# Social contagion of mental health: Evidence from college roommates

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*From a policy standpoint the spread of health conditions in social networks is important to quantify, because it implies externalities and possible market failures in the consumption of health interventions. Recent studies conclude that happiness and depression may be highly contagious across social ties. The results may be biased, however, due to selection and common shocks. We provide unbiased estimates by using exogenous variation from college roommate assignments. Our findings are consistent with at most small contagion effects, with no evidence for happiness contagion, modest evidence for anxiety contagion, and modest evidence for depression contagion among men only.*

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## I. INTRODUCTION

Social interactions can affect health in many ways. A prime example is contagion, in which a disease or condition spreads among people in close contact. Contagion is economically important because it implies potential market failures due to externalities associated with behaviors and interventions. For example, the infectiousness of diseases such as influenza and HIV implies large positive externalities from treatment and preventive behaviors and interventions, and individuals do not necessarily account for these externalities in their decision-making.

This paper examines the contagion of mental health. In a sense, this is one of the most direct forms that an externality could take, because mental health is a fundamental indicator of wellbeing. Among children and young adults, mental disorders account for nearly half of the estimated burden of disease, measured as lost disability-adjusted life years (DALYs), in developed countries such as the United States (Michaud et al., 2006). Aspects of mental health are important in the development of human capital among children (Currie & Stabile, 2007; Heckman, Stixrud, & Urzua, 2006), and among adults mental disorders are important negative predictors of economic outcomes such as employment and earnings (Ettner, Frank, & Kessler, 1997), as well as social outcomes such as marital stability (Kessler, Walters, & Forthofer, 1998).

Recent studies in the medical literature conclude that mental health may be highly contagious, much like infectious diseases. These studies find that, controlling for a range of factors, changes over time in both depression (Rosenquist, Fowler, & Christakis, 2011) and happiness (Fowler & Christakis, 2008) are strongly correlated within friends, spouses, siblings, and neighbors. The striking magnitude of these estimates—e.g., having a happy next-door neighbor is associated with a 34% increase in the probability of being happy—has generated considerable attention in the media and scientific community around the idea that mental health "spreads through social networks…like a virus" (Boyles, 2008).

The major caveat to these studies of mental health contagion, as well as most studies of social contagion in general, is that there are clear sources of potential bias in the estimates. People choose where they live and work, and with whom they interact, and they may share characteristics with others in their social network that lead to similar outcomes. Also, shared contextual factors such as neighborhood characteristics may contribute to similarities in outcomes. To the extent that these shared factors are unobserved or insufficiently measured, estimates of correlated outcomes within social groups are likely to be biased away from zero, relative to the true causal effects of social interactions.

In this paper we provide unbiased estimates of mental health contagion using the natural experiment based on college roommate assignments. At the universities in our study the roommate assignment process is based on predetermined algorithms using a known and observed set of variables. Among students with identical values in these assignment variables, any variation in roommate characteristics at baseline (prior to the school year) should be exogenous, and our checks of the data support this assumption. Therefore, the association between a roommate's mental health at baseline and one's own subsequent mental health, conditional on the variables used in the assignment process, can be interpreted as an unbiased causal effect.

Overall we find mixed evidence regarding the contagion of mental health among roommates. In contrast to the recent research described above, we find that happiness does not exhibit significant contagion. On the other hand, poor mental health—measured as general psychological distress, depression, and anxiety—appears to be modestly contagious, although the depression contagion is only significant for men. In addition, to enhance the interpretation of our results we use data on reported interactions among roommates, and we find that the weakness of the contagion effects cannot be explained by avoidance of roommates with poor mental health or by generally low social contact among roommates. Overall our results suggest that mental health contagion is lower, or at least more context-specific, than implied by the recent studies in the medical literature.

The rest of the paper is organized as follows. In section II we describe conceptual reasons why mental health contagion might occur and empirical evidence from prior studies. In section III we describe our empirical approach and data. In section IV we describe our results, including subgroup analyses motivated by our conceptual framework and an investigation of how mental health relates to the reported closeness of roommate relationships. In section V we conclude with interpretations of results and ideas for future research.

## II. BACKGROUND AND RELATED LITERATURE

Mechanisms for contagion of mental health

Most conceptual discussions of social contagion effects,[1] particularly in the economics literature, focus on behavioral outcomes such as crime or substance use. For example, Glaeser and Scheinkman (2001) emphasize mechanisms for contagion such as acquiring information, modifying preferences, and possibly modifying prices (e.g., decreasing the price of acquiring an illegal drug). Mental health conditions such as depression and anxiety, by contrast, have behavioral aspects but are not behaviors. To conceptualize contagion for conditions such as these, one could think of a health production function mapping inputs into health (Grossman, 1972). The mental health of social contacts would simply be another input, in addition to standard inputs including health in the prior period, health-related behaviors, and health services. In estimating the marginal product of social contacts' mental health (i.e., the contagion effect), the empirical challenge is that this input is likely to be correlated with unobserved factors that are also inputs into mental health (as we discuss in more detail later).

Social contact with a person in poor mental health could be an input into one's own mental health through a variety of mechanisms, most of which have been discussed in the psychology literature.[2] Although our empirical analysis cannot fully disentangle these mechanisms, we briefly review them here in order to provide context for our analysis. First, by imagining oneself in the position of the other person with poor mental health (i.e., empathizing), one might experience some of the same stressful and negative emotions (Hatfield, Cacioppo, & Rapson, 1993). Next, one may feel compelled to offer the person support—which may feel rewarding and improve mental health, or feel taxing and reduce mental health—and the other person may also be less capable of providing support in return (Joiner & Katz, 1999). In addition, the other person may not be enjoyable to be around, which may in turn decrease one's

[1] Other common terms in this literature are "social interaction effects, "peer effects," and "spillover effects." In this paper we use the term "contagion" because it describes more specifically what we are examining: "the transmission of a disease by direct or indirect contact," according to the Webster dictionary. We are using "contagion" as shorthand for what economists have termed *endogenous social interaction* effects (Manski, 1993), in which variable A in one person affects variable A in another person. Social interaction, peer, and spillover effects often have broader meanings, because they can also describe situations where variable A in one person affects variable B in another person.

