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Peer effects on risky behaviors: New evidence from college roommate assignments

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ABSTRACT

Social scientists continue to devote considerable attention to spillover effects for risky behaviors because of the important policy implications and the persistent challenges in identifying unbiased causal effects. We use the natural experiment of assigned college roommates to estimate peer effects for several measures of health risks: binge drinking, smoking, illicit drug use, gambling, having multiple sex partners, suicidal ideation, and non-suicidal self-injury. We find significant peer effects for binge drinking but little evidence of effects for other outcomes, although there is tentative evidence that peer effects for smoking may be positive among men and negative among women. In contrast to prior research, the peer effects for binge drinking are significant for all subgroups defined by sex and prior drinking status. We also find that pre-existing risky behaviors predict the closeness of friendships, which underscores the significance of addressing selection biases in studies of peer effects.

1. Introduction

The spread of substance use and other risky behaviors in social networks is important to understand in order to inform health and social policy. Information about spillover effects can improve predictions about the dynamics of behaviors in populations and assist the design of interventions that mitigate harmful spillovers or leverage beneficial spillovers. Behaviors such as heavy alcohol consumption and other substance use have substantial impacts on health, functioning, and educational outcomes (Rice, 1999; Carpenter and Dobkin, 2011; Carrell et al., 2011).

Economists and other social scientists continue to devote considerable attention to measuring spillover effects for risky behaviors not only because of the important policy implications but also because of the challenges in identifying unbiased causal effects. As Manski (1993) and others have described, there are three main factors that may bias estimates of social interaction effects: (1) the reflection problem, in which the effect of others on the self cannot be disentangled from the reverse; (2) selection into social networks, which may lead to correlations in unmeasured individual characteristics and generate spurious correlations in outcomes; and, (3) unmeasured contextual factors, or "common shocks," which may also generate spurious correlations in outcomes. In addition, from a policy perspective it is useful to distinguish between "endogenous" peer effects that imply multiplier effects (behavior A by one person is directly influenced by behavior A by another person), versus "contextual" peer effects that imply causal but not necessarily multiplier effects (behavior A by one person is influenced by being around another person engaging in behavior A, but the causal mechanism is through other peer characteristics correlated with behavior A).

In this study we use the natural experiment of assigned college roommates to estimate peer effects for substance use and

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other risky behaviors. This empirical approach addresses the identification issues noted above and thus yields unbiased estimates. The approach has been used in previous studies mainly to look at academic outcomes, particularly grade point average (GPA), using administrative data from colleges and universities. For this study we collected new survey data to examine a range of behaviors with important implications for health and wellbeing: binge drinking, cigarette smoking, illicit drug use, gambling, sexual activity, suicidal ideation, and non-suicidal self-injury.

We find significant peer effects for binge drinking but little evidence of effects for the other outcomes. The effects for binge drinking are robust to controlling for a range of additional peer characteristics, suggesting that these are true spillover effects ("endogenous" rather than "exogenous" effects, in Manski's terminology), although we cannot rule out the possibility that the effects are driven at least in part by unmeasured roommate characteristics. The magnitude of the effects—a 8.6 percentage point increase in the probability of binge drinking, as a result of having a binge-drinking roommate—is somewhat smaller than in most previous studies of peer effects. As compared to a previous study based on college roommate assignments, which finds significant peer effects on binge drinking only for men with prior binge drinking (Duncan et al., 2005), we find more widespread effects: for both women and men, and for both prior binge drinkers and prior non-binge drinkers. We also examine the closeness of roommates' relationships, as reported in the follow-up survey. This analysis indicates that similarity in pre-existing behaviors predicts closeness of relationships, for the most part. Also, roommates who end up being close friends exhibit stronger apparent peer effects on binge drinking; this differential is less robust, however, when we look at predicted friendship levels based on baseline measures, rather than the endogenous actual friendship levels.

2. Background and prior research

2.1. Conceptual discussion

The discussion of social interaction effects by Glaeser and Scheinkman (2001) offers a useful starting point for considering how peers might influence each other's behaviors. They describe various mechanisms that could produce such effects, including what they term learning, stigma, and taste-related interactions. Learning about risky behaviors from peers may take place through direct communication as well as observation. The new information may in turn cause changes in the net price of the behavior (e.g., by lowering the search costs) and in the perceived benefits and costs (e.g., by demonstrating positive and negative consequences of the behavior). Whereas learning refers to effects on information, stigma and taste-related interactions refer to effects on preferences. Stigma-related interactions include situations in which one's opinion about the desirability of a behavior is influenced by observing other people doing that behavior and one's opinions or feelings toward those people. For example, if a student observes that her roommate uses marijuana and the student likes or respects the roommate, this may lower the student's stigmatizing attitudes about drug use. Taste-related interactions refer to a more direct influence on preferences: one may simply have a desire for conformity or imitation, such that observing someone else's behavior raises the desirability of doing the same (Cutler and Glaeser, 2007).

These mechanisms suggest that peer effects on risky behaviors may go in either direction. For example, peers' behaviors may reduce one's behavior if the learning from peers highlights adverse consequences, whereas peers' behaviors may increase the behavior if the learning highlights positive consequences. Also, the possible mechanisms imply that, other things equal, peer effects should be larger (in either direction) for behaviors that are more likely to be observed or discussed among peers, as awareness of peer behaviors is a necessary precursor to learning or preference effects. This suggests that, among the behaviors we examine, binge drinking is more likely to exhibit large peer effects, because in college settings drinking frequently takes place in social contexts with many peers (Beck et al., 2008). Also, heavy drinking has relatively low stigma in college-age populations (and on the contrary, is often considered a *positive* marker of social status), suggesting that it is likely to be openly discussed among students (Neighbors et al., 2007). Another reason peer effects might be especially strong for alcohol, as well as drug use, is that peers may have more influence on search costs for goods that cannot be legally purchased.

Young people may also experience peer effects differently depending on their gender and their previous risky behaviors. For instance, males and females differ somewhat in their exposures and responses to social pressures during adolescence and young adulthood, and they also differ more generally in their developmental processes with respect to risky behaviors (Byrnes et al., 1999). Young people with previous risky behaviors may be more influenced by peer effects, if they are more likely to be near the margin in their propensity to engage in a behavior or not. On the other hand, people with previous risky behaviors may be less influenced by peer effects, if they have more solidly formed preferences and information about the behaviors.

2.2. Prior empirical evidence

A number of studies in the recent economics literature estimate peer effects on substance use among adolescents in secondary schools, using various combinations of instrumental variables and fixed effects (Gaviria and Raphael, 2001; Powell et al., 2005; Lundborg, 2006; Clark and Loheac, 2007; Fletcher, 2010, 2012). All of these studies find significant peer effects for the behaviors under examination, and in most cases the estimates imply fairly large effects.[3] These approaches improve upon prior studies in terms of addressing the key identification issues noted earlier, but their validity still depends on some untestable assumptions about the lack of correlations in unobserved variables among peers.[4] A recent study by Card and Giuliano (2013) addresses this issue by specifying structural assumptions about selection into friendships and carefully examining the robustness of these assumptions, finding significant peer effects for sexual behavior, marijuana use, cigarette smoking, and truancy.

The most similar study to ours is that by Duncan et al. (2005), the only published study using college roommate assignments to estimate peer effects on risky behaviors. Their sample includes

[3] For example, these studies find that for each percentage point increase in peers engaging in a behavior, individuals have the following percentage point increases in the likelihood of the behavior: (1) Gaviria and Raphael (2001): 0.35 (SE = 0.13) for drinking, 0.32 (0.08) for drug use, 0.16 (0.12) for smoking. (2) Powell et al. (2005): 0.58 (0.10) for smoking. (3) Lundborg (2006): 0.23 (0.08) for binge drinking, 0.17 (0.05) for smoking, 0.07 (0.02) for drug use. (4) Clark and Loheac (2007): mostly significant effects for smoking, drinking, marijuana, in the range of 0.10–0.20. (5) Fletcher (2010): 0.35 (0.17) for smoking. (6) Fletcher (2012): 0.57 (0.24) for binge drinking.

