order effect on the Online question for students who rehearsed the roster directly beforehand.

We assess H3b by estimating a logistic nomination model with main effects for roster position P and for the experimental treatment (rehearsing the roster) C along with a CxP interaction term. Fig. 6 illustrates the estimated name order effects for the Online network in wave 1, while the coefficients and p-values are given in Table S4 in the Appendix. Specifically, we plot the predicted probability of nominating a peer over the range of roster positions, comparing respondents who rehearsed the roster beforehand with students who did not.

If H3b were supported by our data, we would expect to see negative name order effects in the unrehearsed group attenuate in the rehearsed group; that is, the slopes of the unrehearsed lines (dashed) in Fig. 6 should be negative, while the rehearsed lines (solid) should be flatter (i.e., less negative). However, we do not see evidence that rehearsing attenuates name order effects (H3b) as the slopes are not significantly different for rehearsed and unrehearsed groups and thus the predicted interaction does not occur for any of the four schools. Rehearsing the

[5] All analyses in this paper employ the roster name order as it was presented to subjects, which in this one case was alphabetized by last name rather than first. This natural experiment was enabled by a fortuitous error in survey programming.

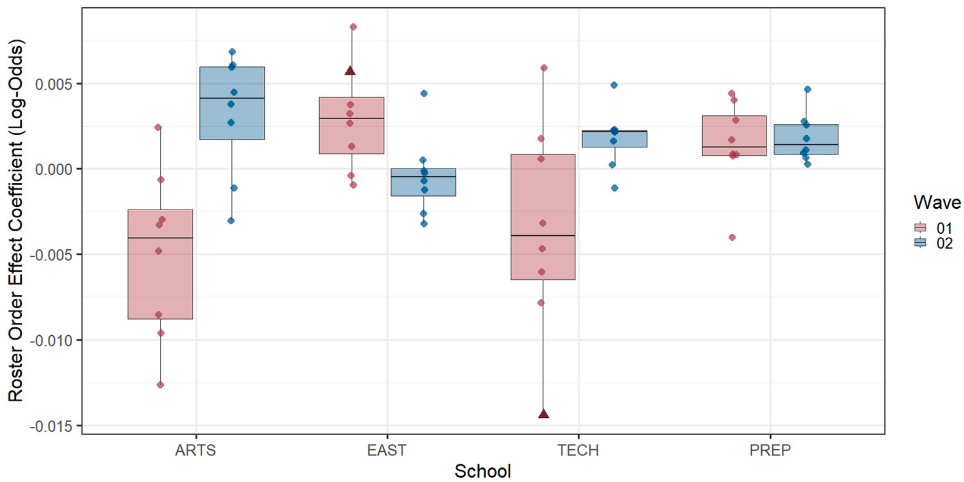

Fig. 4. Name Order Effects in All Schools in Wave 1–2. The triangles represent statistically significant coefficients ($p < 0.05$) and the circles represent non-significant coefficients. The full set of coefficients and p-values is given in Table S2 in the Appendix.

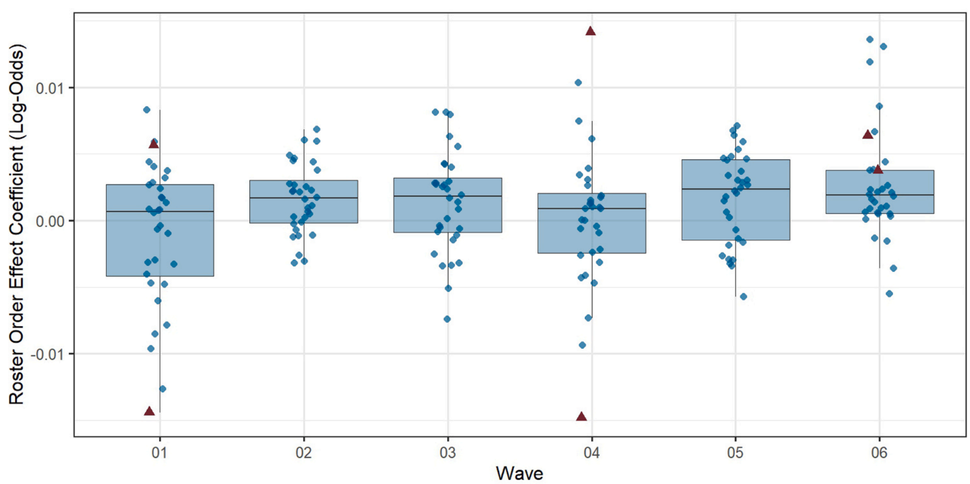

Fig. 5. Distribution of Name Order Effects across Waves. Triangles represent statistically significant coefficients ($p < 0.05$) and the circles represent non-significant coefficients. The full set of coefficients and p-values is given in Table S2 in the Appendix.

roster appears to have little effect on name order effects and we thus do not find support for H3b. Our ability to test this mechanism for name order effects with an experiment here is obviously limited by the lack of name order effects to explain. The method we demonstrate here could be used to investigate the mechanisms of a negative name order effect if it is found in future research, and also to test a method for mitigating name order biases (rehearsing the roster in advance).

