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Estimating the Effects of Friendship Networks on Health Behaviors of Adolescents

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Abstract

Researchers typically examine peer effects by defining the peer group broadly (all classmates, schoolmates, neighbors) because of the lack of friendship information in many data sources as well as to enable the use of plausibly exogenous variation in peer group composition across cohorts in the same school. This paper estimates the effects of friend's health behaviors on own health behaviors for adolescents. A causal effect of friend's health behaviors is identified by comparing similar individuals who have the same friendship opportunities because they attend the same school and make the same friendship choices, under the assumption that the friendship choice reveals information about an individual's unobservables. We combine this identification strategy with a cross-cohort, within school design so that the model is identified based on across grade differences in the clustering of health behaviors within specific friendship options. This strategy allows us to separate the effect of friends behavior on own behavior from the effect of friends observables attributes on behavior, a key aspect of the reflection problem. We use a partial equilibrium model of friendship formation in order to derive the conditions under which our identification strategy will provide consistent estimates, and the key assumption required for our strategy to be feasible is supported by the empirical patterns of across cohort variation that we observe in our data. Our results suggest that friendship network effects are important in determining adolescent tobacco and alcohol use, but are over-estimated in specifications that do not fully take into account the endogeneity of friendship selection by 15-25%.

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Introduction

Individuals in modern societies are socially connected in a multitude of ways. For example, the social networking website Facebook.com has increased its membership by 100 million users during 2009, and now there are over 500 million users worldwide. Individuals use their social networks to receive and send information as well as establish, update, and enforce social norms of behavior. Both information acquisition as well as the impacts of social norms within social networks could have large effects on the health behaviors of individuals, particularly adolescents, who are particularly responsive to peer pressure (Brown et al. 1997). This heightening of peer influence also takes place during the developmental stage when many of the most costly health outcomes and behaviors are initiated. Our analysis will use detailed information on individual's health related behaviors and friendship networks from the National Longitudinal Study of Adolescent Health (Add Health) to examine the role of social interactions in these behaviors.

Many studies of social interactions find evidence of clustering of outcomes or behaviors above and beyond the clustering that might have been expected based on individuals' observables, including studies of crime (Glaeser, Sacerdote and Scheinkman 1996), employment (Topa 1999, Bayer, Ross, and Topa 2008), welfare usage (Bertrand, Luttmer, and Mullainathan 2000), pre-natal care (Aizer and Currie 2004), and youth health behaviors (Weinberg 2008). We also observe unexpectedly high levels of clustering on health behavior within grades of students at the same school in our data. Specifically, if we look within schools, very little variation remains across grades in student composition in terms of racial or socio-economic variables, but we observe substantial across grade variation in health behaviors for student populations that are nearly identical. The purpose of this paper is to determine whether this clustering of health behaviors into specific grades within a school can be attributed to the influence of friendship networks.

Our test for whether the social interactions between friends influences health behavior is built on the idea that individuals who make the same friendship choices are likely to be more similar overall than might be indicated by their observables. Specifically, we examine a partial equilibrium model of friendship formation and use the model to illustrate the effect of controlling for fixed effects associated with clusters of observationally equivalent individuals who face the same friendship opportunity set and make the same friendship choices. We show that if

individual students face a shock in terms of exposure to health behaviors, then as the number of friends becomes large the unobservables of individuals in the same friendship choice cluster will be identical and so a cluster fixed effect will act as a non-parametric control function for unobservable attributes that influence friendship formation and might affect health behaviors.¹ In future versions of the paper, we intend to demonstrate the properties of this identification strategy in the case of a small number of friends using monte carlo simulations.

In order to develop our empirical model of health behavior, we will rely on several empirical features of adolescent friendship networks. First, a large literature suggests that individuals exhibit strong racial, gender, and age preferences when choosing their friends—likes choose likes (Mayer and Puller 2008, Weinberg 2008). Second, data from the Add Health suggests that most friendships occur within grades, which is important for our use of cross-cohort variation in our identification strategy. Finally, as discussed above, individual grades within schools are quite homogenous over racial and socio-economic composition. Specifically, we will estimate models of youth drinking and smoking in high school that control for the share of same sex-same school-same grade friends who exhibit this behavior and fixed effects based on clusters of individuals who have the same race, ethnicity, and maternal educational attainment (individual observables), same school (same friendship opportunity set over observables), and same number of friends overall and for each racial and maternal education subgroup (same friendship choices). In our preferred specification, we will randomly choose one individual from each grade per cluster so that the model estimates are explicitly identified based on variation across cohorts within a school.

This approach is similar to earlier analyses by Dale and Krueger (2002) and Fu and Ross (2010) who use fixed effects for individuals who are equivalent on key attributes and then have the same outcome or make the same choice as a reduced form control in order to minimize bias from unobservables. However, our analysis has the advantage over these earlier studies because the identification strategy contains a clear source of exogenous variation that can create cluster-associated social interactions, namely differences in exposure to health behaviors associated with belonging to a particular cohort or grade of students.

¹ Later in the paper, we will demonstrate that these fixed effects satisfy Blundell and Dias's (2009) definition of a control function under these assumptions.

This strategy can be illustrated by the following thought exercise: consider a 9th grader and 10th grader who attend the same high school. As we show in detail below, these students face very similar friendship opportunities with respect to racial, gender, and socioeconomic composition of their same-grade classmates, and yet there is substantial clustering of health behaviors into specific cohorts within schools. Thus, if we compare two students who choose similar “types” of friends based on race, maternal education, and other demographic characteristics, there will exist substantial differences in health behaviors between the across cohort friendship opportunities, and those differences in friends’ health behaviors is arguably quasi-random. The key is that the age difference between the 9th grader and the 10th grader (who attend the same high school and have the same preferences for “types” of friends) has effectively randomized these two students into their actual friendship network.

As discussed above, under relatively straightforward assumptions concerning friendship formation, the inclusion of fixed effects for friendship choices provides a control function as the number of friends becomes large and will yield consistent estimates of the spillover effects of friend’s behavior. Further, we expect that our on-going simulations will demonstrate that even when the number of friendships is reasonably small (two to five) the reduction in bias can be substantial if friend choices are matched on multiple attributes. Most significantly, these assumptions allow us to separate the influence on individual behavior of friend’s behaviors from the influence of the observable attributes of those friends (the reflection problem) because those comparisons are made between individuals who have observationally equivalent sets of friends.

We find evidence that this strategy produces smaller “network effect” estimates than more standard models; however we still find robust evidence of network effects on smoking and drinking behavior of adolescents. Further, we find that peer health behaviors are statistically insignificant predictors of predetermined student or family attributes and the estimated coefficients in these models are much smaller than our estimates of the effect on health behaviors.

Background Literature

A large body of research across multiple disciplines has shown very strong correlations in health behaviors for individuals who are socially connected. One reason there has been so much research and policy interest in exploring how networks affect health behaviors and outcomes is the potentially large set of health interventions and policies that could be proposed to leverage

social influences on health behaviors. While the promise of using social networks to affect health is compelling, so too are the empirical issues inherent in detecting causal effects of social networks using observational data.

Four difficulties with estimating the causal effects of social networks on health are particularly important (Manski 1993). First, individuals self-select into their social network; smokers befriend smokers. Second, individuals in the same social network are simultaneously affected by their shared environment; common exposure to a smoking ban likely reduces tobacco use among all members of a social network. Third, it is difficult to separate the influence of an individual's behavior and an individual's attributes in determining the health behaviors of his or her friend. Fourth, social influences are likely reciprocal, which leads to simultaneity bias. Unfortunately, failure to overcome these empirical difficulties casts considerable doubt on the current knowledge base linking the health behaviors among individuals in the same social network. Each of these biases can lead a researcher to incorrectly infer that social networks have a causal influence on behavior. Thus, policies intended to utilize social networks to enhance interventions to reduce unhealthy behaviors could be unable to affect change if social networks do not actually have causal effects. Providing evidence of the causal mechanisms and the likely effects of policies is essential to be able to properly leverage social network effects on health behaviors.