[4] Another noteworthy contribution to this literature has been the evidence from a community-level adult sample in the Framingham Heart Study on social interaction effects for cigarette smoking (Christakis and Fowler, 2008) and alcohol use (Rosenquist et al., 2010). Although economists have questioned whether these studies sufficiently address the key identification issues (Cohen-Cole and Fletcher, 2008), the studies have garnered significant media attention (Kolata, 2008) and have enlivened interest in spillover effects in the fields of medicine, public health, and beyond. Also, an important strength of these studies is the rich information on social networks, including neighbors, family members, and friends.

three cohorts who began college in fall 1998, 1999, or 2000 at a large, academically competitive university. In the winter/spring semester of 2002 they surveyed these students when they were in their 2nd, 3rd, or 4th year of college, and they linked the responses to a pre-existing data set with survey responses from the students and their randomly assigned roommates during the summer before their first year of college. They find no evidence of peer effects on marijuana use or number of sexual partners. In contrast, they find significant peer effects on the frequency of binge drinking among men who binge drank in high school, but not among men who did not binge drink in high school and not among women regardless of prior binge drinking.

Two other roommate studies are also relevant to ours, although these studies do not directly examine risky behaviors as outcomes. First, in a study connected to that of Duncan et al. (2005), using data from the same cohorts at the same institution, Kremer and Levy (2008, 2003) find that men who binge drank in high school obtain lower grade point averages (GPAs) when paired with another binge drinker in their first year of college. As noted by the authors, this is consistent with the finding in Duncan et al. (2005) that peer effects for binge drinking are larger among men, and indicates that there are significant academic implications of this peer effect. Kremer and Levy also find that the effect on GPA persists beyond the first year of college, which points toward more lasting mechanisms such as preferences and social networks rather than the contemporaneous disruption of the studying environment in the first-year room. Second, Sacerdote (2001) finds that, among randomly assigned first-year roommates, whether a man joins a fraternity is positively and significantly correlated with his roommate's decision, and that living in a dormitory with more students who drank beer prior to college is also positively and significantly correlated with the probability of joining a fraternity (for men) or a sorority (for women). Given the well-established correlation between fraternity/sorority participation and drinking (McCabe et al., 2005), this finding is also consistent with a significant peer effect for binge drinking, particularly among men, and suggests that an important part of the mechanism may be the influence on which social networks people join.

While our basic purpose and approach are the same as in the study by Duncan et al. (2005), there are several features that extend our study beyond a straightforward replication (which would be valuable in itself, given their intriguing findings). First, by collecting data on the extent to which roommates are close friends and spend time together, we are able to characterize the "exposure" in our natural experiment. This information makes it easier to interpret the peer effects we observe (and the extent to which they generalize to other contexts), and also quantifies the extent to which risky behaviors are intertwined with selection into friendships, which can introduce biases in observational studies of peer effects that are not based on plausibly exogenous natural experiments.

Second, our data are from a more recent cohort (entering college in 2009), for whom peer effects may be quite different, considering how behaviors and social context among young people have evolved over the past 10–15 years.[5] Third, our sample includes two institutions, which is useful for examining whether peer effects might generalize across campuses. Fourth, we look at outcomes during the first year of college; these outcomes, as compared to outcomes later in college, may be more strongly influenced by first-year roommates. Fifth, we expand the set of risky behaviors to include gambling, smoking, suicidal ideation,[6] and non-suicidal self-injury, in addition to the behaviors in the previous study (binge drinking, drug use, and sexual behavior). Finally, we examine a supplemental sample of students with requested roommates, as a point of comparison that further illustrates the likely biases from measuring peer effects without fully addressing selection biases.

3. Methods and data

3.1. Overview

Our data come from online surveys of first-year college students at two large and academically competitive universities: one public (hereafter "university A"), and one private ("university B"). We fielded the baseline survey in August 2009, shortly before students arrived at college, and the follow-up survey in March–April 2010, shortly before the end of the academic year. We linked the survey data to administrative data on housing preferences, room assignments, and academic and demographic characteristics.

First-year students are required to live in campus housing at both universities, except in unusual circumstances. Students have the option of requesting specific roommates, and these requests are typically granted. Students who do not request specific roommates are assigned their roommates. Our analysis focuses on students with assigned roommates, although for comparison's sake we also examine a smaller sample with requested roommates. We describe the roommate assignment process later in this section.

Our main empirical approach is analogous to that of previous studies of peer effects among college roommates, estimating regressions of the form:

$$Y_{(t+1)} = \beta_0 + \beta_1 Prefs_t + \beta_2 Ypeers_t + \beta_3 Y_t + \beta_4 X_t + \varepsilon_{t+1} \quad (1)$$

The subscript t denotes a measurement in the baseline survey, and $t+1$ denotes a measurement in the follow-up survey. Y refers to the risky behavior being examined, *Prefs* is a vector of housing preferences and all other variables used to make roommate assignments (described in more detail later), *Ypeers* is the average risky behavior of the peers[7] with whom the student is assigned to live (roommates in most analyses, but hallmates in some), and X is a vector of individual characteristics including gender, age (exact to the day), race/ethnicity, and parents' education. The key coefficient is β_2, which represents the effect of peer behavior on the individual's behavior. For binary outcomes we estimate probit regressions and report average marginal effects (with standard errors estimated with the delta method), and for other outcomes we estimate linear or ordered probit regressions. In all specifications heteroskedasticity-robust standard errors are corrected for correlated outcomes among roommates (or hallmates in analyses of hallmate effects).

[5] For example, between 1998 and 2008 there were significant decreases in the prevalence of cigarette smoking, and increases in the proportion reporting that their friends would disapprove of their cigarette smoking, among Americans ages 18–22 (Johnston et al., 2009). Also, during that period there was some closing in the gender gap for binge drinking: the prevalence for men fell from 52% in 1998 to 49% in 2008, whereas the prevalence for women rose from 31% to 34%.

[6] Suicidal ideation is not a risky behavior per se, but it is a primary risk factor for suicidal behavior. Suicidal behavior is too rare to be studied meaningfully in most samples including our own; past-year suicide attempts were reported by only 0.6% of college students in a national sample (Eisenberg et al., 2013a,b).

[7] In cases where students have more than one roommate, as in most other studies of peer effects we focus on the average peer behavior under the logic that it is the best proxy of the peer context. Our estimated roommate effects remain very similar if we instead define the roommate variable as the maximum behavior among multiple roommates.

3.2. Survey data collection and sample characteristics

At both baseline and follow-up we recruited students for the surveys by first sending an introductory letter with a $10 bill (a "pre-incentive," with no obligation to participate), and then sending up to four email invitations to those who had yet to respond, spaced by 3–5 days each. All communications included a web link to the survey and a unique, randomly assigned log-in ID for each student. Recruitment messages also informed students that they were entered into a sweepstakes for cash prizes regardless of participation.

Recruitment for the baseline survey was timed at each school to take place during the three weeks prior to the start of the semester. The follow-up survey data collection also lasted three weeks and was timed to conclude one week prior to final exams in the spring. Because obtaining informed consent of minors typically requires parental consent, from the outset of the study we excluded students if they were going to be under the age of 18 as of the follow-up survey in March 2010—this restriction excluded 0.9% of otherwise eligible students.