[2] We illustrate these mechanisms for contagion using the example of poor mental health, but many of these points would apply analogously to good mental health or happiness.

mental health (Hokanson, Rupert, Welker, Hollander, & Hedeen, 1989). Furthermore, depression in particular may exhibit contagion due to negative attributions (e.g., interpretations of recent events) that are developed collaboratively, negative feedback about oneself from a depressive other, and negative attributions about the depressive other's behavior (Joiner & Katz, 1999). On a more primitive level, a variety of experiments show that people tend to unconsciously mimic facial expressions, voices, movements and behaviors of those around them, and these physical expressions affect emotions (Hatfield et al., 1993). Finally, contagion might occur via social comparisons. People may make "upward" comparisons with more "successful" people in order to identify themselves with those people, but these comparisons may also cause envy or a decrease in self-esteem (Exline & Lobel, 1997). Also, "downward" comparisons may provide temporary relief (by showing that one's situation could be worse), or may cause guilt and defensiveness. Collectively, these ideas suggest that, in theory, a social contact's mental health can have positive or negative effects on one's own mental health; the direction and magnitude of the effects are open empirical questions.

As we explore in our empirical analysis, the contagion effect may also vary substantially across types of individuals. In particular, the contagion effect may depend crucially on styles of social interaction and emotional expression. People who openly disclose their emotional distress to others may "transmit" their mental health differently than people who are more withdrawn and reserved. The psychology literature on "co-rumination" suggests that frequent discussion focusing on negative interpretations of distressed thoughts and emotions can exacerbate the level of distress among all people in the discussion (Kennedy-Moore & Watson, 2001). On the other hand, suppressing one's expression of emotional distress can create distance in interpersonal relationships and lead to negative psychological effects on both the self and others (Butler et al., 2003). Thus, verbally expressing one's emotional state can either increase or decrease the contagiousness of distress, depending on the nature of the expressions and discussions (Kennedy-Moore & Watson, 2001).

These factors suggest that mental health contagion is also likely to differ by gender. Women are generally more likely than men to discuss their emotions with other people (Kahn & Garrison, 2009) and less likely to suppress emotional expression (Gross & John, 2003). These differences imply that contagion of poor mental health could either be higher or lower among women, depending on which moderating effect dominates—the exacerbating effects of co-rumination or the buffering effects of lower suppression.

In addition, the degree of contagion may depend on one's own mental health. People with poor mental health may be more susceptible to contagion, because they may have less ability to cope with the stress of being around someone else with poor mental health. This type of logic underlies the belief in the mental health field that intervention can sometimes be more effective if provided at an earlier stage in the progression of mental illness (Curry, Rohde, Simons, & et al, 2006). On the other hand, peers who are each experiencing emotional distress might be able to offer greater empathy and support for each other, which is the basis for peer support groups (Davidson, Chinman, Sells, & Rowe, 2006).

Previous empirical studies on mental health contagion

The empirical literature on mental health contagion is small,[3] but as noted earlier, two recent studies conclude that the contagion effect is large. These studies analyze data from the Framingham Heart Study, which collected extensive health-related information over several decades from a panel of adults in Framingham, Massachusetts. In the first study, Fowler and Christakis (2008) find that happiness is highly correlated over time among social contacts, conditional on a variety of covariates. For example, having a nearby friend who becomes happy is associated with a 25% increase in the probability of being happy, and the analogous estimate for having a next door neighbor who becomes happy is 34%. In the second study, Rosenquist, Fowler, and Christakis (2011) find even larger correlations in depression within social ties; for example, having a close friend who is depressed is associated with a 118% increase in the likelihood of one's own depression.

The conclusions of these and most other studies of social contagion, however, must be tempered by the well-known identification issues, as described by Manski (1993). The three main issues are: 1) the reflection problem, in which the effects of others on the self are difficult to disentangle from the reverse; 2) selection on unobserved variables, in which people select into social networks on the basis of shared and unobserved characteristics that lead to similar

[3] A large number of experimental studies show that emotions can be temporarily induced through exposure to another person's emotional expression (e.g., see review in Hatfield et al, 1993), but this is very different from demonstrating that more enduring states of mental health, such as depression, are contagious.

outcomes; and, 3) common shocks, in which shared and unobserved contextual factors (such as characteristics of the organization or neighborhood) lead to similar outcomes.

In the analyses of data from the Framingham Study, there are a number of open questions related to these identification issues.[4] To address selection, the studies control for lagged measures of mental health for both the reference individual and the individual's social contacts, but this approach rests on the assumption that selection into social networks is not based on unobserved factors that affect future changes in mental health. Factors such as self-esteem or personality type may threaten this assumption, for example. To address concerns about common unmeasured shocks, the authors argue that their estimates of larger effects among "reciprocated" friendships (in which two sample persons each note the other as a friend), as compared to one-sided or "unreciprocated" friendships, implies that unmeasured shocks are not driving the results (or else the estimated effects would be similarly large in both cases). A potential problem with this logic is that reciprocated friends may experience shared, unmeasured contextual factors to a greater extent, since they are likely to be closer friends. In addition to these issues, the reflection problem is also relevant, given that the empirical strategy estimates conditional correlations of contemporaneous changes.