Discussion

Recent research on larger school cohorts (Marks et al., 2016; Poulin and Dishion, 2008) has found significant name order effects and has recommended practices to remedy order effects in future research, such as randomizing name order and randomly selecting subsets of rosters. In many contexts, randomly selecting subsets of rosters will greatly diminish the value of the resulting network data. We further note that randomizing the order of names on a roster will actually exacerbate order effects within individuals (maximize burden by forcing respondents to scan rather than select, and encourage respondents to quit early) but will mask the order effects in the aggregate by averaging over respondents. As strategies previously suggested to mitigate this bias may raise obstacles for further research, we are particularly motivated to investigate the extent of the original problem and help us understand its mechanisms. For starters, this study responds to these recent findings by reconceptualizing order effects as biases in respondent behavior, and shows how this perspective enables observational and experimental work to illuminate explanatory mechanisms for those biases. We demonstrate these lenses here.

We have noted that negative name order effects could be due to respondent *fatigue* while scanning the list, to greater *salience* of early names, or to respondents' *satisficing* on search efforts as they feel they have nominated enough partners. If future work finds further evidence of negative name order effects, the analytical framework presented here could inform future work to experimentally investigate these mechanisms and help us understand and mitigate the biases.

Another key direction for future research is to examine how name order effects may be induced for longer rosters when respondents reach an investigator-imposed *cap* on nominations while scanning down the roster, making them unable to nominate later names. Previous work has considered whether nomination caps might introduce error by depressing nominations overall when the true number of ties exceeds the cap (Kossinets, 2006; Lee and Butts, 2018; Schaefer et al., 2011). However, future work could consider its role in inducing name order effects as well, especially when researchers believe that their respondents will be more likely to scan down the roster rather than selecting names in advance. We did not place a cap on nominations in