As implied by Eq. (1), our primary analytic sample consists of students who completed both baseline and follow-up surveys and whose roommate(s) also completed the baseline survey.[8,9] Prior to the baseline survey, the initial number of eligible students with assigned roommates was 4971, including 3876 from university A and 1095 from university B (which has a large proportion of first-year students in single rooms, unlike university A). A total of 3501 (70%) of these students completed the baseline survey. Among baseline responders, 2589 (74%) had at least one roommate who was also a baseline responder. And among baseline responders with at least one roommate baseline responder, 1641 (63%) completed the follow-up survey.[10]

Because our primary analytic sample is only 33% (1641/4971) of the initially eligible sample, it is important to examine potential biases related to survey non-response. The first two columns in Appendix Table A show that the sample responding to the baseline survey is nearly identical to the initial sample in terms of age, gender, race/ethnicity, and U.S. versus international citizenship. The table also reveals that the other layers of attrition (response by roommate at baseline, and own response at follow-up) are not strongly related to risky behaviors and other characteristics. Despite the reasonably large sample size, the only statistically significant differences across layers of attrition are a slightly higher proportion of women in the final analytic sample (0.53) as compared to the initial sample (0.50) and a slightly lower proportion of binge drinkers (0.34) in the final analytic sample as compared to all baseline respondents (0.37).[11]

Additional characteristics of the primary analytic sample are shown in Table 1. Most students (79%) are in double rooms (i.e., with one roommate), 17% are in triples, and 4% in quads. The typical socioeconomic background is high, with 83% of students having at least one parent with a college degree. Compared to the national population of students in higher education (Planty et al., 2009), our sample has higher percentages of whites (70% versus 63% nationally) and Asians (17% versus 7%), and lower percentages of blacks (3% versus 14%) and Hispanics (5% versus 12%).

We examine substance use and other risky behaviors that are relatively common among adolescents and young adults. Our survey questions are adapted from the Healthy Minds Study, an annual national survey of college student health (Eisenberg et al., 2007). Binge drinking is measured using the question, "Over the past 30 days, on how many occasions have you had X drinks in a row?", where, following standard definitions, X is shown as 4 for women and 5 men. The answer categories in the survey are "None," "Once," "Twice," "3 to 5 times," "6–9 times," and "10 or more times." In most analyses we use a binary coding of binge drinking (none versus any), but in some analyses we examine the frequency. As shown in Table 1, binge drinking increases substantially between baseline (33% at least once) and follow-up (54%). Cigarette smoking is measured using the question, "In the past 30 days, how many cigarettes did you smoke on average?" The prevalence of smoking is low at baseline (only 6% with any smoking) and increases slightly at follow-up (8%). Illicit drug use is measured using the question, "In the past six months, have you used any of the following drugs? (Select all that apply)" The answer choices include marijuana, cocaine, heroin, methamphetamines, other stimulants, ecstasy, other, and none of the above. Drug use increases from 22% at baseline to 32% at follow-up, and the vast majority of drug use is marijuana (among students reporting any illicit drug use, 97% used marijuana, 5% stimulants, 3% ecstasy, 2% cocaine, 0.3% heroin, and 10% other drugs). Gambling is measured by the question, "In the past month, did you make any sort of bet? (By 'bet' we mean betting on sports, playing cards for money, playing gambling games online, buying lottery tickets, playing pool for money, playing slot machines, betting on horse races, or any other kind of betting or gambling)" Gambling decreases slightly, from 22% at baseline to 19% at follow-up. The number of sexual partners is measured by the question, "In the past six months, with how many different people have you had sex (oral sex or sexual intercourse)?" As shown in the table, the distribution of responses shifts upwards for this question between baseline and follow-up, with more students with at least one partner (from 35% to 46%) and also more with multiple partners (from 12% to 15%). Suicidal ideation is measured by the question, "In the past six months, did you ever seriously think about attempting suicide?" The percentage answering yes was 2.5% at baseline and 4.1% at follow-up. Finally, non-suicidal self-injury was measured by the question, "This question asks about ways you may have hurt yourself on purpose, without intending to kill yourself. In the past six months, have you ever done any

[8] If a student has multiple roommates and some but not all completed the baseline survey, we still include that student in the sample. In those cases we code the roommate variable as the average among roommates who completed the baseline survey.

[9] Throughout our analysis roommates are defined based on initial assignments. Therefore one can think of our estimates as "intention-to-treat," ignoring the endogenous changes in roommates during the school year. These changes are discouraged by the universities and occurred for only a small proportion of students. Specifically, between our baseline and follow-up surveys 3% of students received a new room assignment (but remained in a campus residence), and 1.5% of students moved out of campus housing. These numbers are similar across the two universities.

[10] This lower response rate at follow-up is somewhat surprising, given that it is conditional on responding at baseline (which indicates a propensity to respond to surveys). We believe that the response rates were higher at baseline than at follow-up for several reasons: (a) just prior to arrival students may have been especially attentive to solicitations related to the university; (b) by the time of the follow-up survey, students had received a number of requests to complete surveys, in addition to our baseline survey (we do not know the exact number of other surveys but we are aware of at least a couple others at each campus); (c) students were busier while school was in session.

[11] It is also interesting to examine correlations in the likelihood of survey participation among roommates. As expected, at baseline the roommates' participation in the survey is not significantly associated with the probability of participation ($p = 0.64$), controlling for housing preferences. Participation in the follow-up survey, however, is significantly associated with roommates' participation in the follow-up survey (in a linear probability model: $B = 0.067$, $SE = 0.018$, $p < .001$), controlling for housing preferences and participation at baseline. Furthermore, participation at follow-up is significantly associated with roommates' participation at baseline ($B = 0.045$, $SE = 0.019$, $p = 0.02$), indicating that at least part of the correlation in participation at follow-up is due to a causal effect of roommate participation.

Table 1
Mean values of primary analytic sample (*N* = 1641).

	Baseline		Baseline	Follow-up
University A (large public)	0.69	**Binge drinking (past 30 days)**		
University B (large private)	0.31	None	0.67	0.46
		Once	0.11	0.14
Double room	0.79	Twice	0.08	0.12
Triple room	0.17	3–5 times	0.09	0.16
Quad room	0.04	6–9 times	0.04	0.10
		10+ times	0.02	0.03
Age	18.4 (SD = 0.41)			
		Smoking (past 30 days)		
Female	0.54	None	0.94	0.92
		<1 cigarette/day	0.04	0.06
White	0.70	1–5 cigarettes/day	0.01	0.02
Asian	0.17	About 1/2 pack/day	0.002	0.001
Black	0.03	About 1 pack/day	0.002	0.001
Hispanic	0.05	>1 pack/day	0.001	0.001
Other	0.02			
Multi	0.04	Illicit drugs (past 30 days)	0.22	0.32
Parents' education		Gambled (past 30 days)	0.23	0.19
Less than college degree	0.16			
College degree	0.27	**Sex partners (past 6 months)**		
Graduate degree	0.56	None	0.65	0.54
		1	0.27	0.32
		2	0.06	0.07
		3–5	0.02	0.06
		6 or more	0.004	0.02
		Suicide ideation (past 6 mos.)	0.025	0.041
		Non-suicidal self-injury (past 6 mos.)	0.102	0.114

All values are proportions, except age. Primary sample consists of first-year undergraduates meeting these conditions: (a) at least 18 years old as of follow-up survey (March 15, 2010); (b) assigned to their roommate(s) (i.e., did not request their roommate(s)); (c) completed both baseline and follow-up surveys; (d) at least one roommate completed baseline survey.

of the following intentionally? (Select all that apply)" There were 13 different answer choices, including 11 types of self-injury, an "Other (specify" option, and "No, none of these." The percentage reporting at least one type of self-injury was 10.2% at baseline and 11.4% at follow-up, and the specific types of self-injury, in order of prevalence at follow-up, were "punched or banged myself" (3.7%), "punched or banged an object to hurt myself" (3.5%), "scratched or pinched myself severely" (2.5%), "prevented wound from healing" (2.0%), "cut myself" (1.9%), "bit myself" (1.9%), "pulled my hair, eyelashes, or eyebrows with intent to hurt myself" (1.6%), "ripped or tore my skin" (1.1%), "burned myself" (0.9%), "other" (0.8%), "rubbed sharp objects into my skin" (0.7%), and "carved words or symbols into skin" (0.6%).

3.3. Exogeneity of roommate assignments

For students who do not request roommates, the assignment processes differ somewhat between the two universities in our sample, but the common feature is that assignments are based only on known variables that we observe in our data set. Therefore, any variation in roommate characteristics (such as substance use), conditional on the variables that explicitly determine the assignments, should be uncorrelated with the error term in Eq. (1).