Studies of peer effects among college roommates

Our study capitalizes on an opportunity to address these identification issues in the context of peer relationships, by examining the natural experiment in which college roommate assignments are made based on a known set of variables. Our approach builds on a literature that has mainly used this natural experiment to examine academic outcomes. Although academic peer effects are likely to operate very differently than mental health contagion, previous results from this literature have potential implications for our study. Several of the studies find evidence for heterogeneous effects; for example, Sacerdote (2001) and Zimmerman (2003) find significant

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[4] We are not the first to raise these issues for these studies. The authors themselves acknowledge the issues in their original papers, and Cohen-Cole and Fletcher provide a critique on two levels using data from the National Longitudinal Survey of Adolescent Health (AddHealth). First, Cohen-Cole and Fletcher (2008b) show that, in the context of AddHealth data, the basic empirical approach is sensitive to the inclusion of school fixed effects (to help address unobserved contextual factors) and individual fixed effects (to help address selection into friendships). Second, Cohen-Cole and Fletcher (2008a) show that the empirical approach produces apparent peer effects that are arguably implausible (for height, acne, and headaches). It is important to keep in mind, however, that the biases highlighted by Cohen-Cole and Fletcher probably vary by outcome and setting, and they did not examine mental health.

effects for some but not all subgroups defined by initial academic ability, and Stinebrickner and Stinebrickner (2006) find strong evidence for peer effects for women but not for men. In addition, social behavior helps explain academic peer effects among roommates, which is notable for our context because social behavior is often correlated with mental health. In particular, Kremer and Levy (2003) find that male students who binge drank in high school have lower GPAs if they are paired with a roommate who also binge drank during high school, and Stinebrickner and Stinebrickner (2008) find that assignment to a roommate who brings video games to college causes less studying and lower grades.

A notable advantage of studying peer effects for mental health outcomes among college roommates, as compared to academic outcomes, is that college students have a wide distribution of mental health levels, whereas college admissions restrict the distribution of academic ability by design. There are two previous studies in the psychology literature that specifically examine mental health contagion among randomly assigned college roommates. Sanislow et al (1989) find that having a roommate with depression plus other psychopathology predicts mood disturbance, and Howes et al (1985) find that being assigned to a roommate with persistent mild depression is associated with an increase in one's own depressive symptoms. These studies provide interesting suggestive evidence, but they share two key limitations. First, they define the roommate's mental health based on measures taken after the students have been living together for several months, which means that their estimates are subject to the identification problems of reflection and common shocks. Second, they have low precision due to sample sizes of 51 and 44 roommate pairs, respectively.

## III. EMPIRICAL APPROACH AND DATA

### Overview

Our data come from online surveys of first-year college students. We conducted the surveys 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 in a later section.

Our main empirical approach builds on the framework of previous studies of peer effects among college roommates, by estimating linear regressions of the form:

$$(1) \qquad MH_{(t+1)} = \beta_0 + \beta_1 Prefs_t + \beta_2 RoommateMH_t + \beta_3 MH_t + \beta_4 X_t + \varepsilon_{t+1}$$

The subscript *t* denotes a measurement in the baseline survey, and *t+1* denotes a measurement in the follow-up survey. *MH* refers to a mental health measure, *Prefs* is a vector of housing preferences and all other variables used to make roommate assignments (described in more detail later), *RoommateMH* is the mental health of the roommate(s), and *X* is a vector of individual characteristics including gender, age (exact to the day), race/ethnicity, and parents' education. The key coefficient is $\beta_2$, which represents the effect of roommate mental health on the individual's mental health. Heteroskedasticity-robust standard errors are corrected for correlated outcomes among roommates.

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,[5] 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

[5] We chose the \$10 cash "pre-incentive" (not conditional on participation) based on survey methods research generally indicating that this is as effective as "post-incentives" (awarded only after participation), at least for relatively small amounts (Sanchez-Fernandez, Munoz-Leiva, Montoro-Rios, & Ibanez-Zapata, 2010). Also, the "pre-incentive" has the advantage of not requiring an additional letter or email for delivery of the incentive.

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 equation (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.[6],[7] Prior to the baseline survey, the initial number of eligible students with assigned roommates was 4,971, including 3,876 from university A and 1,095 from university B (which has a large proportion of first-year students in single rooms, unlike university A). A total of 3,501 (70%) of these students completed the baseline survey. Among baseline responders, 2,589 (74%) had at least one roommate who was also a baseline responder. And among baseline responders with at least one roommate baseline responder, 1,641 (63%) completed the follow-up survey.[8]

Because our primary analytic sample is only 33% (1,641/4,971) of the initially eligible sample, it is important to examine potential biases related to survey non-response. The first two

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[6] 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.

[7] 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.

[8] 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.

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 unrelated to mental health measures and other characteristics. Despite our reasonably large sample size, the only statistically significant difference across layers of attrition is a slightly higher proportion of women in the final analytic sample (0.53) as compared to the initial sample (0.50). Also, we find (in regression results not shown) that, conditional on gender, whether a student responds at follow-up is not significantly associated with own or roommate mental health at baseline.

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 mental health issues that are relatively common among adolescents and young adults, and we focus on mental health "scores" rather than binary measures in order to maximize statistical power. We employ widely-used brief screens that have been shown in previous studies to correlate highly with diagnoses by clinicians and longer diagnostic questionnaires. Depressive symptoms are measured by the PHQ-2 instrument (Löwe et al., 2005), which asks about frequency in the past two weeks of the two core symptoms of major depression, anhedonia ("little interest or pleasure in doing things") and depressed mood ("feeling down, depressed or hopeless"), and is scored on a scale of 0-6. Overall psychological distress is measured using the K-6 instrument (Kessler et al., 2003), which is scored on a scale of 0-24. The K-6 asks about frequency in the past 30 days of feelings related to depression and anxiety: feeling "nervous," "hopeless," "restless or fidgety," "so depressed that nothing could cheer you up," "that everything was an effort," and "worthless," We also use the two K-6 items specific to anxiety (feeling "nervous" and "restless or fidgety") as a proxy for anxiety level, scoring these on a 0-8 scale. Finally, on the positive side of mental health we measure happiness using three of the same items from the Center for Epidemiologic Studies Depression Scale (CES-D) that Fowler and Christakis (2008) use in their study of happiness contagion. These questions ask about frequency in the past week of "I felt hopeful about the future," "I was happy," and "I enjoyed life." We added these items for a happiness score of 0-9.

As shown in Table 1, mental health appears to be good on average in our sample, but a substantial minority of students has significant symptoms of depression and anxiety. Also, mental health generally becomes worse between the baseline and follow-up surveys, with statistically significant increases in depression and anxiety and a decrease in happiness. The within-student correlation in mental health scores over time (from baseline to follow-up) is 0.38 for depression, 0.42 for anxiety, 0.48 for psychological distress, and 0.45 for happiness. This suggests that baseline mental health is a good but far from perfect predictor of mental health during the academic year when roommates live together.[9]

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 mental health), conditional on the variables that explicitly determine the assignments, should be uncorrelated with the error term in equation 1.