There have been two directions that researchers have taken in estimating peer effects on health behaviors: [1] focus on broadly defined peer groups, such as all classmates in a school, in order to either (a) exploit cross-cohort population variation² in classmate composition (Bifulco et al. 2011, Fletcher 2010, 2008, Trodgon et al. 2008, Lundburg 2006, Clark and Loheac 2007) and/or (b) use instrumental variable strategies (Powell et al. 2005, Gaviria and Raphael 2001³) or [2] focus on narrowly defined peer groups, such as nominated friends, where the issues with endogeneity are thornier and the estimates are likely less credible (Trodgon et al. 2008, Christakis and Fowler 2007, 2008, Renna et al. 2008). In this paper, we seek to combine the more credible research designs from the first literature with the more credible peer group definitions of the second literature.

² See also the similar literature estimating peer effects in education outcomes (Hoxby 2000, Lavy and Schlosser 2008, Hanushek et al. 2003)

³ Instruments used in these analyses are often questionable, such as census poverty measures. Fletcher (2010) provides suggestive evidence that these instruments are invalid and proposes alternatives. Trodgon et al. (2008) and Fletcher (2010) use a combination of fixed effects and instruments.

Since we focus on friendship networks as the definition of peer group in this paper, it is necessary to outline what other researchers have done previously and how our strategy adds to the literature in this area. There have been recent examinations of the effects of social networks on obesity and smoking in the medical literature (Christakis and Fowler 2007, 2008), where “friends” are measured by the names respondents provide as potential contact sources for future survey waves. In order to control for endogeneity of friendships, Christakis and Fowler assume that including lags of the outcome for both the respondent and his/her friend is sufficient, and further they do not control for common environmental factors. Cohen-Cole and Fletcher (2008a) show that adding controls for environmental factors eliminates any detectable social network effects for obesity, and Cohen-Cole and Fletcher (2008b) show more generally that these parsimonious models will produce social network effects even in outcomes where none are expected to exist, such as for height.

Renna et al. (2008) and Trodgon et al. (2008) also focus on estimating social contagion in obesity and control for endogeneity of friendship in part by using school fixed effects. However, since substantial friendship sorting occurs within schools, school fixed effects likely do not provide a full solution to the endogeneity of friend selection, unless students select friends randomly within schools. In fact, our estimates of the influence of friends behavior using school fixed effects are notably larger than estimates using friendship cluster fixed effects suggesting that school fixed effects may not be sufficient to control for endogeneity. In addition, Renna et al. (2008) and Trodgon et al. (2008) use instruments for friends’ weight, including friends’ parents’ obesity. Trodgon et al. also uses friends’ birth weight. It is unclear whether these instruments are adequate, though, as they are observable or correlated with observables at the time of friendship selection.

Calvó-Armengol et al. (in press) and Patachini and Zenou (2010) have extended the literature by using a network fixed effects approach in their examination of peer effect in education outcomes. Adolescents are assumed to choose among mutually exclusive networks of friends. Within these networks, their best friends (based on friendship nominations) are used as the peer exposure and their model of behavior controls for network fixed effects. The maintained assumption with this approach is that adolescents endogenously choose a friendship group, but within that group, actual “best friends” are random, an assumption that is verified for

observables. Patachini and Zenou (2010) also use the outcomes of friends' friends (once removed in the network) as instruments.

All of these studies rely on information about the individual and their friends in order to identify the effect of friend's behavior. Whether identification is based on controlling for lagged outcomes, instrumenting for friends attributes or controlling for network fixed effects, all of these studies use variation across individuals who are in the same social environment and so reasonably may have contributed to that variation through their own choices. In the next section, we develop a simple model of friendship formation and demonstrate circumstances under which consistent estimates of the effect of friends' health behavior on own health behavior can be uncovered, and show that identification requires an exogenous shock in exposure to potential friends who exhibit certain health related behaviors. Following the literature on peer effects, we propose that across cohort variation within schools can provide this exogenous variation in exposure to health behaviors and demonstrate empirically that health behaviors vary substantially more across cohorts than student attributes, like race or parental education, evidence consistent with our identification strategy.

Identification Strategy

In this paper, we seek to estimate the causal effects of friends' health behaviors by overcoming the many empirical obstacles we outline above, including selection into networks, unobserved determinants of behaviors, and the joint determination of outcomes within a network. The intuition behind our approach is that we seek to form comparison groups based on information in the data that describes the friendship *options* of students as well as the students' *choices* of friends (given these options) following the premise that individuals who make similar decisions or have similar outcomes when facing the same set of options likely are very similar on both observable and unobservable attributes. The beginning of this section illustrates this intuition, the next two subsections derive formal results, and in a future draft, the final subsection will present Monte Carlo results to illustrate how our identification strategy works in practice.

We begin with a slight modification to the relatively straightforward linear-in-means model of social interactions (Manski, 1993; Moffit, 2001; Brock and Durlauf, 2001) by restricting social interactions to arise from a subset of individuals "friends" within a social environment (or school s) and dividing the unobservable into two components: an unobservable

that also affects friendship choice ε_i and an orthogonal unobservable error that does not enter the friendship choice model μ_i .⁴ Specifically, we consider the following empirical model:

$$H_{is} = \beta_0 + \left(\frac{1}{n_i} \sum_{j \in \Omega_{is}} H_{js} \right) \beta_1 + \left(\frac{1}{n_i} \sum_{j \in \Omega_{is}} X_j \right) \beta_2 + X_i \beta_3 + \varepsilon_{is} + \mu_{is} \quad (1)$$

where H_{is} indicates a particular health behavior, such as tobacco consumption, of individual i in a broad social environment or school s , X_i contains the individual's observable attributes, n_i is the number of friends of person i , Ω_{is} defines the set of individual i 's friends in s , and H_{js} and X_{js} indicate the health behavior and observable attributes of individuals within Ω_{is} .

As Manski (1993) demonstrates, even without correlations in social networks that are caused by sorting into and within networks based on unobservables, e.g. ε_{is} orthogonal to $\left(\frac{1}{n_i} \sum_{j \in \Omega_{is}} X_j \right)$, this model is intrinsically unidentified. By this we mean that there is insufficient information in the regression to estimate uniquely the parameters of interest (β_1 in particular). This occurs because group member characteristics that might explain the health of group members j and so act as instruments for health behavior cannot be excluded from the second stage regression for the health behaviors of i because these attributes may just as reasonably directly influence i 's behaviors (the reflection problem).⁵⁶

⁴ An alternative specification might involve a single unobservables each for determining health behavior and friendship outcomes. The specification in equation (1) is equivalent to such a model with the imposition of one restriction. We start with a model where the composite unobservables in equation (1) and a friendship formation model, $\tilde{\mu}_{is}$ and ε_{is} , are correlated, and then we can define μ_{is} as $\tilde{\mu}_{is} - E[\tilde{\mu}_{is} | \varepsilon_{is}]$ where we assume that the $E[\tilde{\mu}_{is} | \varepsilon_{is}] = \alpha_0 + \alpha_1 \varepsilon_{is}$ so that the composite error $\tilde{\mu}_{is}$ depends upon the uncorrelated disturbances μ_{is} and ε_{is} and α_1 is simply initialized to one in the health behavior model and generality is maintained by allowing ε_{is} to enter the friendship formation model in a general manner.