At university A, a public school with approximately 6000 first-year students, the housing administrators match roommates based on gender plus preferences regarding the following variables (as indicated on housing applications): geographic area of campus (three options); room type (double, triple, or quad); co-ed versus same-sex hallway; and substance use environment (the student can indicate that he or she is a smoker, a non-smoker, or someone who wants to live in an entirely substance free residence). To the extent possible the housing administrators match roommates with identical preferences on the above variables. If a perfect match is not available, the housing officials prioritize the variables in the order listed above. For students who submit their housing application by a certain deadline, the order in which they are allocated to residences and rooms is determined by a random lottery (generated by the housing officials using Microsoft Excel's random number function). This accounts for the vast majority (89%) of first-year students with assigned roommates at university A. The remaining students (11%) who miss the deadline are assigned in the order in which their housing applications are received (e.g., a student who prefers a double room would be matched with the next student to submit an application with identical preferences). This implies that for university A, roommate assignments are truly random, conditional on the preferences noted above, for the vast of majority of students, whereas the date of housing application needs to be controlled for as flexibly as possible for the remaining students.

At university B, a private school with approximately 4000 first-year students, the housing office uses a commercial software program to match students based on a more extensive list of variables. Although we do not have access to the proprietary algorithm by which the matching is done, we have complete data on all variables used and we know from conversations with housing officials that the variables receiving the most weight are similar to that for university A: gender (which is always matched among assigned roommates at both universities), preferred room type (double, triple, quad), preference for co-ed versus single-sex hallways, and smoking status. The secondary matching variables include preferences about sleeping hours, background noise while studying, types of music, and the extent of socializing in the room.

The key assumption in our empirical strategy is that the roommate's baseline characteristics (risky behaviors or any factors that might influence risky behaviors) are uncorrelated with the error term in the regression in Eq. (1). This assumption cannot be tested unequivocally, but as in prior studies in the roommate literature we obtain suggestive evidence by examining the correlation among roommates in baseline variables, conditional on the variables used to make assignments. In these checks of exogeneity as well our main analyses, we control for the variables used to make housing assignments in the following way. We relax parametric assumptions to the extent possible by using a set of dummy variables corresponding to all combinations of the primary variables used for matching roommates at each university. To control for the date of housing application at university A, we include a vector of dummy variables corresponding to each week during the 9-week period in which late applications were received (and on-time applications are denoted by a tenth dummy variable). For university B, we include the secondary matching variables as sets of categorical dummies corresponding to each answer choice for each variable. We also include a dummy variable for university (A or B).[12]

If housing assignments are exogenous conditional on these variables, then the conditional correlations among roommates at baseline should not be significantly different from zero. We check this by estimating Eq. (2) below for each risky behavior variable that we consider as an outcome in this paper, as well as several other characteristics that might plausibly be related to risky behaviors (happiness, depression, anxiety, eating disorder symptoms, suicidal ideation, non-suicidal self-injury, parents' education, religiosity, binge drinking, physical activity, hours studying for school, admissions test scores, and GPA in high school). We estimate this equation both with and without controlling for the correction proposed by Guryan et al. (2009); we implement this correction by adding a control for the average value of the Y variable among other students with an identical combination of values for the primary housing variables (i.e., the pool of potential roommates).[13]

$$Y_t = \beta_0 + \beta_1 Prefs_t + \beta_2 Ypeers_t + \varepsilon_t \qquad (2)$$

Appendix Table B shows the estimates of β_2 in equation 2 for the main outcome variables in this study as well as several other variables that might plausibly be related to risky behaviors. As shown in the table none of the variables are significantly correlated among roommates at baseline except for illicit drug use. The overall pattern of results in the table is consistent with what one would expect due to chance if the null hypothesis of conditionally random assignment were true: only one of 15 variables exhibits a significant correlation at $p < 0.10$. More generally, when we examine all 33 variables available in our baseline data, we continue to find a pattern consistent with random chance: 3 of 33 variables exhibit conditional correlations significant at $p < 0.10$, with one positive correlation (for illicit drug use) and two negative. Also, as shown in the table, the correction from Guryan et al. does not change the estimates appreciably, which is not surprising, given that they demonstrate that the correction has little impact in larger samples.

4. Estimates of peer effects

As shown in Table 2, we find strong evidence for peer effects on binge drinking, but no apparent effects for the other behaviors. Having a roommate who binge drinks at baseline increases the probability of binge drinking at follow-up by 8.6%, or a 19% increase relative to the mean. The null results for the other behaviors allow us to rule out large peer effects with high confidence, but we cannot rule out small but meaningful effects. Particularly in the case of smoking, which is reported at follow-up by only 8% of our sample, we cannot rule out peer effects that are nontrivial relative to the mean.

It is important to interpret the results in Table 2 with the caveat that these could be considered multiple tests of a related hypothesis—that peers influence each other's risky behaviors in general, or that peers influence each other's health in general. When thinking of the hypotheses in this composite way, one should adjust the p-values to reflect the greater risk of type I errors (false positives). Appendix C illustrates this issue by showing the estimated peer effects for not only the risky behaviors but also other health-related outcomes in our larger project.[14] Although the simple Bonferroni adjustment has been criticized as overly conservative (over-adjusting to eliminate type I errors, at the expense of introducing type II errors), it illustrates the basic story that would remain true under other types of adjustments: the estimated peer effect for binge drinking remains highly significant, and a composite null hypothesis of no peer effects for risky behaviors is easily rejected.

We also estimate hallmate effects, where hallmates are defined as students who live in the same floor and residence. In order to focus on hallmate effects independent of roommate effects, we also control for roommate behaviors in the regressions with hallmate behaviors. Because hallways typically include many students (the majority of our sample has at least 10 hallmates also in the sample), there is only modest variation in hallmate averages, which results in relatively large standard errors in our estimated peer effects. We find for all seven behaviors that the estimates are not significant at $p < 0.10$ (results available on request). When we restrict attention to same-gender hallmates, however, we do find some evidence for peer effects among men for smoking ($B = 0.18$, $SE = 0.07$, $p = 0.01$) and gambling ($B = 0.17$, $SE = 0.10$, $p = 0.07$). The same-gender peer effect is not significant for binge drinking among men ($B = -0.10$, $SE = 0.10$, $p = 0.31$), and it is also not significant for any of the behaviors among women.

4.1. "Endogenous" versus "contextual" peer effects

As noted earlier, from a policy perspective it matters whether, in Manski's terminology, peer effects are "endogenous" versus "contextual." Both are causal peer effects, but only the former implies multiplier effects whereby changing the behavior in one person leads to changes in the same behavior among peers. Careful consideration of this issue has often been absent from peer effects studies, perhaps because it is already challenging enough to identify any type of causal effect. As in other peer effects studies, there is no way for us to rule out definitively the importance of unmeasured peer characteristics, but we can examine the sensitivity of our findings to controlling for additional roommate characteristics.[15]

We find that the main pattern of results is robust to including many other roommate characteristics in the regressions,

[12] Note that residence fixed effects are not necessary to obtain unbiased estimates, because our identification strategy, like most previous roommate studies, focuses on roommates' behaviors at baseline (prior to when any common "shocks" could occur within residences). Nevertheless, we confirm that our estimates are not sensitive to including residence fixed effects: the significant peer effect for binge drinking remains unchanged and the peer effects for other behaviors are still non-significant.

[13] This correction is intended to counteract a negative conditional correlation among roommates that can be present mechanically when there are small numbers of people in groups defined by identical housing preferences. The negative correlation arises because roommates are increasingly likely to be on opposite sides of the group mean (which the dummy variables control for) as the group size gets smaller.

[14] The mental health results are described in more detail in Eisenberg et al. (2013a,b), and the obesity and physical activity results are in Yakusheva et al. (2013).

[15] Even in a randomized trial it would be very difficult to make a definitive distinction between endogenous and contextual peer effects. Consider, for example, a trial that randomizes individuals to an educational and counseling intervention designed to reduce binge drinking, and then assesses outcomes among the intervention group, the control, and the peers of both groups. If one finds that the peers of the intervention group have lower binge drinking than the peers of the control group, this would suggest a social multiplier effect from the intervention. But it would not necessarily imply a universal social multiplier driven by binge drinking behavior per se. It is possible, for example, that the mechanism for the peer effect was information or attitudes, rather than the behavior per se.