[9] One might argue that our main results are conservative estimates, because we focus on the effect of a roommate's mental health measured prior to the academic year, which is an imperfect predictor of a roommate's mental health during the academic year. An alternative empirical strategy would be to use the roommate's mental health at baseline as an instrument for the roommate's mental health at follow-up. One would expect this approach to yield coefficients approximately twice the magnitude of our main results, given that the within-person correlation between baseline mental health and follow-up mental health is close to 0.5 for most measures. We find that this is indeed the case for most estimates of mental health contagion, when we implement this IV approach using two-stage least squares. These IV estimates for happiness and depression contagion are not appreciably closer to those from the Framingham study, however, which is not surprising given that the reduced form estimates (in Table 2) are very close to zero (and in fact negative for happiness). The IV estimates are considerably less precise than our main estimates, because the IV approach reduces the useable sample size due to the need for roommate survey data at follow-up. Aside from this practical consideration, we think that our main approach generates a more meaningful approximation of spillover effects that might result from mental health interventions, because our estimates can be thought of as a lasting effect (what would we expect person B's mental health to look like several months later, if we manipulate person A's mental health?) whereas the IV estimates can be thought of as a more instantaneous effect (what would we expect person B's mental health to look like this week, if we manipulate person A's mental health?).

At university A, a public school with approximately 6,000 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/she is a smoker, a non-smoker, or someone who wants to live in an entirely substance free residence). Where 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 the vast majority of students (89%) who submitted 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). The remaining 11% of students who missed the deadline are assigned in the order in which their housing applications are received (i.e., 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 preferences noted above, for the vast of majority of students, whereas the date of application submission needs to be controlled for as flexibly as possible for the remaining students.[10]

At university B, a private school with approximately 4,000 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 the variables used and we know from 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.

[10] We find that the main patterns of results are consistent when we restrict the analysis to the sample with conditionally random assignment, as compared to the full sample. Also, as described in this section, the full sample passes the exogeneity checks. Therefore we report results for the full sample, which is considerably larger and yields more precise estimates.

The key assumption in our empirical strategy is that the roommate's baseline characteristics (particularly mental health) are uncorrelated with the error term in the regression in equation 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 key 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.[11,12] To control for 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 answer choices for each variable.[13]

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 equation (2) below for each mental health variable that we consider as an outcome in this paper, as well as several other characteristics that are or might be related to mental health (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 implemented this by adding a control for the average value of the key variable (mental health or other characteristic) among

[11] These regressions only include one (randomly selected) student per room, as recommended by Kremer and Levy (2003), to avoid an artificial negative correlation that can occur when controlling for many preference-combination strata.
[12] In order to pool the samples from the two universities, despite the different assignment processes and variables, we code each assignment variable as a set of categorical dummies including a missing/not applicable category. Note that collectively these assignment variables control for whether a student is at university A or B (the linear combination of missing/not applicable dummies is the same constant within each university).
[13] We cannot include these variables in constructing the dummy variables corresponding to combinations of preferences, because we would have far more dummy variables than observations in the data set.

other students with an identical combination of values for the primary housing variables (i.e., the pool of potential roommates).[14]

$$(2) \qquad MH_t = \beta_0 + \beta_1 Prefs_t + \beta_2 RoommateMH_t + \varepsilon_t$$

We find, as expected, that the estimates of $\beta_2$ are close to zero for all outcome variables, and none of the estimates are significant at $p<0.05$ (Appendix Table B).[15] The correction from Guryan et al. does not change the estimates appreciably, which is not surprising given that they demonstrated that the correction has little impact in larger samples.

## IV. RESULTS

Contagion in the overall sample

Table 2 contains the results of the estimation of equation (1) for the overall sample of assigned roommates; each row shows the key coefficient, $\beta_2$, from a separate regression. We find significant contagion effects for the general index of psychological distress and for anxiety symptoms, but not for depression or happiness. The significant effect for general psychological distress appears to be driven by the anxiety symptoms, as the effect for the score calculated from the other items (which are essentially depressive symptoms) is smaller and not significant (0.02, SE=0.03). Though statistically significant, the anxiety contagion is modest in size: a 0.05 point increase for every one point increase for the roommate(s). The null results for depression and happiness are precise zeros in the sense that the 95 percent confidence intervals include only small effects (upper bounds of 0.07 and 0.04 respectively). For a point of reference for the magnitudes of these coefficients, consider that the coefficients on own baseline mental health are the following (all significant at $p<0.01$): 0.51 for happiness; 0.57 for psychological distress; 0.42 for depression; and 0.46 for anxiety.

[14] This correction is intended to address the fact that, when a pairing is made from a small group of people, a person with a high baseline value in the variable of interest is drawing a match from a subgroup that is appreciably below the group mean (because of the index person's nontrivial contribution to the mean).

[15] Also, when we expand these checks to all 33 measures available from our baseline survey, we again find that all estimates are close to zero (ranging from -0.09 to 0.07) and only three are significant at $p<0.10$ (including two negative and one positive), which is what we would expect due to chance.