⁵ For example, if one observes clustering of criminal behavior among friends whose parents have less education, even after controlling for all possible individual and environmental factors that might explain such clustering available in the data, we still cannot conclusively determine whether the clustering is caused because having friends whose parents have less education contributes to criminal behavior or individuals whose parents have less education are more likely to engage in criminal behavior and such criminal behavior influences the behavior of the individual's friends. See Brock and Durlauf (2001, 2006) for recent methodological progress on this problem.

⁶ As noted by Sacerdote (2001) and Bayer and Ross (2008), when social network effects are determined in part by unobservable characteristics, even random assignment cannot solve this identification problem. While random assignment breaks the correlation between the health behavior i 's peers and i 's unobservable characteristics, the coefficient estimate on the behavior of peers is a composite of both the direct effect of peer's behaviors and the effect of peers' unobservable characteristics.

Our identification strategy is to sort students into clusters c based on comparing similar students who faced similar friendship options and made similar friendship choices. This sorting is based on both observable (to the researcher) and unobservable characteristics. Following the standard selection argument: if two individuals make similar choices and differ on observables, then they are expected to differ on unobservables, as well (Heckman, 1979). Similarly, if two individuals are the same on observables and make similar choices, they are expected to be quite similar on unobservables. Therefore, as argued by Dale and Krueger (2002) and Fu and Ross (2010), the inclusion of fixed effects for such clusters should assure that we are comparing students who are similar on both observables and unobservables, which breaks or weakens the correlation between peers' behaviors and a student's unobservable characteristics. Further, since all students in a cluster should have similar observable characteristics, the inclusion of the fixed effect also captures the observables associated with the students' peers while allowing the effect of behavioral differences within a cluster to identify the effect of friend behavior on individual behavior. This feature of the approach solves the empirical problem outlined above and isolates the causal effect of student behaviors on the behavior of their friends from the effect of observable friends' attributes.

Specifically, define a cluster of individuals c in the same school who are observationally equivalent on X_i and choose observationally equivalent friends based on X_j . This structure implies that the individual and friendship group observables are the same within a cluster so that the contribution of the variables that determine clusters to individual's health behavior are constant within cluster or

$$\left(\frac{1}{n_i} \sum_{j \in \Omega_{is}} X_j \right) \beta_2 + X_i \beta_3 = \left(\frac{1}{n_i} \sum_{j \in \Omega_{ks}} X_j \right) \beta_2 + X_k \beta_3 \quad (2)$$

for all $i, k \in c$. Further, we assume that the models that define selection over friendships on health behaviors and on observable attributes depend monotonically on the same observable vector of attributes X_i and the same single index unobservable ε_{is} . This assumption is central to our identification strategy. Without monotonicity, multiple values of the unobservable might be consistent with the same friendship choices for observationally equivalent individuals. With monotonicity, individuals who face the same friendship options based on the available social network (s) and make the same choices should have similar values on the unobservable that

influences health behavior because if they differed substantially on the unobservable they would likely have made different friendship choices.

Specifically, we can define ρ_c as a cluster fixed effect where based on the discussion in the preceding paragraph

$$\rho_c \approx \beta_0 + \left(\frac{1}{n_i} \sum_{j \in \Omega_{is}} X_j \right) \beta_2 + X_i \beta_3 + \varepsilon_{is} \approx \beta_0 + \left(\frac{1}{n_i} \sum_{j \in \Omega_{ks}} X_j \right) \beta_2 + X_k \beta_3 + \varepsilon_{ks} \quad (3)$$

Further, based on the construction of μ as an idiosyncratic disturbance, $E[\mu_{is} | \rho_{c_i}] = 0$ and substituting equation (2) into equation (1) yields

$$H_{ics} \approx \left(\frac{1}{n_i} \sum_{j \in \Omega_{is}} H_j \right) \beta_1 + \rho_c + (\mu_{is} - \bar{\mu}_c) \quad (4)$$

where $(\mu_{is} - \bar{\mu}_c)$ represents the deviation of the right hand side expression in equation (3) from the average of this expression for all individuals in cluster c , $\bar{\mu}_c$.

The two critical assumptions for equation (4) to yield unbiased estimates are [1] that the systematic choices of friends in Ω_{is} over X_j are sufficiently dense to eliminate within cluster deviations in ε from the right hand side of equation (4) and [2] that there exists some unobservables that affect friendship formation over health behavior, e.g. the friendship behavior of friends, but does not directly influence either health behavior or friendship formation over X_j , e.g. the exogenous attributes of friends. The first assumption is required to assure that $(\mu_{is} - \bar{\mu}_c)$ no longer contains information about ε_{is} , which influences friendship formation over health behavior by construction, and the second assumption is required so that an additional source of variation in $\left(\frac{1}{n_i} \sum_{j \in \Omega_{is}} H_{js} \right)$ remains after eliminating variation in ε_{is} . The first assumption is supported by balancing tests we perform below, where we find little evidence of bias from sorting into friendship. The second assumption relies on our finding of substantial across cohort variation in exposure to health behaviors and little variation in the demographic composition of a school across cohorts. These assumptions and our findings concerning the proposed across cohort estimator are formalized in the next section.

Naturally, the approach of using friendship cluster fixed effects as a solution to many of the empirical issues in estimating social network effects requires stronger assumptions than

random assignment or even traditional cohort based studies of peer effects, but this strategy provides a significant payoff by potentially providing estimates of the effect of peer behaviors on individual behaviors that are not contaminated by the direct influence of peer observable characteristics, which is not accomplished by either random assignment or traditional across cohort variation studies.⁷

We provide an appendix with a partial equilibrium model of friendship selection and key assumptions and theorems for our analysis online:

<http://medicine.yale.edu/labs/fletcher/YorkAppendix.pdf>

Friendship Data

In order to accomplish our research goals, we use the only available national dataset containing rich friendship network information as well as health behaviors, the National Longitudinal Study of Adolescent Health (Add Health). The Add Health is a school-based, longitudinal study of the health-related behaviors of adolescents and their outcomes in young adulthood. In short, the study contains an in-school questionnaire administered to a nationally representative sample of students in grades 7 through 12 in 1994-95 and three in-home surveys that focus on a subsample of students in 1995 (Wave 1), and approximately one year (Wave 2) and then six years later (Wave 3). The fourth wave of the survey should be available for analysis later this year. The study began by using a clustered sampling design to ensure that the 80 high schools and 52 middle schools selected were representative of US schools with respect to region of country, urbanicity, size, type, and ethnicity. Eligible high schools included an 11th grade and enrolled more than 30 students. More than 70 percent of the originally sampled high schools participated. Each school that declined to participate was replaced by a school within the stratum.

For this paper, we focus on the In-School data collection, which utilized a self-administered instrument to more than 90,000 students in grades 7 through 12 in a 45- to 60-minute class period between September 1994 and April 1995. The questionnaire focused on topics including socio-demographic characteristics, family background, health status, risk behaviors, and friendship nominations. In particular, each student respondent was asked to identify up to 10 friends (5 males, 5 females) from the school's roster. Based on these

⁷ See discussion in footnote 6.

nominations, social networks within each school can be constructed and characterized, linking the health behaviors of socially connected individuals.

Of the nearly 90,000 students in the schools originally surveyed, several reductions in the sample size were made in order to construct the analysis sample. First, nearly 4,500 students did not have individual identification numbers assigned. Nearly 12,000 students did not nominate any friends and 5,000 individuals nominated friends who were not able to be linked with other respondents due to nominations based on incomplete information (“nicknames” rather than names, or the nominated friend did not appear on the Add Heath school roster, etc.) These issues reduced the sample to approximately 66,000 respondents. In this paper, our main focus is on individuals with same-sex/same-grade level friends, which reduces the sample to approximately 58,000 students.⁸ One reason to focus on same-sex friends is that romantic relationships may be nominated as “friends”. In addition, most previous studies of friendship networks also limit the network definition to same-sex friends. We limit our analysis to same-grade friends in order to use cross-cohort (grade) variation in friendship opportunities and choices, as we describe below. While our main focus is on same-sex friendship networks, we also present some evidence of opposite sex friendship networks to examine potential heterogeneity of effects and extend the literature in this direction. In order to retain sample size, we impute missing covariates, such as maternal education, and control for missingness, but we do not impute missing outcomes.