Table 2
Effects of roommate behaviors on own behaviors.

	Binge drinking	Smoking	Illicit drug use	Gambling	Multiple sex partners	Suicidal ideation	Non-suicidal self-injury
Roommate behavior at baseline							
Average marginal effect	0.086	0.002	0.012	−0.004	−0.035	−0.001	0.016
Standard error of marginal effect	0.024	0.026	0.025	0.023	0.031	0.033	0.024
p-Value	<.001	0.950	0.616	0.875	0.259	0.964	0.503
Own behavior at baseline							
Average marginal effect	0.375	0.243	0.443	0.256	0.278	0.131	0.267
Standard error of marginal effect	0.021	0.026	0.017	0.018	0.024	0.023	0.020
p-Value	<.001	<.001	<.001	<.001	<.001	<.001	<.001

N = 1641. Average marginal effects calculated from probit regressions with controls for housing preferences and gender, age, race/ethnicity, and parents' education.

suggesting (but not proving) that we are measuring a true social multiplier effect for roommate binge drinking. The additional roommate characteristics in these regressions include: the other risky behaviors in this study, a measure of mental health (the K-6 psychological distress score (Kessler et al., 2003)), parents' educational attainment, religiosity, frequency of exercise in the past 30 days, amount of studying per day in high school, standardized test scores, and high school grade point average. With these additional covariates the estimated peer effect for binge drinking remains almost identical ($B = 0.085$, SE = 0.028, $p = 0.002$), and the estimates for other behaviors also remain nearly identical as before (in each case, close to zero and insignificant). In future work, a more confident distinction between endogenous and contextual peer effects might be made by controlling for a richer set of additional peer characteristics, such as personality type, and also examining specific mechanisms related to information and attitudes.

4.2. Subgroup analyses of roommate effects

When the roommate effects are estimated separately by gender (Table 3), the binge drinking effect is significant and similar in magnitude for men and women. This is a notable difference from the results in Duncan et al. (2005), where the effects are only present for men. In our results the only apparent difference by gender is for smoking, where there is a positive, though not significant, peer effect for men and a negative and significant effect for women. This difference in estimated effects by gender is significant at $p = 0.02$.

Next we examine how one's susceptibility to peer effects varies as a function of one's behavior at baseline (Table 4). For binge drinking the peer effect is similar whether or not the individual is a binge drinker at baseline, and if anything the effect is a bit stronger for non-binge drinkers at baseline. This is again somewhat different than Duncan et al.'s findings, in which the only group experiencing a peer effect is men with prior binge drinking. Because our analysis uses a binary outcome, the peer effect on binge-drinkers at baseline could be attenuated by a ceiling effect (87% of baseline binge drinkers report binge drinking at follow-up), which we explore later by looking at frequency of binge drinking. There are no significant differences by baseline behavior in the peer effects for the other behaviors, either. We obtain large negative peer effects for students who smoked or had multiple sexual partners at baseline, but these estimates are very imprecise due to the small size of these subgroups.

We also estimate the peer effects separately by university (not shown in tables), as tentative evidence on whether effects might generalize across campuses. The binge drinking effect is highly significant at both universities in our sample, with average marginal effects of 0.08 (SE = 0.03, $p = 0.01$) at university A and 0.12 (SE = 0.04, $p = 0.01$) at university B. Although the sample includes just two universities, this suggests that the peer effects we observe for binge drinking may be a general phenomenon on college campuses, or at least on large, academically competitive campuses such as the two in our study. We also find that the null results for the other risky behaviors hold across the two universities, with the exception that university B has a marginally significant peer effect for smoking ($B = 0.06$, SE = 0.04, $p = 0.10$).

4.3. Asymmetries in peer effects based on social status or influence

It is possible that peer effects between roommates are asymmetric. For example, student A may have strong influence over his or her roommate, student B, whereas student B has little influence over student A. This might occur if student A has higher social status. To examine this possibility, we consider three possible markers of social status: parental education (i.e., socioeconomic background), admissions test score (as an indicator of academic status/ability), and number of sexual partners in the previous six months (as an indicator of sexual experience, which is perceived by many adolescents to have a positive association with social status (Ott et al., 2006)). For each marker of social status, we code a student as being higher, lower, or the same as the roommate (or roommates' average for multiple roommates). This is straightforward for the categorical variables (parental education and number of sexual partners), and the continuous standardized test score we use plus or minus one standard deviation as the threshold for higher or lower. We then re-run our main regressions with the addition of an interaction between the roommate behavior variable and the dummy for being higher status than the roommate and an interaction between the roommate behavior variable and the dummy for being lower status.

For binge drinking the results provide some support for the idea that peer influence is stronger from higher to lower status students. Students with lower test scores than their roommates experience higher peer effects (interaction term: $B = 0.11$, SE = 0.07, $p = 0.09$) and students with more sexual experience than their roommates experience lower peer effects ($B = -0.16$, SE = 0.06, $p = 0.008$). For other risky behaviors, however, students with lower status than their roommates appear to experience smaller peer effects, if anything. For example, students with lower test scores than their roommates experience lower peer effects for drug use ($B = -0.21$, SE = 0.06, $p = 0.001$), gambling ($B = -0.16$, SE = 0.06, $p = 0.008$) and self-injury ($B = -0.10$, SE = 0.06, $p = 0.11$). Also, students with lower parental education experience lower peer effects for smoking ($B = -0.10$, SE = 0.06, $p = 0.10$) and students with fewer sexual partners experience lower peer effects for drug use ($B = -0.08$, SE = 0.06, $p = 0.14$). Overall, these results suggest that asymmetries related to social status are highly variable by indicator of status and by behavioral outcome.

Table 3
Subgroup analysis by gender: effects of roommate behaviors on own behaviors.

	Binge drinking	Smoking	Illicit drug use	Gambling	Multiple sex partners	Suicidal ideation	Non-suicidal self-injury
Men ($N = 774$)							
Roommate behavior at baseline							
Average marginal effect	0.079	0.057	0.033	−0.004	−0.016	0.015	0.021
Standard error of marginal effect	0.035	0.033	0.032	0.034	0.051	0.037	0.037
p-Value	0.023	0.084	0.303	0.898	0.756	0.69	0.564
Women ($N = 863$)							
Roommate behavior at baseline							
Average marginal effect	0.095	−0.078	−0.004	−0.015	−0.060	−0.02	0.011
Standard error of marginal effect	0.034	0.041	0.034	0.040	0.042	0.044	0.033
p-Value	0.005	0.053	0.901	0.700	0.152	0.64	0.732

Average marginal effects calculated from probit regressions with controls for housing preferences and gender, age, race/ethnicity, and parents' education. Sample sizes do not always add up across sub-groups to the exact total reported in Tables 1 and 2 because of a small number of missing values for specific variables.

Table 4
Subgroup analysis by baseline behavior: effects of roommate behaviors on own behaviors.

	Binge drinking	Smoking	Illicit drug use	Gambling	Multiple sex partners	Suicidal ideation	Non-suicidal self-injury
Baseline behavior = 0 (no)							
N	1083	1531	1277	1261	1495	1605	1479
Roommate behavior at baseline							
Average marginal effect	0.101	−0.001	0.002	−0.015	−0.032	0.008	0.013
Standard error of marginal effect	0.034	0.025	0.029	0.025	0.032	0.031	0.025
p-Value	0.003	0.967	0.954	0.555	0.313	0.787	0.604
Baseline behavior = 1 (yes)							
N	549	103	354	372	135	41	167
Roommate behavior at baseline							
Average marginal effect	0.072	−0.395	0.062	0.043	−0.258	n/a[a]	0.024
Standard error of marginal effect	0.035	0.408	0.053	0.059	0.134		0.179
p-Value	0.042	0.333	0.241	0.465	0.054		0.895

Average marginal effects are calculated from probit regressions with controls for housing preferences and gender, age, race/ethnicity, and parents' education. Sample sizes do not always add up across sub-groups to the exact total reported in Tables 1 and 2 because of a small number of missing values for specific variables.
[a] Insufficient observations to estimate the regression (more righthandside variables than observations).