Contagion by gender and baseline mental health

As noted earlier, there are reasons to expect different contagion effects by gender, and the sign of these differences is ambiguous a priori, due to offsetting factors. As shown in Table 3, we do in fact observe some differences. Most notably, there is a significant contagion effect for depression among men, but not among women, and this difference by gender is significant (p=0.01). Within gender we also examine whether susceptibility to contagion depends on one's own baseline mental health, by stratifying the sample at a binary cutoff for each baseline mental health measure.[16] As described earlier, this analysis is motivated by the idea that students with poor mental health at baseline may have less ability to cope effectively with being around another person in poor mental health. We find evidence consistent with this for depression contagion among men. Depression is transmitted from depressed roommates primarily to men with pre-existing depression, and the effect experienced by this subgroup is large (0.22, SE=0.13). Among women, by contrast, students with poor mental health appear, if anything, to do *better* when paired with roommates who also have poor mental health—for this subgroup we estimate a *negative* coefficient for depression (-0.17, SE=0.11), meaning that women who are depressed at baseline become *less* depressed if their roommate is also depressed at baseline. This may reflect mutual support that results from the higher tendency of women to disclose their feelings, as discussed earlier.[17]

Depression contagion by distress disclosure of roommate

To investigate further the contagion of depression specifically, we included a question in the baseline survey about the tendency to disclose depressed feelings. As discussed earlier, the net moderating effect on contagion of disclosing emotional distress is an open empirical question. On the one hand, higher disclosure could augment contagion by making the roommate's depression more salient and perhaps more burdensome, and may lead to co-

[16] We use a cutoff established as an indicator of a probable depressive disorder in validation studies of the PHQ-2 screen for depression (Löwe, Kroenke, & Gräfe, 2005). For the overall K-6 score we use a cutoff of 8 rather than the standard cutoff of 12 (Kessler et al., 2003), because the latter is intended to focus attention on *severe* mental illness (and is only met at baseline by 4.3% of our sample) rather than mental health problems more generally. We use a cutoff of 4 for the anxiety subscore because that corresponds to approximately 20% with positive screens, which is similar to the estimated prevalence of anxiety disorders among college students (Blanco et al., 2008).

[17] Another possibility is that women are more likely than men to compare themselves to people around them when self-assessing their mental health. Given that assessments of mental health necessarily depend on self-reports, there is no way to distinguish this possibility from "true" relief from being around others who are also struggling emotionally.

rumination. On the other hand, disclosure could reduce contagion by reducing misunderstandings about the reasons for the depressed mood and associated behavior.[18] For this analysis we limit the sample to students in double rooms—this allows for a cleaner interpretation of the roommate's disclosure measure.[19] We measure the disclosure tendency in the baseline survey using an item from the Distress Disclosure Index (Kahn & Hessling, 2001), which asks "How much do you agree with the following statement: 'When I feel depressed or sad, I tend to keep those feelings to myself.'" We code students as "disclosers" if they answer strongly disagree or disagree, "non-disclosers" if they answer strongly agree or agree, and neither if they answer "neither agree nor disagree." By this definition, among women there are 23% disclosers, 58% non-disclosers, and 20% neither, and among men there are 15% disclosers, 66% non-disclosers, and 18% neither.

As shown in Table 4, the estimated contagion effects of depression are higher from non-discloser roommates among both men and women, although these differences across roommate discloser status are not statistically significant. Among women with disclosing roommates, having a depressed roommate actually appears to *reduce* one's own depressive symptoms.

<u>Closeness of roommate relationships and mental health</u>

As another approach to enhancing the interpretation of our contagion estimates, we examine a number of measures of the closeness of roommates' relationships. These measures illustrate the extent and nature of the social contact in the natural experiment we are evaluating. We also use these measures to look at whether students avoid contact with roommates with poor mental health (which would presumably mitigate contagion), and to look at whether similarity in mental health at baseline predicts closer relationships (which would be evidence of endogenous sorting based on mental health variables).

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[18] As noted earlier, there is a considerable literature on the psychological benefits of disclosing emotions (see, e.g., Kennedy-Moore and Watson (2001)). Our data also suggest that students who disclose their depressed feelings are more likely to experience improvements in their depression, as higher disclosure tendency at baseline is significantly associated with reduced depression from baseline to follow-up.

[19] In scenarios with multiple roommates one could imagine a large variety of hypotheses related to different combinations of depressed and disclosing/non-disclosing roommates. Examining the average disclosing tendency among roommates is probably not appropriate, for example, since the individual-level interaction between disclosure and mental health may matter. Given the number of potential hypotheses and the fact that our sample size is not large enough for precise estimates comparing the many alternative combinations, we do not pursue this analysis.

Our follow-up survey contains five questions about the level and type of interactions among roommates during the academic year. Students with multiple roommates are asked to think about their roommates on average. The questions ask respondents to report: 1) how much they agree that they are close friends with their roommate(s); 2) how much time, on average per day, they spent doing things or hanging out with their roommate(s) during the academic year; 3) how much they agree that they enjoy spending time in the room with their roommate(s); 4) how often they discussed their own personal problems with their roommate(s) during the academic year; 5) how often they discussed the personal problems of their roommate(s) with the roommate(s) during the academic year.

We find substantial variation in the responses, as shown in Table 5. In our primary sample (students with assigned roommates), students are relatively dispersed across the spectrum from strongly disagreeing to strongly agreeing that they are close friends with their roommate(s). Similarly, answers are dispersed for questions about amount of time spent together, enjoyment of being in the room together, and frequency of discussing personal problems. This underscores the nature of the "treatment" in this natural experiment: assigned roommates interact frequently and closely in many but far from all cases.[20] As a point of comparison, our smaller sample of students with requested roommates, as expected, has more frequent and enjoyable interactions on average. Also, comparisons by gender indicate that women are slightly more likely than men to be at either end of the spectrum for closeness of friendship, time spent together, and enjoyment of being together, and women report a substantially higher frequency of discussing each other's personal problems.

Next, we examine how the mental health of roommates affects the reported closeness of relationships with those roommates, by estimating regressions described by equation 3 below. In these regressions we examine the effect of not only roommate mental health per se but also the dissimilarity of roommate mental health from one's own mental health. This latter variable is

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[20] This is generally consistent with a number of prior studies, which find that assigned roommates have a relatively large but highly varying degree of interaction. Marmaros and Sacerdote (2006) find that freshman roommates exchange 45 times more emails than two freshmen not in the same dorm, and even as seniors, former freshman roommates exchange 9.8 times more emails than two randomly chosen seniors. Foster (2005) finds that the most important factor determining students' choice of roommates in their second year in college is whom they were assigned to be live with (roommates and hallmates) in the first year. In addition, Stinebrickner and Stinebrickner (2006) collect detailed time-use data and find that students spend 21.7 waking hours per week with their randomly assigned roommate, and 47% of students spend more time with their roommate than any other student. At the same time, they find that only 37% of students list their roommate as one of their best four friends.