Table 1 presents descriptive statistics of the analysis sample and shows that approximately 34% of the sample reports smoking and 54% of the sample reports drinking alcohol. The average adolescent nominates 2.4 same-sex friends. In Table 2 we present the distribution of friends’ health behaviors in the data. Friendship networks include considerable variation, including individuals who have no smoking/drinking friends through individuals who have all smoking/drinking friends. Appendix Table 1A presents an analysis of the correlates associated with individuals being dropped from the sample for these reason discussed above, as well as additional sources of selection arising from the empirical specification discussed below. Briefly, race, gender, family structure, and missingness on other variables predicts sample selection in to the original 66,000 observations to some extent, however health behaviors are not robust important predictors. In regards to same-sex/same-grade friendship nominations, the

⁸ Of the 66,000 students, 4,300 do not nominate any same grade friends and 4,100 do not nominate any same-grade/same-gender friends (that is, they nominate same grade friends but no same-grade/same gender friends).

likelihood of making such nominations increases by grade and is smaller for more advantaged students. We find that the proportion of smokers in the grade (potential friends) is not related to these nomination patterns, however, individuals with drinking grademates are slightly more likely to nominate same-grade/same-gender friends (a 10 point increase in grademates drinking is associated with a 1 percentage point increase in the probability).

Evidence of Variation in Friendship Options

As we demonstrate above, identification of the effect of friend's health behavior requires a shock in exposure to potential friends with specific health behaviors. In our empirical analysis, we control for fixed effects associated with similar students who make the same friendship choices on student attributes, but because they belong to different cohorts of the same school draw groups of friends who systematically exhibit differing health behavior. That is, the dataset contains multiple cohorts within each surveyed high school, which allows us to combine our friendship type fixed effects with the use of cross-cohort, within-school variation and in doing so are able to compare students who face similar friendship options (are in the same school) and make similar friendship choices. This extension relies heavily on the assumption that individuals who attend the same school, but different grades, have essentially the same "types" of friendship options.

To what extent do students in the same school face similar friendship options? Using the Add Health data, we show below in Table 3 that controlling for school and grade effects can predict over 95% of the variation in racial composition of potential friends (classmates) in the data. Likewise, controlling for school and grade predicts 93% of the variation in peers' maternal education level and 96% of the variation in classmate nativity. These findings suggest that students in different grades but who attend the same school have very similar friendship options based on race and family background of peers.

In addition, there is substantially more variation across cohort, within schools in unhealthy behaviors. Using the same regression analysis, our data show that we only predict 77% of peer smoking rates, 76% of exercise rates, and 81% of peer drinking rates. Thus, these results suggest that there is substantial variation in exposure to health behaviors of potential friends (classmates) even within school, while at the same time the friendship options based on race, maternal education, and nativity is nearly identical for students across grades within the same school. We use these features of our data to make comparisons within schools—students

who face similar environments in terms of friendship opportunities and make similar friendship choices over attributes but have different friendship outcomes over health behavior and unhealthy behavior outcomes.

Empirical Specification

Our friendship clusters are based on students in the same school choosing sets of friends with very similar demographic attributes. As there is evidence that adolescents have strong preferences to befriend classmates based on age, gender, and race (Mayer and Puller 2008; Weinberg 2008), we create our “individual type-friendship type clusters” by focusing primarily on those attributes. Given a limited sample, there is clearly a trade-off between how restrictive we make our definitions of observationally similar individuals and of same friendship types. We begin by placing the most weight on obtaining very specific “friendship-type” clusters. The reason behind this focus is that most of our demographic variables are binary and so after controlling for individual-type on those variables very little information is left that can be used in our specification tests in order to examine whether peer attributes can explain predetermined student attributes. For example, we examine whether peer attributes can explain student race or ethnicity in a model that only controls for within school friendship types. However, we also examine model specifications that include the student’s race (white, black, Hispanic, and Asian) and whether their mother is a college graduate in the creation of individual type-friendship type clusters, and then for years of maternal education we can test whether peer within cluster variation can explain a student’s own maternal education.

The friendship clusters are based on the following exogenous characteristics of chosen friends, including (1) race (black vs. Hispanic vs. white vs. Asian vs. other) (2) maternal education (no college vs. some college vs. college graduate) (3) family structure (living with mother vs. not living with mother) and (4) nativity (native vs. foreign born). Specifically, the number of friends chosen from each characteristic is used in the cluster. Importantly, our clusters are quite flexibly created, such that an individual who chooses five black friends is in a different cluster than an individual who chooses four black friends.⁹ In yet another refinement of our

⁹ As an example, friendship cluster 15 could be created based on nominating four friends such that: friend A is white, has a college educated mother, lives with his mother, and is native born; friend B is white, has a mother with some college, lives with his mother, and is native born; friend C is white, has a college educated mother, lives with his mother, and is foreign born; friend D is black, has a college educated mother, lives with mother, and is native

cluster approach, in some analyses we also include grade levels-pairs within the clusters, so that 7th and 8th graders are compared to each other (and 9th/10th and 11th/12th) in order to move closer to the thought experiment described in the introduction.

In our final model, as discussed above, we restrict our comparisons to students in different grades who are observationally equivalent on X and chose the same friendship set on the X 's. These students are unable to form the same own-grade friendships and so one student could not intentionally select away friends in their comparison group's friendship set. In order to accomplish this, we randomly choose only one student in each grade from each friendship type cluster so that the estimated effect of peer behavior cannot be identified off of within grade variation. In these estimates, the substantial differences in health behavior across cohorts provide the shock to the health behavior of potential same-grade friends that identifies the effect of friends on health behavior.¹⁰

The rich structure of friendship type clusters, as outlined above, will create single clusters of students—those students who have unique or “unusual” friendship preferences. These single clusters will, implicitly, not contribute to the identification of the network effects estimates, as there will be no within-cluster variation to exploit. Our appendix 3A on sample attrition also examines the significance of excluding the variation associated with these observations from our estimates of the effects of friends health behaviors. While we find some evidence that attrition on this dimension varies with observable attributes, the estimated relationship between smoking and drinking status and placement in a single cluster is fairly small. In addition, we repeat the

born. Cluster 16 could be identical except the individual nominated four white friends instead of three white friends and one black friend; Cluster 17 could be identical to cluster 15 except all the nominated friends are native born.

¹⁰ As discussed, an illustration of our combined methodology is that we can compare two students who attend the same high school and each selected five African American, male friends in their same grade. This indicates that these two students faced similar friendship choices and also selected similar friends, given these choices. The difference between these two individuals who seem to have very similar preferences for friends is that one individual is in the 9th grade (and thus selects 9th grade friends) and the second student is in 10th grade in the same school (and thus selects 10th grade friends). We therefore leverage the fact that age has determined whether each student is in 9th or 10th grade in this specific school, and we argue that this “quasi-experiment” allows us to use the 9th grader as a counterfactual to the 10th grader when examining whether health behaviors of friends (H_{jst}) impacts

own-health behavior outcomes (H_{ist}). Thus, we use these two students as the counterfactual for what would have happened had they been in a different grade in the same school, and thus had a different set of friends. We argue that this comparison technique addresses two of the empirical difficulties with estimating causal social network effects: selection of network members (friends) and unobserved causal factors. We address these difficulties by comparison individuals in the same environment (same school) and who, but for their assignments to different grade levels, would have chosen the same friends (randomization based on age).

substantive analyses presented below for subsamples excluding observations associated with singleton clusters and their exclusion has no effect on the pattern of estimates observed.