Table 5
Subgroup analysis by baseline behavior and gender (binge drinking only).

	Men, baseline = 0	Women, baseline = 0
N	501	578
Roommate behavior at baseline		
Average marginal effect	0.130	0.089
Standard error of marginal effect	0.050	0.046
p-Value	0.009	0.051
	Men, baseline = 1	**Women, baseline = 1**
N	269	280
Roommate behavior at baseline		
Average marginal effect	0.014	0.127
Standard error of marginal effect	0.057	0.053
p-Value	0.809	0.016

Average marginal effects are calculated from probit regressions with controls for housing preferences (as explained in the text) and gender, age, race/ethnicity, and parents' education. Sample sizes do not always add up across sub-groups to the exact total reported in Tables 1 and 2 because of a small number of missing values for specific variables. The difference between the estimated effect for men baseline non-drinkers (0.130 and men baseline drinkers (0.014) is not quite significant ($p = 0.15$).

4.4. More detailed examination of binge drinking effects

Given the strong evidence of peer effects on binge drinking in our sample, as well the interesting findings in Duncan et al. (2005), we examine the findings for this behavior in more detail. First, to provide a more direct comparison with Duncan et al., we estimate subgroup effects by both baseline behavior and gender (Table 5). These results are nearly the opposite of those in the prior study: we find the strongest evidence for peer effects in the subgroups *other than* men who reported binge drinking prior to college, whereas they find peer effects only for that subgroup. As noted earlier, our lack of evidence for peer effects on prior binge drinkers could be due to a ceiling effect. This does not appear to be the story, however. When we mimic the main specification in Duncan et al. (2005), with frequency of binge drinking as the dependent variable and a binary measure of roommate binge drinking as the key independent variable,[16] we find a pattern of results mostly similar to that in Table 5: no evidence for peer effects on male baseline binge drinkers ($B = -0.07$, $SE = 0.46$, $p = 0.87$), significant effects for both male ($B = 0.47$, $SE = 0.24$, $p = 0.05$) and female non-binge drinkers at baseline ($B = 0.45$, $SE = 0.21$, $p = 0.04$), and a positive but not significant effect on female binge drinkers at baseline ($B = 0.17$, $SE = 0.45$, $p = 0.71$).

We also examine the possibility that peer effects may be different depending on the frequency of binge drinking by roommates.[17] We define roommate binge drinking (or roommates' average, for multiple roommates) as occasional if the number of episodes is greater than zero but less than three times in the past month,

[16] In these specifications we estimate linear regressions, where the dependent variable is linear and corresponds to the number of times in the previous two weeks. The answer choices in our survey question about binge drinking include numerical intervals, so we approximate the number of times binge drinking as 4 if "3 to 5 times" was selected, 8 if "6 to 9 times was selected," and 10 if "10 or more times" was selected.

[17] We can also examine the effects of intensity/frequency for two other behaviors, smoking and the number of sexual partners. The problem, however, is that there is little variation to work with; as indicated in Table 1, at baseline only 2% of students report smoking more than one cigarette per day, and similarly only 2% report more than two sexual partners in the past six months. Given this lack of variation, it is not surprising that we still find non-significant peer effects for these behaviors when we distinguish between lower intensity/frequency (<1 cigarette per day; 1 or 2 sexual partners) and higher intensity/frequency (>1 cigarette per day; >2 sexual partners) of roommate behaviors.

and frequent if it is three times or more. The reference category is no binge drinking by roommates. We find that being assigned to roommates with occasional binge drinking causes a 0.04 (SE = 0.03, p = 0.11) increase in the probability of binge drinking, and being assigned to roommates with frequent binge drinking causes a 0.11 (SE = 0.03, p = 0.001) increase. The distinction between roommates' occasional and frequent binge drinking appears to matter more for men: the effects for men are 0.01 (SE = 0.04, p = 0.86) from occasional roommate binge drinking versus 0.12 (SE = 0.05, p = 0.01) from frequent roommate binge drinking, as compared to 0.08 (SE = 0.04, p = 0.04) and 0.11 (SE = 0.04, p = 0.01) for women.

4.5. Correlations among roommate outcomes

For the sake of comparison to our main results, we also estimate how roommates' behaviors are correlated at follow-up, controlling for baseline behaviors. This analysis is biased by any contextual factors shared by roommates during the academic year ("common shocks"), but is useful to examine as a point of reference. The analytic sample size is now reduced to 1041, because data are required from the reference individual and his or her roommate(s) at both baseline and follow-up. We find non-significant peer effects for all risky behaviors (even binge drinking), except for a surprising negative peer effects for having multiple sexual partners (B = −0.12, SE = 0.05, p = 0.02).

4.6. Results with requested roommates

Also for the sake of comparison, we replicate our analysis with a smaller supplementary sample of students who requested their roommates. We drew this survey sample only from university A. In contrast to the assigned roommates, this sample exhibits highly significant correlations among roommates at baseline for some risky behaviors, even after controlling for the housing preference variables in equation 1 (even students who requested roommates are required to fill out the full housing applications). For example, having a roommate who binge drinks at baseline is associated with a 0.34 increase (SE = 0.08, p < 0.001) in the probability of binge drinking at baseline. This further underscores the selection bias that may be present in estimates of peer effects not based on exogenous variation in peer contacts.

When we replicate our main analysis with this sample of requested roommates, we find that the baseline behavior of roommates is a significant predictor of own behavior at follow-up for smoking and drug use, but not for the other behaviors (results available on request). Although we cannot disentangle the true peer effects from selection biases in this supplemental sample, it is important to keep in mind that in theory the true effects among requested roommates may be higher or lower than those among assigned roommates. As Kremer and Levy (2008) point out, students who have known each other for a long time may have already exerted most of their peer effects on each other, which may lower the effects apparent during the first year of college. On the other hand, requested roommates tend to have closer relationships than assigned roommates, as we find in our survey measures at follow-up (e.g., 85% of requested roommates report being close friends), which could contribute to stronger peer effects.

5. Analysis of roommates' relationships

In the follow-up survey we asked students how much time they typically spend doing things or hanging out with their roommates, whether they are close friends with them, and how much they enjoy spending time with them. Students with multiple roommates were asked to think about their roommates on average. We use this information to supplement our analysis in several ways.

First, we examine the distribution of responses to these questions, to understand the extent and nature of contact between peers in our natural experiment. In our sample of assigned roommates, 41% spend at least an hour per day doing things or hanging out with their roommates, 49% agree or strongly agree that they are close friends with their roommates, and 54% agree or strongly agree that they enjoy being in the room together with their roommates. This suggests that assigned roommates have close relationships in about half of cases, whereas in the other half of cases the "treatment" in this natural experiment can be thought of as sharing a small living area without a close relationship.

Second, we conduct subgroup analyses estimating how peer effects vary depending on the closeness between roommates. These estimates should be viewed with caution, because the subgroups are defined by a variable that is measured at follow-up and is endogenous with respect to peer and own binge drinking. We find, as one might expect, that the peer effects on binge drinking are stronger for students who report being close friends with their roommates (B = 0.12, SE = 0.03, p < 0.001) as compared to those who do not report being close friends with their roommates (B = 0.05, SE = 0.04, p = 0.14), and this difference in effects is significant at p = 0.07. For the other risky behaviors the estimated peer effects are close to zero and not significant regardless of closeness between roommates.