measured as the absolute value of the difference in mental health score at baseline between the self and the roommate. The idea is that students may interact less with and become less close to roommates who are less like them in terms of mental health.[21] This analysis is restricted to students in double rooms to allow for a cleaner interpretation of differences among roommates in baseline mental health. Table 6 reports results from linear regressions, which yield very similar patterns to ordered probit regressions (results available on request).

$$(3) \qquad Close_{t+1} = \beta_0 + \beta_1 prefs_t + \beta_2 rmMH_t + \beta_3 \left|(rmMH_t - MH_t)\right| + \beta_4 X_t + \varepsilon_t$$

As shown in Table 6, dissimilarity in mental health, particularly happiness and depression, is a significant negative predictor of being close friends, time spent together, and enjoying time together, whereas the absolute level of roommate mental health (whether measured as happiness, depression, or anxiety) does not independently predict closeness. When we stratify this analysis by gender (not shown in table), we find that the effect of dissimilarity on each measure of relationship closeness is larger for women than men, although these differences by gender are not statistically significant.[22] Even for women, however, the effects are modest in size: for example, being one standard deviation apart from one's roommate in terms of baseline depression corresponds to a 0.12 standard deviation decrease in reported closeness of friendship.

As another way of examining the role of closeness between roommates, we also look at whether closeness is a moderator of the contagion effects. We might expect, for example, that contagion is higher among roommates who spend more time together or report being close friends. To evaluate this, we estimate our main regressions separately for students who report being close friends with their roommate versus those who do not, and separately for students who report spending an hour or more per day with their roommate versus those who spend less time. This is a potentially biased moderator analysis, given that we are stratifying based on an endogenous follow-up measure rather than a baseline measure, but it may be nonetheless informative. We find (in results available on request) that the difference in contagion estimates

[21] This analysis is analogous to that by Marmaros and Sacerdote (2006), in which they estimate how baseline similarities and differences among students predict the number of emails exchanged (as a proxy of friendship). For example, they find that the absolute difference in SAT scores is a significant though modest negative predictor of friendship.

[22] This appears to align with our previously discussed finding that female roommates with pre-existing mental health problems tend to experience improved mental health, if anything, by being paired together.

is very small and statistically insignificant across subgroups defined by closeness of friendship or time spent together.

Collectively, our results related to the closeness of roommates have two notable implications. First, the overall weakness of contagion effects in our study cannot be explained by the avoidance of roommates with poor mental health. Other things equal, students spend just as much time with and are just as close to roommates in poor mental health,[23] and contagion effects do not appear to vary by closeness of friendships or time spent together. Second, roommates with similar mental health are somewhat more likely to become close, which underscores possible selection biases in studies of contagion based on endogenously formed social networks.

Alternative specifications and sensitivity checks

In addition to varying across types of individuals, mental health contagion may be nonlinear with respect to the mental health scores we use in our analysis. For example, depression contagion may occur primarily once a roommate's level of depression reaches a certain threshold. We examine this possibility by coding roommate mental health as a set of dummy variables corresponding to several intervals within the ranges of scores for happiness, depression, anxiety, and overall psychological distress. These results (available on request) do not indicate any significant nonlinearities, however, and are generally imprecise due to the small numbers of students in some intervals in the mental health scores.

We also check whether our results are affected by controlling for additional roommate characteristics beyond mental health. Characteristics correlated with mental health, rather than mental health per se, could be contributing to the estimates that we have been referring to as contagion effects. There is no way to rule this out definitively, but examining the sensitivity of our results to the inclusion of additional roommate covariates provides suggestive evidence that this is not the case. In particular, in our main regression (equation 1) we add controls for the

[23] A minor caveat is that roommate baseline mental health is a small but significant predictor of some of the closeness measures when we do not also include dissimilarity in mental health in the regressions. For example, roommate depression score is associated with a 0.07 reduction (SE=0.04) in the closeness of friendship measure and a 0.08 reduction (SE=0.05) in the measure of time spent together. The small magnitude of these estimates, combined with the fact that they are even smaller and insignificant when dissimilarity in mental health is controlled for, suggests that avoidance of roommates with poor mental health is at most a small factor.

following roommate characteristics measured at baseline: parents' education (highest level obtained by either parent); how religious one is (very, somewhat, a little, not at all); frequency of binge drinking in the past 30 days; frequency of exercise in the past 30 days; average hours per day spent studying in the last year of high school; standardized admissions test score (total ACT and/or SAT, converted to a z-score based on the within-school distribution); and high school GPA (also converted to a z-score). We find that our main results, both overall and by gender, remain nearly identical after adding these controls, indicating that our estimates are not being driven by other roommate characteristics such as these. While it is still possible that other unmeasured characteristics may affect our estimates in either direction,[24] the robustness of our estimates to a broad set of controls suggests that we are largely picking up true contagion effects.

In addition, we estimate the contagion effects separately by university, to check the consistency across two different settings. The results are nearly identical between university A and university B: in both cases the contagion estimates are close to zero for happiness and depression, and small and positive for anxiety ($B$=0.05 and SE=0.03 for university A; $B$=0.06, SE=0.05 for university B). Although our sample only includes two universities, the consistency between the two is at least suggestive that the findings may generalize to other university settings.

Finally, we examine the effect of hallmates' mental health, where hallmates are defined as students who live on the same floor within one's residence. These estimates (shown in Table 7) are considerably less precise than the roommate effects, because there is much less variation in average hallmate mental health (hallways typically consist of 20-40 students). In specifications controlling for roommate mental health, we find that hallmate mental health does not generate statistically significant contagion, although the lack of precision prevents us from ruling out sizeable effects.

[24] For example, personality characteristics such as extroversion and neuroticism are known to be associated with mental health (Kendler, Gatz, Gardner, & Pedersen, 2006), and would be useful to examine more closely in future studies.