Evidence of Friendship Selection

We can partially test the validity of our approach by examining whether students seem to be sorting into specific friendship patterns within our friendship clusters. Specifically, we test whether a student's own observable attributes correlate with the attributes of their friends within student clusters. Following the logic of Altonji, Elder, and Tabor (2005), if individuals do not sort on observables into friendships within clusters, it is very unlikely that they have sorted based on unobservable characteristics. For example, if we find no evidence of additional correlation between an individual's own parental education and the parental education of their friends after conditioning on the average level of correlation for all students in this cluster, which might include broader educational categories, then it is unlikely that students are sorting based on unobservable characteristics like the parents' involvement with the students' education or the parents' educational and academic expectations since those unobservable characteristics are likely correlated with parental education. Similar diagnostic tests have been used elsewhere (Bayer, Ross and Topa 2008; Bifulco, Fletcher and Ross 2011).

In Table 4, we present evidence from these diagnostic tests. Each set of rows examines the correlation between a different “outcome” (individual-level characteristic) and friend's characteristics. Columns add controls from left to right. The first column and row shows the correlation between whether an individual is of Hispanic ethnicity (vs non-Hispanic) and the average of his or her friends' maternal education levels (-0.03). Column 2 controls for school fixed effects and reduces the coefficient by 1/3. Column 3 controls for school by cluster fixed effects and reduces the coefficient to 1/10th the size of the baseline regression, and Column 5 adds grade-pairs to the clusters so that 7th/8th, 9th/10th, and 11th/12th graders are compared. and Column 6 adds individual characteristics to the cluster definition, including race and whether the student's mother graduated from college, and Column 7 estimates the Column 6 model selecting one observation per cohort per cluster and weighting clusters back up to their original size for

comparability to Column 6, though the model is not identified for these two columns for this outcome (student race). Similar results arise for whether the individual is white in Row 2.¹¹

In Row 3, we examine the correlation between own-maternal education and the average maternal education of friends. Here, the correlation is quite high—0.33—in the baseline specifications. As we add school X cluster fixed effects in column 5, the coefficients is reduced by more than two-thirds, but is still statistically significant. Finally, we include individual characteristics in Column 6 in the clusters definitions, and the correlation between own and friends' maternal education falls to 0.01 and is not statistically significant. The one observation per cohort sample results in Column 6 indicate a slight increase in the magnitude of the estimates as compared to Column 6, but the effects are still statistically insignificant and substantially smaller than the estimates in the school fixed effects model.

In a second set of balancing tests (Table 4B), we examine the correlations between individual characteristics and friends' health behaviors in order to further assess our ability to control for observables and unobservables in our estimation strategy. In the first row, we show that maternal education is highly associated with friends' drinking behaviors. However, when we control for clustering, the coefficient is reduced by over 90% and is no longer statistically significant. In row 2, we find similar evidence from the correlation between maternal education and friends' smoking behaviors. In row 3, we find that individuals with highly educated mothers are more likely to have friends with caring mothers. However, as we add cluster fixed effects in the final column, this correlation is reduced over 80% and is no longer statistically significant. This result is a strong test of the adequacy of our clusters, as maternal caring might be a typically unobserved characteristic that researchers would worry is not completely captured in our clusters.¹² In two of the three cases, the effect size increases when we shift to the one per cohort sample, but as before the estimated effects are still insignificant and small relative to the school fixed effect estimates. This is suggestive evidence that our cluster controls are reducing endogeneity bias associated with students choosing their friends.

Results

¹¹ The estimated effects in OLS for explaining whether an individual is black is small relative to the standard error in our cluster fixed effect estimates and so a counterfactual based on whether black is non-informative.

¹² Of course, we will control for maternal caring in our results, so any residual correlations in unobservables between the respondent and his friends will be net of these controls and the cluster fixed effects

Same-Sex Friends

Table 5 presents estimates for adolescent smoking where same-sex/same-grade friends are used to define the friendship network. In Column 1, the baseline results suggest that increasing the share of friends who smoke by 10 percentage points would increase own-smoking by nearly 3.9 percentage points. Following previous research on smoking (Fletcher 2010), we find that black students smoke less than white students, as do students with more highly educated mothers. We also show a steep rise in smoking at the transition between middle school and high school and then a plateau during high school.

In Column 2, we follow some previous literature and control for high school fixed effects; however this only reduces the coefficient from 0.388 to 0.368 for friends' smoking. In Column 3 we do not use school fixed effects, but instead use our friendship cluster fixed effects. As discussed above, we create cluster fixed effects based on several aspects of the respondent's friendship nomination patterns, including (a) number of nominations (b) race of nominated friends (white vs. black vs Hispanic vs. Asian vs. other race), maternal education of nominated friends (college graduate vs. non college graduate), whether friend is native born, and whether friend lives with his/her mother. With the inclusion of cluster fixed effects, the coefficient estimate mirrors that of the school fixed effects results (column 1 vs. column 3) from 0.39 to 0.37 and little reduction in the estimates is observed. However, when we control for school X cluster fixed effects in column 4 and so control for same friendship choices given the same friendship opportunity set, we observe a substantially larger decline in the estimated to 0.31. The last three columns limit comparisons to adjacent grades (7/8, 9/10, 11/12), incorporate same observables into the cluster definitions and restrict the sample to one observation per cohort in turn. All of these estimates fall between 0.30 and 0.32

Overall, we see approximately a 25% reduction in the baseline estimate with our inclusion of individual-friendship type fixed effects, and this reduction is substantially more than the reduction associated with controlling for school fixed effects. However, these changes are very small relative to the declines in estimates across the same model specifications for our balancing tests where the declines are typically on the order of 75 to 90 percent and the magnitudes of the final estimates are well below a 1% reduction for a 10 percent increase in peer behavior. As discussed above, as we control for richer cluster definitions, the sample size used to identify the coefficients is reduced due to "singleton clusters". In Appendix Table 5A, we show

that the change in composition is not the explanation for our results by estimating the baseline results in Table 5 using the non-singleton sample across columns.

Table 6 examines drinking behaviors. Baseline results in column 1 suggest that a 10 percentage point increase in friends' drinking is associated with a 3.3 percentage point increase in own-drinking. Like the results for smoking, school fixed effects (added in column 2) reduce this association by a modest amount to 3.0. Using the same cluster definition as in smoking, the results using friendship-cluster fixed effects (but not school fixed effects) in column 3 the coefficient is reduced slightly, suggesting that increasing friends' drinking by 10 points will increase own drinking by 3.2 percentage points. As before, we control school X cluster fixed effects in column 4, our estimated effect falls to 2.5 percentage points. The restriction of comparisons of adjacent grades and the inclusion of individual attributes into the cluster definitions have little impact on our estimates resulting in a 2.4 percentage point effect. However, in the case of drinking, the estimated effect for the one per cohort sample is substantially larger at 2.8 percentage points and so our best estimate of causal effects is only about 15 percent below the OLS estimates quite close to the school fixed effect estimates.

In Table 7, we examine gender and racial differences in the effects of same-sex friends. Results for both smoking and drinking suggest that the baseline social network effects are 1/3 higher for females than males. Interestingly, the gender gap shrinks by about 1/2 once controls are added for all of our cluster specifications. This is suggestive evidence that rather than females being more susceptible to peer pressure/social network effects, there is higher selection into friendships for females than males based on health behaviors. For the racially stratified results, we find evidence of larger social network effects for whites—the differentials are largely unaffected after we include our cluster fixed effects, while for blacks we find no statistically significant effects on either drinking or smoking and for Hispanics the effects for drinking are statistically insignificant.