Third, we examine whether the risky behaviors of roommates at baseline predict the closeness of their relationship. As shown in Table 6, for three of the risky behaviors—binge drinking, illicit drug use, and gambling—a student who has engaged in the behavior before arriving at college ends up spending significantly, more time with a roommate who has also engaged in the behavior, as compared to one who has not (B_3 is significantly larger than B_2). The opposite is true, however, for smoking and self-injury: a smoker (or someone who self-injures) at baseline spends *less* time with a smoker (or self-injuring) roommate, as compared to a non-smoker (or non-self-injuring) roommate.[18] This may be related to stigma and shame associated with these behaviors. On the other hand, for a student who has *not* engaged in risky behaviors at baseline, the time spent with their roommate is not significantly predicted by the risky behavior of the roommate (B_1 is not significantly different from zero). Each of these patterns remains similar when we look at our other measures of closeness among roommates: whether they consider themselves close friends, and whether they enjoy spending time together (results not shown in table, available on request).[19]

Although peer effects for binge drinking influence students regardless of their own prior binge drinking (as shown earlier in the paper), the results in Table 6 suggest a more nuanced pattern for how the closeness of roommate relationships are influenced by risky behaviors. It appears that the type of roommate (having engaged in risky behavior or not) influences the relationship

[18] Although we previously showed that peer effects for smoking appear to be negative among women and positive among men, this negative effect of peer smoking on friendships among roommates is present for both men and women (although more precise for women): for women the estimate of B_3 is −0.95 and the p-value is <0.001, whereas for men the estimate is −0.85 with p-value = 0.07.

[19] Note also that, if "time spent with roommate" is an objective and accurate measure of time that roommates spend together, then we should impose the constraint $\beta_1 = \beta_2$in the regressions shown in Table 6. We thank an anonymous reviewer for pointing this out. When those constraints are imposed, the basic pattern of results remains the same, however. We display the unconstrained version of the regression because this seems somewhat more intuitive to interpret and it acknowledges that relationship closeness among roommates can be asymmetric for other measures (such as perceived closeness).

Table 6
Interactive effects of baseline behavior of self and roommate (RM), on time spent with RM (h/day).

	Binge drinking	Smoking	Illicit drug use	Gambling	Multiple sex partners	Suicidal ideation	Non-suicidal self-injury
Self no, RM no (reference category)							
Self no, RM yes							
β_1	−0.048	−0.032	−0.188	0.011	0.023	−0.317	−0.146
SE	0.133	0.170	0.125	0.137	0.186	0.244	0.151
p-Value	0.718	0.852	0.131	0.935	0.900	0.194	0.335
Self yes, RM no							
β_2	0.050	−0.214	−0.206	−0.116	−0.006	−0.058	−0.341
SE	0.139	0.207	0.134	0.133	0.188	0.323	0.148
p-Value	0.717	0.301	0.126	0.381	0.975	0.856	0.021
Self yes, RM yes							
β_3	0.374	−0.925	0.530	0.297	−0.546	−1.33	−0.587
SE	0.172	0.257	0.243	0.230	0.395	0.25	0.21
p-Value	0.030	<0.001	0.029	0.196	0.168	<.001	0.005
p-Value for Wald test of $\beta_2 = \beta_3$	0.072	0.011	0.004	0.079	0.215	0.001	0.301

Sample is limited to students in double rooms (with only one roommate) ($N = 1274$). Average marginal effects are calculated from OLS regressions with controls for housing preferences (as explained in the text) and gender, age, race/ethnicity, and parents' education. When we run regressions with just the "main effects" of roommate behavior at baseline, we find the following estimates of the effects on time spent with roommate(s): for binge drinking, $B = 0.10$, SE = 0.10, $p = 0.33$; for smoking, $B = -0.06$, SE = 0.16, $p = 0.73$; for illicit drug use, $B = 0.08$, SE = 0.11, $p = 0.51$; for gambling, $B = 0.11$, SE = 0.11, $p = 0.32$; for multiple sex partners, $B = -0.02$, SE = −0.17, $p = 0.92$; for suicidal ideation, $B = -0.36$, SE = 0.23, $p = 0.13$; for non-suicidal self-injury, $B = -0.16$, SE = 0.13, $p = 0.24$.

among roommates only for students who themselves have engaged in the behavior. Also, these results are direct causal evidence of self-selection into friendship networks based on risky behavioral characteristics (in this case, a positive selection for binge drinking, drug use, and gambling, but a negative selection for smoking), and highlight the potential biases in analyses of peer effects that do not sufficiently control for such selection.

Fourth, we take a broader approach to estimating the determinants of closeness among roommates. We estimate regressions similar to those shown in Table 6, except with the addition of several other indicators of roommate similarity/difference at baseline: dummy variables for whether the roommates are of different race, binge drinking, smoking, drug use, gambling, multiple sexual partners, and sexual orientation; and the absolute value of the difference in religiosity, political orientation, parental education, standardized test *z*-score, average study hours, body mass index (BMI), frequency of exercise, tendency to disclose feelings, psychological distress score, and happiness score. Nearly all of these variables have negative coefficients, indicating that larger differences at baseline predict lower closeness of relationships, but only three factors are significant at $p < 0.10$: difference in drug use ($p = 0.003$), difference in religiosity ($p < 0.001$), and difference in happiness score ($p = 0.02$).[20]

Finally, we examine the predicted friendship level as a possible moderator of peer effects, in a cleaner version of the previous analysis that examined actual friendship level as a moderator. In this case, we do not find clear-cut evidence that peer effects are stronger for closer friends. For example, students with low (below the median) predicted friendships with their roommates experience only slightly smaller peer effects for binge drinking, as compared to students with high predicted friendships ($B = 0.08$ versus 0.11, and the difference is not significant in a regression with an interaction between predicted friendship level and roommate binge drinking). This analysis is limited, however, by the fact that predicted friendship levels have much less variation than actual friendship levels.[21] Our lack of clear evidence for differential peer effects by friendship closeness is consistent with a previous study using a different identification strategy (based on year-to-year continuity in residential co-location) that finds no evidence for larger academic peer effects among students who are more likely to be friends (Foster, 2006).

6. Discussion

In this study we estimate peer effects for risky behaviors using a natural experiment based on college roommate assignments. The analysis yields four notable findings. First, the estimated peer effects are significant for binge drinking, but not for smoking, illicit drug use, gambling, sexual activity, suicidal ideation, and non-suicidal self-injury. Second, in contrast to the study by Duncan et al., the peer effects for binge drinking are significant for not only men but also women, and are stronger for students who did not binge drink prior to college. Third, there is tentative evidence that peer effects for smoking may be positive for men and negative for women. Fourth, the matching of baseline substance use behaviors between roommates significantly predicts friendships.

The robust peer effects for binge drinking are consistent with the highly social nature of this behavior among young people, particularly college students. The null findings for smoking and illicit drug use are somewhat surprising, however, in light of the large effects estimated for secondary school students in other studies. Similarly, the null findings for suicidal ideation and self-injury are somewhat surprising, considering the widespread notion that suicide and self-injury can spread like a contagion (Prinstein et al., 2010; Velting and Gould, 1997). Although our estimates are not sufficiently precise to rule out meaningful effects, particularly for smoking, we can easily reject that the effects for substance use are as large as in the studies of secondary school students, most of which are in the range of a 0.30 or higher. Even our estimates for binge drinking are significantly lower than those in most studies of secondary school students. Our lower estimates could be due to either or both of two factors: (1) differences in true effects between secondary and postsecondary settings; (2) upward biases that might remain

[20] Other studies find that significant predictors of friendship among college students include similarity in race/ethnicity, socioeconomic background, political orientation, and academic performance (Marmaros and Sacerdote, 2006; Mayer and Puller, 2008; Foster, 2005).

[21] Baseline differences in roommate characteristics are too weak to be used as instruments in an instrumental variable version of this analysis (with differences in baseline roommate characteristics and their interactions with roommate risky behaviors as instruments for actual friendship levels and interactions with roommate behaviors). For example, the first-stage partial *F*-statistic is only 1.7 in this specification for binge drinking.

in empirical identification strategies used in the secondary school setting, where natural experiments such as conditionally random roommate assignments are not available.

There a few possible explanations for the differences in binge drinking effects in our study versus that of Duncan et al. The differences are probably not related to the campus settings: the university in their study is the same as university A in our study, which accounts for 69% of our sample, and our main pattern of results remains consistent when we restrict our analysis to university A (results available on request). The differences may be related to changes in drinking behaviors over time in college populations, however, given that our cohorts entered college 10–12 years later than those in their study. For example, the prevalence of binge drinking among college-age women has been catching up to that of college-age men (Grucza et al., 2009), and changes in norms may be shifting how women respond to peer behavior. Another possibility relates to the fact that Duncan et al. estimate the peer effects of the first-year roommates as of the second, third, of fourth year of college, whereas we estimate effects near the end of the first year. Peer effects on male binge drinkers may persist and even grow over the course of the college career because of the strong interrelationship between drinking and fraternity participation. Peer effects for the other three subgroups may dissipate, on the other hand, if fraternity/sorority participation does not mediate drinking outcomes to the same extent for those subgroups.