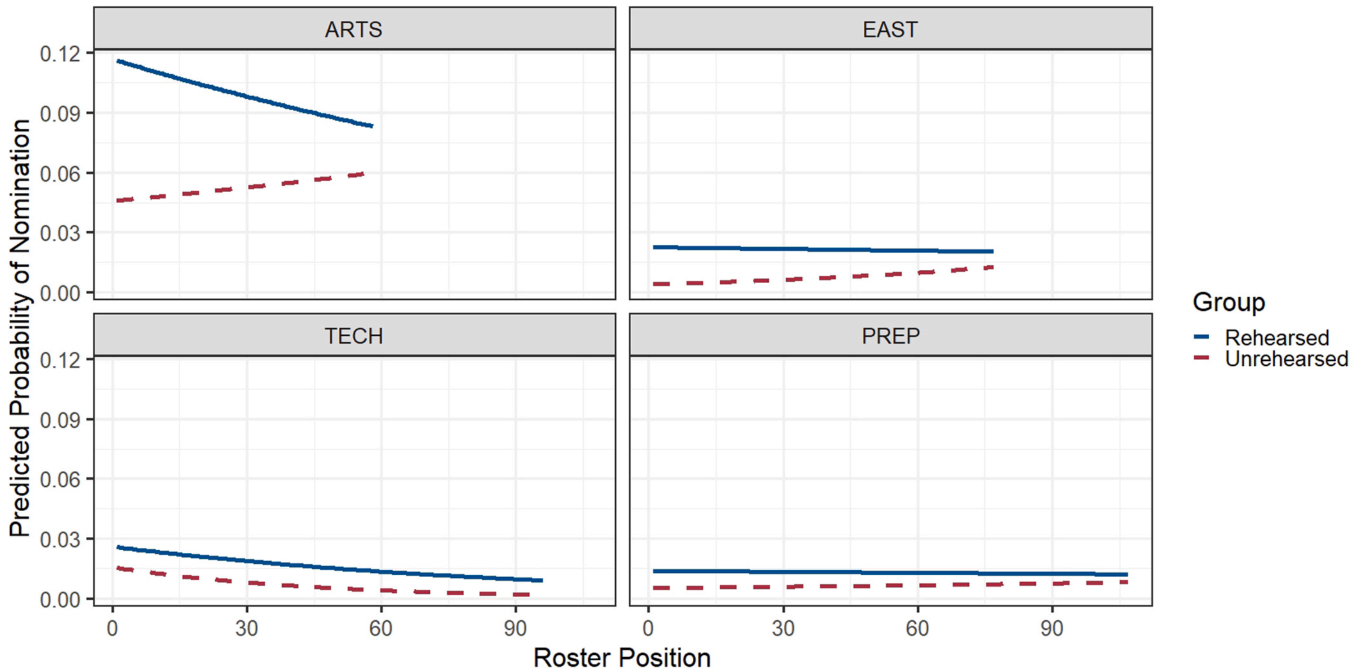

Fig. 6. Results of the Rehearsing Roster Experiment in Wave 1. Refer to Table S4 in the Appendix for coefficients and p-values.

this study, so a cap could not have produced negative order effects in our study. Here we can revisit the findings of earlier name order effects and consider the possible role of nomination caps: Marks (2016) found negative order effects with a nomination cap of 5 best friends, while this was not significant for Poulin and Dishion (2008), who used a cap of 3 best friends. In the interaction network *without* a nomination cap ('hang out with'), Marks et al. (2016) found negative order effects, while this was not significant for Poulin and Dishion (2008). Both Marks et al. (2016) and Poulin and Dishion (2008) found name order effects on Like and Dislike networks, measured without nomination caps. Nomination caps do not appear to explain the variability in their findings.

A key takeaway is that we could not replicate the previous finding of negative name order effects in the schools and relations we studied. We have gone beyond this null result to discuss diagnostics for future research to explore name order effects, illuminate why they might appear, and prevent or mitigate them. First, whereas previous research on name order effects (Marks et al., 2016; Poulin and Dishion, 2008) had assumed order effects would play out homogeneously regardless of the organization of names on a roster, we considered whether order effects could differ by row and column. Although we found a null result there too, we illustrate a method for testing biases due to row and column placement that could be used in future research in other populations and for longer rosters than we used here, and could then be expanded to include decreasing attention to later roster pages.

Importantly, we developed auxiliary hypotheses that could illuminate the mechanisms of name order effects. If fatigue or satisficing were driving name order effects, then features of the survey situation that increase or decrease burden of scanning the roster could moderate any order effects. For example, increasing the burden of searching the roster should exacerbate negative order effects and decreasing the burden should ameliorate negative order effects. We used multiple observational and experimental methods to investigate these moderating forces and again found nothing but null results for our data. Null results for moderators here do not rule out the possibility that these mechanisms might offer powerful explanations where order effects *do* appear, however. Our analyses again demonstrate a wealth of tools and techniques for assessing name order effects for other relations on other rosters.

Researchers employing name rosters for network data collection in schools (and other contexts) can find some comfort in our many null results. However, we cannot suggest that our research settles the issue. Order effects in previous work were demonstrated for rosters with 216–230 names (Poulin and Dishion, 2008) and with 273–334 names (Marks et al., 2016), whereas our null results were for smaller grade cohorts of 58–107 students. Thus, although we find no evidence for an overall order effect for four schools with smaller cohorts, we cannot rule out the possibility that longer rosters could produce order effects as the previous work has found. Within the range of cohort sizes we examined, we found no evidence of an interaction with roster length that would suggest bias increasing with roster length, but that pattern may appear for even longer rosters. Previous work pooled across schools and did not consider the issue of whether roster length moderated any order effects. Future work is needed to extend our analysis to longer rosters and reconcile our findings with theirs.

Future research on order effects for longer rosters must also consider the possibility that any such order effects may disappear for even *longer* rosters. Some studies use rosters with thousands of names (e.g., Goodreau et al., 2009; Paluck et al., 2016) and in this case it seems implausible that any respondents are scanning the roster at all. Enormous rosters would force respondents to select names in advance and find them on the roster, rather than scanning over the names, so this would guard against order effects due to fatigue or satisficing. Further attempts to replicate earlier work on name order effects might begin with moderately long rosters, such as those in work by Poulin and Dishion (2008) and Marks et al. (2016), and recognize that the finding of negative order effects might apply to a relatively narrow range of roster lengths, short enough to allow scanning but long enough to engender fatigue or satisficing while scanning.

A distinct issue is whether a large target population is divided into subgroups, such as multiple grades or other subdivisions (e.g., special education, honors or experimental program). If a researcher is trying to measure ties for an entire school, they might choose to provide separate rosters for each grade hoping to make the nomination task more tractable. However, if respondents are following a selecting approach (thinking of names in advance), then dividing a roster into grades would only complicate their response process. Separating grades or other divisions may encourage scanning by making a roster seem shorter, but in the end the number of names is still the same. A researcher might compromise by providing a shorter roster for own-grade and a longer roster (or open text field for free nominations) for all-other-grades, intending for the respondent to scan the own-grade roster while still selecting the most salient ties from other grades. Any of these approaches may entail biases, including order effects, and future research is needed to explore measurement error and develop best practices if the roster is an entire school. This is beyond the scope of the present study.