Opposite Sex Friends

We next extend our analysis to focus on opposite-sex friends. The effects are likely a combination of the influence of opposite sex friends as well as romantic partners, but represent a contribution to the literature because most studies focus on same-sex friends. The results in Table 8 suggest smaller influences from opposite-sex friends—a 10 point increase in friends' smoking is associated with a 2.3 percentage point increase in the likelihood of own-smoking.

While this effect falls after controlling for “friendship types and options”, the effect is stable at 2.3 percentage points for the one per cohort sample. In Table 9, we estimate the effect of increasing friends’ drinking by 10 points is associated with an increase of 2.1 percentage points in own-drinking. The effect is reduced by over 25% for the one per cohort sample with cluster controls. In Table 10, we examine the effects by gender and race. We find no evidence of differential effects by gender. The results by race suggests larger friendship network effects for white students and again little evidence of effects for black students after including controls. The shift to the one per cohort sample has little effect on the estimates, but all results are statistically insignificant due to the larger standard errors associated combining smaller opposite-sex effects with the subsamples and the reduced sample size in the one per cohort sample.

Empirical Extension

Although not included in this draft, we plan on extending the methods in this paper in several directions. One of which is the method by which we force comparison within school, across cohorts. Rather than removing school fixed effects via a general mean differencing, which then compares all student outcomes in a school based on an average baseline for the school, we will calculate unique means for differencing from student information in each grade where the mean is based on all students in a friendship pattern cluster that are not in that particular grade. Further, this differences process also addresses a bias that arises in fixed effects models with a small numbers of students in each cluster. As noted in previous research (Bayer, Ross, and Topa 2008), leaving an individual in their own cluster for mean differencing creates a positive correlation between the fixed effect and the individual’s idiosyncratic error, but dropping the individual creates a negative correlation because the cluster mean is no longer a random sample. By differencing based on students in a cluster from other grades, the mean is based on a random sample of students from those grades and yet is not correlated with the student’s idiosyncratic error. Our initial investigations of this alternative model suggest results that are very close to the estimates from our one per cohort sample with somewhat more precisely estimated standard errors.

Conclusions

While researchers typically examine peer effects by defining the peer group broadly, this paper focuses attention on actual friends and implements a new research design to study the effects of friend's health behaviors on own health behaviors for adolescents. The main idea is to combine a cross-cohort, within school design with controls for friendship options through high school fixed effects and friendship choices through the use of "friendship type" fixed effects. We show that in the Add Health data used in this paper, there is evidence that our design is successful in narrowing down relevant comparison groups by controlling for the friendship choices and friendship options of adolescents. Our initial estimates also suggest that all results are robust to the restriction of sample to one student per cluster per cohort, which assures that the model is only identified based on comparisons of students across clusters.

Further, we use a model of friendship formation to investigate the circumstances under which our identification strategy will provide consistent estimates. We find that our approach can be applied under quite general circumstances. For example, our model allows for a very general non-linear process of friendship selection, allows for correlation between observable attributes and unobservables that affect friendship formation, and allows for a simultaneity between own health behavior and friendship choice over health behavior as long as we are interested in an estimate of the effect of friends behavior that includes feedback effects. The key assumption that is required to apply this identifications strategy is that individuals experience some type of shock in exposure to health behavior of potential friends that does not directly enter own health behavior. This shock assures that some variation remains in friends' health behavior even after eliminating variation across individuals in friendship outcomes. In our application, this "treatment" is the variation across cohorts in the exposure to friends' health behavior. Our empirical analysis is very supportive of this assumption in that we find very small variation in the demographic attributes of students across cohorts in the same school, but substantially larger variation in health behavior.

Overall, our results suggest that friendship network effects are important in determining adolescent tobacco and alcohol use but are over-estimated in specifications that do not fully take into account the endogeneity of friendship selection by 15-25%, and we also find evidence that gender differences in social network effects are explained by selection bias. We present new evidence of the effects of opposite sex friends on health behaviors and also find racial differences in friendship network effects.

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Table 4
Balancing Tests of Friendship Sorting

Outcome Specification	Hispanic OLS	Hispanic OLS	Hispanic OLS	Hispanic OLS	Hispanic OLS	Hispanic OLS
Fixed Effects	None	School	School-Cluster	School-GradePair-Cluster	School-GradePair-Cluster-X	School-GradePair-One Per Cluster-X
Friends' Maternal Education	-0.032*** (0.006)	-0.022*** (0.005)	-0.003 (0.002)	-0.003 (0.003)		
Observations	65456	65456	65456	65456		
R-squared	0.027	0.306	0.696	0.725		
Outcome Specification	White OLS	White OLS	White OLS	White OLS	White OLS	White OLS
Fixed Effects	None	School	School-Cluster	School-GradePair-Cluster	School-GradePair-Cluster-X	School-GradePair-One Per Cluster-X
Friends' Maternal Education	0.026*** (0.007)	0.019*** (0.004)	0.002 (0.002)	0.002 (0.003)		
Observations	65495	65456	65456	65456		
R-squared	0.003	0.009	0.570	0.752		
Outcome Specification	Maternal Education OLS	Maternal Education OLS	Maternal Education OLS	Maternal Education OLS	Maternal Education OLS	Maternal Education OLS
Fixed Effects	None	School	School-Cluster	School-GradePair-Cluster	School-GradePair-Cluster-X	School-GradePair-One Per Cluster-X
Friends' Maternal Education	0.331*** (0.026)	0.197*** (0.021)	0.024 (0.020)	0.007 (0.014)	-0.010 (0.017)	0.018 (0.072)
Observations	65456	65456	65456	65456	65456	49511
R-squared	0.061	0.123	0.530	0.586	0.869	0.975

Each set of rows and each column displays coefficients from separate regressions. All regressions control for grade-level fixed effects.

Table 4B
Balancing Tests of Friendship Sorting (Health Behaviors)

Outcome Specification	Maternal Education OLS	Maternal Education OLS				
Fixed Effects	None	School	School-Cluster	School-GradePair-Cluster	School-GradePair-Cluster-X	School-GradePair-One Per Cluster-X
Friends' Drinking	-0.239*** (0.057)	-0.280*** (0.033)	-0.179*** (0.046)	-0.064 (0.048)	-0.059 (0.066)	-0.004 (0.203)
Observations	53895	53895	53895	53895	54027	43797
R-squared	0.002	0.112	0.603	0.665	0.915	0.980
Outcome Specification	Maternal Education OLS	Maternal Education OLS				
Fixed Effects	None	School	School-Cluster	School-GradePair-Cluster	School-GradePair-Cluster-X	School-GradePair-One Per Cluster-X
Friends' Smoking	-0.303*** (0.074)	-0.337*** (0.048)	-0.234*** (0.059)	-0.091* (0.046)	-0.031 (0.072)	-0.050 (0.217)
Observations	54027	54027	54027	54027	53564	43895
R-squared	0.003	0.113	0.602	0.665	0.939	0.981
Outcome Specification	Maternal Education OLS	Maternal Education OLS				
Fixed Effects	None	School	School-Cluster	School-GradePair-Cluster	School-GradePair-Cluster-X	School-GradePair-One Per Cluster-X
Friends' Maternal Caring	0.216*** (0.034)	0.157*** (0.023)	0.082** (0.041)	0.079** (0.035)	0.039 (0.047)	0.053 (0.137)
Observations	51017	51017	50289	51017	50289	41009
R-squared	0.002	0.112	0.601	0.602	0.938	0.980

Each set of rows and each column displays coefficients from separate regressions. All regressions control for grade-level fixed effects.