The discrepancies between our results and those of Duncan et al. need to be understood further, because they have different implications. Their results suggest that intervention strategies designed to leverage or mitigate peer effects should focus specifically on men with prior binge drinking, and also that avoiding the pairing of binge drinking men as roommates would reduce the overall prevalence of binge drinking. Our results, on the other hand, suggest that intervention focused on peer effects should pay roughly equal attention to men and women. Also, our results raise questions about whether the overall prevalence of binge drinking can be influenced by accounting for prior drinking when pairing roommates. Among women, the effect of a binge drinking roommate is similar for prior drinkers and non-drinkers. Among men, the roommate effect is larger for prior non-drinkers, suggesting that overall binge drinking might be lowered by pairing drinkers together and pairing non-drinkers together, but this difference in effects is not statistically significant ($p = 0.15$).

The apparent differences by gender that we find for smoking peer effects should be regarded as tentative and warranting further study, considering the lack of a clear reason to anticipate this difference a priori. If the difference is real, it may be related to differences by gender in motivations for smoking. For example, concerns about controlling body weight are frequently connected to smoking behavior among women, but less so among men (French and Jeffery, 1995). If this type of personal reason is less prominent for men, it may be that social context has a relatively larger role. In future work it would also be valuable to learn more about whether our estimate of an inverse peer effect among women is real. This estimate suggests that women have a negative reaction to being around a smoker, such that they become *less* likely to smoke. A better understanding of this scenario might be leveraged in peer-based interventions to reduce smoking.

Finally, the mutual affinity among roommates who engage in certain risky behaviors underscores the potential biases in analyses of peers who select each other. This finding also raises the question of whether the behaviors draw these peers closer together, or whether there are other characteristics correlated with the behaviors that draw the peers together. A related question is whether we have estimated pure spillover effects for binge drinking (in which binge drinking by one person directly affects the likelihood of binge drinking of another person), or if our estimates also reflect the effects of unmeasured peer characteristics that are correlated with binge drinking. The robustness of our estimates to controlling for a variety of roommate characteristics supports the former interpretation, but there still may be other characteristics, such as personality traits, that could be important. At a minimum, our estimates imply a true causal effect of living with someone who binge drinks. Also, our findings suggest a mutually reinforcing dynamic between binge drinking behavior and the closeness of friendships, and this angle will also be useful to investigate in future research. In addition, the interesting finding by which smokers actually tend to have less close relationships with a roommate who also smokes, as compared to a nonsmoker roommate, is also worth exploring further. This finding is present for both men and women, but might still be connected to some of the same factors that lead to negative peer effects for smoking among women.

Overall, the clearest conclusion from our study is that peer effects for binge drinking are important, regardless of gender or prior drinking behavior. Peer effects for the other risky behaviors, on the other hand, appear to be small at most. Our study also raises questions that warrant further exploration, particularly related to the surprising null effects for a number of behaviors, possible differences by gender in peer effects for smoking, and the relationship between risky behaviors and the formation of peer relationships.

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Appendix A. Baseline characteristics by sample attrition (examining nonresponse bias)

	Initial sample	Baseline respondents (BRs)	BRs w/roommate (RM) BRs	Final analytic sample (BRs who responded at follow-up, w/RM BRs)
N	4971	3501	2589	1641
Age	18.4	18.4	18.4	18.4
Female	0.50	0.50	0.51	0.53
Asian or Pacific Islander	0.15	0.16	0.16	0.16
Black	0.04	0.04	0.04	0.04
Hispanic or Latino	0.04	0.04	0.04	0.04
Other or multiple categories	0.07	0.06	0.07	0.06
White	0.70	0.70	0.69	0.70
U.S. citizen	0.91	0.92	0.91	0.92
Parents' education: less than college degree		0.16	0.16	0.16
Parents' education: college degree		0.28	0.28	0.27
Parents' education: graduate degree		0.56	0.56	0.56
Binge drinking (past 2 weeks)		0.37	0.36	0.34
Smoking (past 30 days)		0.07	0.07	0.06
Illicit drug use (past 30 days)		0.23	0.23	0.22
Gambling (past 30 days)		0.24	0.25	0.23
Multiple sex partners (past 6 months)		0.10	0.09	0.08
Suicide ideation (past 6 months)		0.03	0.03	0.02
Non-suicidal self-injury (past 6 months)		0.11	0.11	0.10

Note: None of the differences are significant across a single layer of attrition (from one column to the next one on the right); the difference in the proportion of females in the initial sample versus the final sample is significant, however (Z = 2.1, p = 0.04).

Appendix B. Conditional correlations among roommates at baseline (randomness checks)

	RM coefficient, w/o correction in Guryan et al. (2009)			RM coeff., with correction in Guryan et al. (2009)		
	β	SE	p	β	SE	p
Binge drinking (past 30 days) (0/1)	0.011	0.033	0.75	0.020	0.031	0.51
Smoking (past 30 days) (0/1)	−0.002	0.031	0.94	0.006	0.027	0.84
Illicit drug use (past 30 days) (0/1)	0.077	0.033	0.02	0.065	0.030	0.03
Gambling (past 30 days) (0/1)	−0.013	0.034	0.69	0.006	0.030	0.85
Multiple sex partners (past 6 months) (0/1)	−0.002	0.033	0.96	0.006	0.030	0.84
Suicide ideation (past 6 months) (0/1)	−0.005	0.024	0.85	0.006	0.021	0.77
Non-suicidal self-injury (past 6 months) (0/1)	−0.013	0.030	0.67	−0.003	0.028	0.91
Happiness score (0–9)	−0.001	0.033	0.98	0.014	0.029	0.64
Psychological distress score (0–24)	−0.013	0.034	0.70	0.006	0.030	0.83
Parents' education (highest attainment) (1–7)	0.041	0.034	0.23	0.025	0.031	0.79
Religiosity (0–3)	−0.005	0.032	0.87	−0.004	0.030	0.89
Exercise (frequency in past 30 days) (0–3)	−0.012	0.033	0.71	−0.016	0.031	0.60
Studying (time per day in high school) (0–5)	−0.037	0.034	0.28	−0.025	0.031	0.43
Admissions test (standardized z-score, SAT/ACT)	0.035	0.032	0.27	0.01	0.029	0.72
GPA in high school (standardized z-score)	0.037	0.032	0.25	0.037	0.029	0.21

N = 1053 (we only include one student per room, as explained in the text). Each row corresponds to a separate linear regression—for each regression only the estimate for the key coefficient on the RM variable is shown. All regressions include controls for the variables used for housing assignments, as described in the text.

Appendix C. Adjusting for multiple hypothesis testing across all health outcomes in the broader study

	Risky behaviors						
	Binge drinking	Smoking	Illicit drug use	Gambling	Multiple sex partners	Suicide ideation	Non-suicidal self-injury
Roommate behavior at baseline							
Average marginal effect	0.086	0.002	0.012	−0.004	−0.035	−0.001	0.0162
Standard error	0.024	0.026	0.025	0.023	0.031	0.033	0.024
p-Value	0.0004	0.950	0.616	0.875	0.259	0.964	0.503

	Mental health			Physical health	
	Happiness	Depression	Anxiety	Body weight (BMI)	Physical activity
Roommate behavior at baseline					
Average marginal effect	−0.020	0.012	0.053	0.013	0.047
Standard error	0.028	0.031	0.027	0.011	0.027
p-Value	0.483	0.712	0.049	0.230	0.080
Bonferroni p-value: risky behaviors		0.003			
Bonferroni p-value: mental health		0.147			
Bonferroni p-value: physical health		0.160			
Bonferroni p-value: health overall (all three domains)		0.005			

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