## V. CONCLUSION

This study provides novel evidence on the contagion of mental health, using a natural experiment in which social contacts vary according to conditionally random assignment. Overall we find mixed evidence regarding the presence and strength of contagion. We obtain relatively precise null results for the contagion of happiness. On the other hand, we find modest evidence that poor mental health is contagious: anxiety exhibits a small but significant contagion, as does depression for men only. We also find suggestive evidence that depression is more contagious when the depressed person tends not to disclose his or her feelings. Collectively, our results indicate that the contagion of mental health may be weaker and more specific than suggested by recent studies in the medical literature.

When drawing overall conclusions from our study, it is important to consider that we evaluated a number of hypothesis tests pertaining to related issues. Adjustments for multiple hypothesis testing may be appropriate, depending on the type of question one is asking (Schochet, 2008). In our context, given the different natures of the mental health conditions measured in the study, one could consider each hypothesis test as pertaining to a separate issue, in which case adjustments for multiple testing would not be appropriate. In this perspective, however, it is important not to give disproportionate attention to significant results (such as the anxiety contagion we found) as compared to null results. Alternatively, if one is evaluating an overarching question such as whether mental health is contagious in any way, adjustments for multiple testing are clearly necessary. With these adjustments we would not be able to reject the null hypothesis of no contagion at conventional significance levels.[25] Furthermore, our subgroup results should be viewed as exploratory rather than confirmatory (Schochet, 2008), given that we examined a number of subgroups and we had ambiguous a priori hypotheses. Overall, these considerations underscore that the main story of our results is the overall weakness of contagion effects, at least as much as it is the presence of significant results for certain measures and subgroups.

[25] The most parsimonious approach to this global hypothesis would be to focus on two outcome measures, happiness and psychological distress. The latter is a reasonable composite measure of poor mental health, given that it reflects a combination of depressive and anxiety symptoms, as discussed earlier. Thus, we would have two hypothesis tests pertaining to an overall hypothesis that mental health is contagious. Standard adjustments for multiple testing such as the Bonferroni correction, or even a less conservative control for the false discovery rate such as Simes' procedure (Benjamini & Hochberg, 1995), would imply that the p-value for the contagion estimate for psychological distress should be approximately twice as large as shown in Table 2, which would be 0.14.

Related to this point, our estimates are clearly smaller than those in the recent studies using Framingham data. In particular, regarding the contagion of happiness, our estimate's 95 percent confidence interval has an upper bound of 0.04, whereas the analogous estimate by Christakis and Fowler (2008) for friends living near each other is 0.25. Regarding the contagion of depression, our estimate's confidence interval has an upper bound of 0.07, as compared to an analogous estimate of 1.18 by Rosenquist, Fowler, and Christakis (2011).

Although our overall findings suggest that the contagion of mental health is not as large as in previous studies, it is useful to consider the potential implications of our significant results for particular measures and subgroups. From a policy standpoint one of the main motivations for quantifying contagion effects is to gain a sense of the potential positive externalities that would result from interventions that improve mental health. This raises the question of how large our estimates of contagion are, notwithstanding the caveats about multiple testing and statistical significance. If we assume that our results represent reasonable estimates of contagion, then what would they imply about the size of potential externalities?

One way to think about this would be to make an assumption about the relative importance of a roommate within a student's overall social network. For example, suppose that for an average student the roommate is one of five people who are in close and frequent enough contact to be significantly affected by the student's mental health, and the effect on the roommate is roughly the same as the effect on the other four people. Our estimates would then imply that for every one point increase in depression score for a male student, five other people experience a 0.09 increase in depression scores. In this case, we would infer that the individual treatment effect of an intervention to reduce depression among men is supplemented by a 45% (5*0.09) additional externality on social contacts.[26] Although this example involves crude assumptions and the dynamics of contagion would obviously depend on the structure of social networks, this illustrates how seemingly small estimates can still imply large overall externalities. Considering that mental health status predicts a range of other social, economic and health outcomes (Ettner et al., 1997; Kessler et al., 1998), these externalities could have

[26] Further ripple effects to friends-of-friends would add to this externality. Related to this, the effect might expand over time in a social multiplier effect discussed by Carrell et al (2008), Glaeser et al (2003), and others. This would depend on the time dynamics of the mental health production function, which are not well understood in general, let alone in the specific context of social interaction effects.

important implications for overall wellbeing. To understand these externalities fully for the purpose of policymaking, future studies of mental health contagion will need to generate not only on well-identified estimates but also careful characterizations of social networks.[27]

One of our most striking findings is the apparent large contagion effect for men with pre-existing depression. This implies that, in theory, the overall prevalence of depression among college students could be reduced by avoiding the pairing of male roommates with depression. Accounting for mental health in matching of roommates does not seem realistic or desirable, however, given that this type of health information is protected by privacy laws. More importantly, it would be valuable to learn more about why depression appears to be more contagious among men, and whether interventions (e.g., focusing on interpersonal skills and communication) can mitigate the transmission of depression across social contacts. Our results suggest that men's lower tendency to disclose depressed feelings is part of the story, which may be a starting point for interventions. Our findings also imply that, to the extent that depressed men cluster in social networks (as they clearly do in the Framingham data), the positive externalities from prevention and treatment would be especially large. This is particularly important given that less than half of depressed adults in the U.S. receive what is considered minimally adequate treatment, and men are less likely to receive treatment than women (Wang et al., 2005).[28]

Perhaps the most important question about the results of our study is how they generalize to other social contexts. Assigned roommates live in close proximity for about seven months, and they become close friends in about half the cases according to our data. Contagion may be quite different across other social ties, particularly more intimate relationships such as spouses, siblings, and longtime friends.[29] Contagion may also vary considerably by age group, considering how people's social relationships and networks evolve during their lifetime. Therefore, while our findings call into question the universal strength of mental health contagion, they cannot be considered a direct refutation of the much larger estimates in the recent analyses of Framingham Heart Study data.

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[27] The studies of the Framingham data offer good examples of this type of detailed modeling of social networks.
[28] The same is true of college students specifically (Blanco et al., 2008).
[29] Also, it is important to keep in mind that contagion effects may occur on macro-levels of social context, such as neighborhoods and schools, whereas our focus is on a micro-level consisting of two to four peers.