Our results also sound a note of caution about interpreting any name order analysis in individual networks with relatively short rosters. In any given network, there is typically a skewed distribution of indegrees, and it would be unreasonable to expect popular actors to evenly scatter across an alphabetical roster. Purely by chance, such indegree outliers will occasionally fall far enough toward either end of the roster, and the resulting distribution of indegrees will produce what looks like a positive or negative order effect. Shorter rosters will be particularly vulnerable to spurious effects due to popular outliers near the beginning or end of the roster, and we have observed sporadic and idiosyncratic positive and negative order effects throughout this study. This is a problem of inferring a pattern from a small sample, and will also occur if a researcher reduces the number of cases by examining order effects within subcategories (e.g., using race or gender as subsets or covariates, or separating columns of the roster).

Truly documenting an order effect pattern will require not just showing a correlation or name order and indegree within a single network; researchers will ideally look across many networks (relations, waves, and populations) to explore and document the overarching patterns and underlying forces for order effects in general. This study included a wealth of 192 networks (eight relations in four schools over six waves), which afforded us an opportunity to see just how idiosyncratic these ostensible order effects can be, even when they appear significant within a single network.[6] We have thus focused on overall patterns rather than individual school-wave-relation cases. A researcher with only a single network lacks that broad perspective, and should hesitate to conclude evidence of an order effect when it may be a spurious result of popular nodes' positions on the roster.

More complicated problems also arise. For example, if we look at PREP at wave 1 and analyze boys and girls separately, we find that boys have a negative order effect and girls have a positive order effect across many relations. Before being tempted to invent a theory about gender differences in fatigue on sociometric surveys, we note that by chance the top of the alphabetical roster at this school in this wave includes more boys and the bottom includes more girls. Because adolescents gravitate toward relations with others of the same gender – the well-known pattern of gender homophily (McMillan, 2022) – the randomly uneven gender distribution on the alphabetical roster produces an apparent order effect in nominations for each gender. A researcher examining only this one case (with this random predominance of boys early in the alphabet on the roster) might have been led to believe that there was a name order effect moderated by gender, where boys are less likely to nominate their peers (i.e., a decreasing order effect) as they read down the roster and girls are more likely to nominate their peers (i.e., an increasing order effect) as they read down the roster. This example shows how *homophily* may lead to spurious order effects for individual rosters, if gender or race is associated (even by chance) with early or later position on an alphabetical roster. The modeling approach we have demonstrated offers an opportunity to control for such spurious order effects by introducing controls for the sender, receiver, sender-receiver dyad, school contexts (e.g., gender composition, ethnic-racial composition etc.), or any other factors that could influence nominations (including gender homophily).

It would be ideal to have an 'objective' measure of a network, so any order effect bias could then be estimated directly as a deviation of respondents' self-reports from ground truth. With this in mind it is tempting to include a receiver effect (actor indegree) in the nomination model to control for unobserved features that influence an individual's attractiveness to nominations in that network, serving as a proxy for ground truth. We did not do so in this study because it would effectively control for the very order effects that we aimed to investigate here, as roster position is inseparable as one of those unobserved features of a nominee that may influence its attractiveness. Future research could include specific features that may influence nodes' attractiveness to partners as covariates in a model without including indegree directly, to try to isolate and understand order effects.[7] Other work could illuminate order effects by implementing experiments on survey design, as we have done. It makes sense to pursue those strategies in a context where significant order effects do appear, so this study is only a suggestive first step toward investigating where (if anywhere) name order effects may indeed be a problem for social network research.

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Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at doi:10.1016/j.socnet.2023.07.002.

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[6] In our study, the rosters are identical for eight relations within a school-wave and the rosters are highly similar from one wave to the next within the same school, so we do not have 192 independent cases (rosters). If we had observed a negative name order effect on all of these networks, we could conclude at least that we observed it on four entirely independent rosters, and that the result was robust across eight relations and six waves. In fact, this commonality of rosters makes it even *more* notable that we observed idiosyncratic variation of estimated name order effects across schools, waves, and relations.

[7] We also recognize that it is possible to control for receiver covariates in the linear indegree model. However, the previous approach does not allow controlling the sender, sender-receiver dyad, or any other factors that could influence nomination behavior.

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