Table 5
Friendship Network Effects on Smoking

Outcome Specification Friends	Smoke OLS Same Sex/ Same Grade	Smoke OLS Same Sex/ Same Grade School- GradePair- Cluster	Smoke OLS Same Sex/ Same Grade School-GradePair- Cluster-X	Smoke OLS Same Sex/ Same Grade School-GradePair- One Per Cluster-X			
Fixed Effects	None	School	Cluster	School-Cluster	School-GradePair- Cluster	School-GradePair- Cluster-X	School-GradePair- One Per Cluster-X
% Smoke	0.385*** (0.010)	0.366*** (0.011)	0.368*** (0.012)	0.308*** (0.019)	0.297*** (0.022)	0.295*** (0.028)	0.315*** (0.031)
Age	0.035*** (0.004)	0.034*** (0.004)	0.033*** (0.005)	0.035*** (0.008)	0.036*** (0.010)	0.039*** (0.015)	0.036** (0.015)
Male	-0.010** (0.005)	-0.014*** (0.005)	-0.017*** (0.006)	-0.012 (0.010)	-0.010 (0.013)	-0.016 (0.020)	-0.002 (0.018)
Hispanic	-0.024** (0.011)	0.001 (0.009)	-0.002 (0.011)	-0.003 (0.022)	-0.010 (0.028)	-0.006 (0.040)	
Black	-0.093*** (0.008)	-0.108*** (0.009)	-0.093*** (0.013)	-0.088*** (0.028)	-0.092*** (0.033)	-0.054 (0.048)	
Asian	-0.081*** (0.009)	-0.080*** (0.011)	-0.056*** (0.012)	-0.089*** (0.025)	-0.098*** (0.028)	-0.072 (0.068)	
Live with Mom	-0.066*** (0.009)	-0.065*** (0.009)	-0.072*** (0.009)	-0.077*** (0.019)	-0.072*** (0.024)	-0.072 (0.061)	-0.073* (0.037)
Maternal Education	-0.004*** (0.001)	-0.005*** (0.001)	-0.003*** (0.001)	-0.006*** (0.002)	-0.006** (0.002)	-0.003 (0.006)	-0.006* (0.004)
Maternal Caring Index	-0.071*** (0.003)	-0.071*** (0.004)	-0.070*** (0.004)	-0.070*** (0.007)	-0.074*** (0.009)	-0.073*** (0.015)	-0.082*** (0.013)
Native Born	0.067*** (0.014)	0.059*** (0.010)	0.050*** (0.014)	0.054** (0.024)	0.056** (0.023)	0.043 (0.045)	0.047 (0.034)
Observations	50249	50249	50249	50249	50249	50249	40427
R-squared	0.140	0.147	0.248	0.581	0.651	0.772	0.775

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Additional Controls: Grade dummies, Constant, Missing Indicator

Table 6
Friendship Network Effects on Drinking

Outcome Specification	Drink OLS Same Sex/ Same Grade						
Friends					School-GradePair- Cluster	School-GradePair- Cluster	School-GradePair- Cluster-X
Fixed Effects	None	School	Cluster	School-Cluster			
% Drink	0.329*** (0.011)	0.302*** (0.011)	0.316*** (0.013)	0.253*** (0.019)	0.239*** (0.022)	0.235*** (0.033)	0.284*** (0.089)
Age	0.032*** (0.004)	0.031*** (0.004)	0.034*** (0.005)	0.032*** (0.009)	0.033*** (0.011)	0.033** (0.015)	0.025 (0.057)
Male	0.005 (0.004)	0.002 (0.004)	0.003 (0.005)	0.007 (0.010)	0.006 (0.012)	0.009 (0.020)	0.022 (0.066)
Hispanic	0.018* (0.010)	0.026*** (0.008)	0.033*** (0.011)	0.044* (0.023)	0.044 (0.027)		
Black	-0.039*** (0.010)	-0.051*** (0.011)	-0.070*** (0.015)	-0.074*** (0.028)	-0.072** (0.028)		
Asian	-0.101*** (0.011)	-0.118*** (0.012)	-0.078*** (0.014)	-0.081*** (0.026)	-0.078** (0.032)		
Live with Mom	-0.066*** (0.009)	-0.065*** (0.009)	-0.071*** (0.009)	-0.069*** (0.018)	-0.075*** (0.021)	-0.050 (0.076)	-0.091 (0.250)
Maternal Education	-0.004*** (0.001)	-0.006*** (0.001)	-0.004** (0.001)	-0.006*** (0.002)	-0.006** (0.002)	0.005 (0.010)	0.006 (0.022)
Maternal Caring Index	-0.064*** (0.004)	-0.064*** (0.003)	-0.064*** (0.004)	-0.063*** (0.007)	-0.062*** (0.009)	-0.065*** (0.014)	-0.069 (0.057)
Native Born	0.086*** (0.014)	0.084*** (0.013)	0.070*** (0.012)	0.079*** (0.022)	0.083*** (0.028)	0.077 (0.052)	0.118 (0.209)
Observations	49656	49656	49656	49656	49656	49656	40570
R-squared	0.153	0.163	0.270	0.609	0.674	0.807	0.942

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Additional Controls: Grade dummies, Constant, Missing Indicator

Table 7
Racial and Gender Differences for Same-Sex Friendship Networks

Outcome Specification	Smoke OLS	Smoke OLS	Smoke OLS	Smoke OLS	Smoke School-GradePair-Cluster	Smoke School-GradePair-Cluster-X	Smoke School-GradePair-One PerCluster-X
Fixed Effects Females	None	School	Cluster	School-Cluster	School-GradePair-Cluster	School-GradePair-Cluster-X	School-GradePair-One PerCluster-X
% Smoke	0.428*** (0.013)	0.403*** (0.014)	0.403*** (0.016)	0.312*** (0.031)	0.292*** (0.036)	0.288*** (0.054)	0.322*** (0.057)
Males							
% Smoke	0.330*** (0.014)	0.305*** (0.014)	0.313*** (0.015)	0.267*** (0.030)	0.257*** (0.037)	0.253*** (0.057)	0.269*** (0.057)
White							
% Smoke	0.432*** (0.010)	0.413*** (0.011)	0.417*** (0.011)	0.354*** (0.019)	0.343*** (0.024)	0.338*** (0.022)	0.372*** (0.038)
Black							
% Smoke	0.189*** (0.022)	0.148*** (0.023)	0.160*** (0.023)	0.074 (0.052)	0.072 (0.068)	0.058 (0.102)	0.131 (0.126)
Hispanic							
% Smoke	0.321*** (0.014)	0.288*** (0.013)	0.280*** (0.017)	0.249*** (0.039)	0.238*** (0.051)	0.244*** (0.084)	0.244** (0.115)
Outcome Specification	Drink OLS	Drink OLS	Drink OLS	Drink OLS	Drink School-GradePair-Cluster	Drink School-GradePair-Cluster-X	Drink School-GradePair-One PerCluster-X
Fixed Effects Females	None	School	Cluster	School-Cluster	School-GradePair-Cluster	School-GradePair-Cluster-X	School-GradePair-One PerCluster-X
% Drink	0.365*** (0.013)	0.335*** (0.013)	0.348*** (0.015)	0.268*** (0.024)	0.239*** (0.030)	0.219*** (0.048)	0.267*** (0.052)
Males							
% Drink	0.283*** (0.015)	0.250*** (0.016)	0.267*** (0.017)	0.213*** (0.031)	0.204*** (0.034)	0.206*** (0.050)	0.225*** (0.066)
White							
% Drink	0.384*** (0.011)	0.353*** (0.011)	0.373*** (0.012)	0.311*** (0.019)	0.292*** (0.023)	0.284*** (0.029)	0.339*** (0.046)
Black							
% Drink	0.198*** (0.016)	0.167*** (0.017)	0.181*** (0.017)	0.138*** (0.035)	0.130*** (0.045)	0.159** (0.062)	0.145 (0.096)
Hispanic							
% Drink	0.232*** (0.019)	0.206*** (0.020)	0.177*** (0.026)	0.129*** (0.046)	0.113* (0.062)	0.111 (0.080)	0.125 (0.136)

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8
Friendship Networks Effects of Smoking: Opposite Sex Friends

Outcome Specification Friends	Smoke OLS Opp Sex/ Same Grade School- GradePair-Cluster	Smoke OLS Opp Sex/ Same Grade School-GradePair- Cluster-X	Smoke OLS Opp Sex/ Same Grade School-GradePair- One Per Cluster-X			
Fixed Effects	None	School	Cluster	School- GradePair-Cluster	School-GradePair- Cluster-X	School-GradePair- One Per Cluster-X
% Smoke	0.228*** (0.010)	0.203*** (0.010)	0.228*** (0.011)	0.202*** (0.022)	0.214*** (0.037)	0.233 (0.168)
Age	0.040*** (0.005)	0.037*** (0.005)	0.038*** (0.006)	0.029** (0.014)	0.028 (0.021)	0.029 (0.102)
Male	-0.014 (0.010)	-0.014 (0.009)	-0.023** (0.011)	-0.009 (0.020)	-0.023 (0.033)	-0.043 (0.121)
Hispanic	-0.039** (0.016)	-0.004 (0.013)	-0.021 (0.014)	-0.021 (0.030)		
Black	-0.109*** (0.010)	-0.127*** (0.011)	-0.095*** (0.014)	-0.120*** (0.034)		
Asian	-0.089*** (0.014)	-0.092*** (0.016)	-0.048*** (0.017)	-0.070 (0.046)		
Live with Mom	-0.053*** (0.012)	-0.053*** (0.012)	-0.063*** (0.014)	-0.037 (0.032)	-0.054 (0.099)	-0.075 (0.499)
Maternal Education	-0.006*** (0.001)	-0.007*** (0.001)	-0.005*** (0.001)	-0.009*** (0.003)	-0.004 (0.009)	-0.001 (0.053)
Maternal Caring Index	-0.077*** (0.004)	-0.078*** (0.004)	-0.075*** (0.005)	-0.073*** (0.010)	-0.075*** (0.017)	-0.065 (0.109)
Native Born	0.077*** (0.019)	0.066*** (0.015)	0.062*** (0.020)	0.054 (0.035)	0.047 (0.066)	0.048 (0.319)
Observations	33807	33807	33807	33807	33807	26346
R-squared	0.083	0.096	0.225	0.663	0.796	0.957

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 9
Friendship Networks Effects of Drinking: Opposite Sex Friends

Outcome Specification Friends	Drink OLS Opp Sex/ Same Grade						
Fixed Effects	None	School	Cluster	School-Cluster	School-GradePair-Cluster	School-GradePair-Cluster-X	School-GradePair-Cluster-X
% Drink	0.211*** (0.011)	0.180*** (0.010)	0.213*** (0.013)	0.183*** (0.020)	0.170*** (0.024)	0.168*** (0.040)	0.156 (0.183)
Age	0.039*** (0.006)	0.035*** (0.006)	0.038*** (0.006)	0.032*** (0.012)	0.030** (0.013)	0.025 (0.022)	-0.000 (0.104)
Male	0.017** (0.007)	0.017** (0.007)	0.012 (0.008)	0.011 (0.013)	0.011 (0.016)	0.010 (0.026)	0.016 (0.125)
Hispanic	0.017 (0.013)	0.020** (0.010)	0.021 (0.014)	0.038 (0.025)	0.034 (0.034)		
Black	-0.049*** (0.012)	-0.070*** (0.012)	-0.061*** (0.014)	-0.056** (0.028)	-0.067* (0.034)		
Asian	-0.113*** (0.014)	-0.139*** (0.014)	-0.072*** (0.015)	-0.072** (0.035)	-0.079* (0.043)		
Live with Mom	-0.069*** (0.011)	-0.070*** (0.010)	-0.085*** (0.012)	-0.091*** (0.024)	-0.090*** (0.030)	-0.154* (0.089)	-0.117 (0.478)
Maternal Education	-0.006*** (0.002)	-0.009*** (0.001)	-0.006*** (0.002)	-0.008*** (0.003)	-0.008*** (0.003)	0.000 (0.010)	-0.003 (0.065)
Maternal Caring Index	-0.063*** (0.004)	-0.063*** (0.004)	-0.062*** (0.005)	-0.060*** (0.009)	-0.058*** (0.011)	-0.059*** (0.019)	-0.066 (0.109)
Native Born	0.113*** (0.020)	0.112*** (0.018)	0.111*** (0.021)	0.129*** (0.031)	0.140*** (0.038)	0.143** (0.066)	0.161 (0.337)
Observations	33702	33702	33702	33702	33702	33702	26259
R-squared	0.114	0.132	0.264	0.627	0.691	0.816	0.961

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 10
Racial and Gender Differences for Opposite-Sex Friendship Networks

Outcome Specification	Smoke OLS	Smoke OLS	Smoke OLS	Smoke OLS	Smoke OLS	Smoke OLS
Fixed Effects Females	None	School	Cluster	School-GradePair-Cluster	School-GradePair-Cluster-X	School-GradePair-One Per Cluster
% Smoke	0.233*** (0.012)	0.214*** (0.012)	0.231*** (0.013)	0.225*** (0.029)	0.227*** (0.062)	0.223 (0.365)
Males						
% Smoke	0.224*** (0.012)	0.212*** (0.012)	0.228*** (0.014)	0.230*** (0.037)	0.257*** (0.066)	0.199 (0.528)
White						
% Smoke	0.258*** (0.011)	0.234*** (0.011)	0.257*** (0.012)	0.231*** (0.029)	0.246*** (0.041)	0.238 (0.168)
Black						
% Smoke	0.102*** (0.016)	0.062*** (0.017)	0.111*** (0.022)	0.065 (0.067)	0.071 (0.076)	0.156 (0.573)
Hispanic						
% Smoke	0.187*** (0.021)	0.155*** (0.020)	0.173*** (0.028)	0.179** (0.082)	0.200* (0.111)	0.303 (0.698)
Outcome Specification	Drink OLS	Drink OLS	Drink OLS	Drink OLS	Drink OLS	Drink OLS
Fixed Effects Females	None	School	Cluster	School-GradePair-Cluster	School-GradePair-Cluster-X	School-GradePair-One Per Cluster
% Drink	0.211*** (0.012)	0.191*** (0.011)	0.214*** (0.014)	0.172*** (0.036)	0.168** (0.073)	0.107 (0.488)
Males						
% Drink	0.211*** (0.015)	0.181*** (0.014)	0.219*** (0.016)	0.188*** (0.036)	0.210*** (0.064)	0.254 (0.633)
White						
% Drink	0.251*** (0.013)	0.212*** (0.012)	0.254*** (0.013)	0.212*** (0.026)	0.202*** (0.042)	0.189 (0.207)
Black						
% Drink	0.105*** (0.021)	0.076*** (0.021)	0.101*** (0.027)	0.045 (0.053)	0.045 (0.069)	0.026 (0.551)
Hispanic						
% Drink	0.167*** (0.022)	0.154*** (0.023)	0.159*** (0.034)	0.119** (0.050)	0.143** (0.068)	0.131 (0.580)

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix Tables
<http://medicine.yale.edu/labs/fletcher/YorkAppendix2.pdf>