Nevertheless, contagion among people who are placed together largely by chance, as in the case of assigned roommates, may be especially relevant for estimating externalities that generate market failures and thereby motivate policy interventions. These externalities may be less likely to be "internalized" through altruistic behavior by people with mental health problems, as compared to potential externalities across closer social ties. For example, people might seek treatment or take other significant actions to shield their spouses or children from the harmful effects of their depression, whereas they might do less of this on behalf of social contacts such as co-workers and neighbors.

At a minimum, our estimates suggest that the social contagion of mental health resulting from physical proximity, if not always emotional closeness, is modest overall and varies by gender. Obtaining well-identified causal estimates of contagion within other social contexts, such as spousal or sibling relationships, will be more challenging. Perhaps the most promising approach will be to use to experimental designs in which people are randomized to an intervention with established effectiveness, and then outcomes of social contacts (not directly exposed to the intervention) are compared between the intervention and treatment groups, as researchers have done to examine externalities in other contexts (Duflo & Saez, 2003; Miguel & Kremer, 2004).

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**Table 1: Characteristics of primary analytic sample (N=1,641)**

| | *Baseline* | *Follow-up* | |
|---|---|---|---|
| University A (large public) | 0.69 | | |
| University B (large private) | 0.31 | | |
| | | | |
| Double room | 0.79 | | |
| Triple room | 0.17 | | |
| Quad room | 0.04 | | |
| | | | |
| Age | 18.4 (0.41) | 19.0 (0.41) | |
| | | | |
| Female | 0.54 | | |
| | | | |
| White | 0.70 | | |
| Asian | 0.17 | | |
| Black | 0.03 | | |
| Hispanic | 0.05 | | |
| Other | 0.02 | | |
| Multi | 0.04 | | |
| | | | |
| Parents' education | | | |
| Less than college degree | 0.16 | | |
| College degree | 0.27 | | |
| Graduate degree | 0.56 | | |
| | | | |
| Happiness (three items from CES-D) | | | |
| Score (0-9) | 7.67 (1.75) | 7.15 (2.06) | *t*=-10.7 |
| Positive screen (score=9) | 0.49 | 0.40 | z=-5.2 |
| Depression (PHQ-2 screen) | | | |
| Score (0-6) | 0.84 (1.05) | 1.07 (1.22) | *t*=7.3 |
| Positive screen (score>=2) | 0.24 | 0.33 | z=5.7 |
| Anxiety (two items from K-6 screen) | | | |
| Score (0-8) | 2.45 (1.44) | 2.65 (1.56) | *t*=5.1 |
| Positive screen (score>=4) | 0.21 | 0.26 | z=3.2 |
| Psychological distress (K-6 score) (0-24) | 4.13 (3.28) | 5.13 (3.91) | *t*=11.3 |

*Primary sample consists of 1st yr 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. The t- and z-stats are for tests of equal means and proportions between baseline and follow-up.*

**Table 2: Effects of roommate mental health on own mental health**

| | Mean | SD | Roommate effect $\beta$ | SE | p-value |
|---|---|---|---|---|---|
| Happiness score (CES-D items) (0-9) | 7.15 | 2.06 | -0.020 | 0.028 | 0.48 |
| Psychological distress score (K-6) (0-24) | 5.13 | 3.91 | 0.049 | 0.030 | 0.10 |
| Depression score (PHQ-2) (0-6) | 1.07 | 1.22 | 0.012 | 0.031 | 0.71 |
| Anxiety score (K-6 items) (0-8) | 2.65 | 1.56 | 0.053 | 0.027 | 0.05 |

*N=1,641. Each row corresponds to a separate linear regression--for each regression only the estimate for the key coefficient on the roommate variable is shown. All regressions include controls for the variables noted in equation (1): variables used for housing assignments, baseline level of the dependent variable, gender, age, race/ethnicity, parents' education.*

**Table 3: Subgroup analysis by gender and baseline mental health**

| | Men | | | | | | | Women | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | N | Mean | SD | Roommate effect | | | | N | Mean | SD | Roommate effect | | | |
| | | | | $\beta$ | SE | p | | | | | $\beta$ | SE | p | |
| Happiness score (0-9) | | | | | | | | | | | | | | |
| All | 769 | 7.12 | 2.12 | 0.009 | 0.040 | 0.82 | | 866 | 7.18 | 2.00 | -0.053 | 0.040 | 0.18 | |
| Happy at baseline (score=9) | 386 | 7.99 | 1.59 | -0.014 | 0.051 | 0.79 | | 415 | 7.86 | 1.65 | -0.077 | 0.058 | 0.19 | |
| Not happy at baseline (score<9) | 383 | 6.20 | 2.22 | 0.033 | 0.067 | 0.62 | | 451 | 6.58 | 2.10 | -0.105 | 0.066 | 0.11 | |
| Psychological distress score (0-24) | | | | | | | | | | | | | | |
| All | 769 | 4.77 | 3.83 | 0.045 | 0.047 | 0.34 | | 865 | 5.47 | 3.96 | 0.047 | 0.039 | 0.24 | |
| Distressed at baseline (score>=8) | 78 | 8.99 | 4.73 | 0.134 | 0.255 | 0.60 | | 126 | 9.21 | 4.51 | 0.065 | 0.241 | 0.79 | |
| Not distressed at baseline (score<8) | 691 | 4.30 | 3.43 | 0.052 | 0.051 | 0.31 | | 739 | 4.80 | 3.45 | 0.052 | 0.039 | 0.18 | |
| Depression score (0-6) | | | | | | | | | | | | | | |
| All | 771 | 0.95 | 1.17 | ***0.088*** | 0.039 | 0.03 | ** | 872 | 1.17 | 1.26 | -0.059 | 0.046 | 0.20 | |
| Depressed at baseline (score>=2) | 167 | 1.66 | 1.27 | ***0.220*** | 0.127 | 0.08 | * | 227 | 1.75 | 1.35 | ***-0.172*** | 0.110 | 0.12 | |
| Not depressed at baseline (score<2) | 604 | 0.76 | 1.06 | 0.030 | 0.043 | 0.49 | | 645 | 0.96 | 1.15 | -0.022 | 0.054 | 0.68 | |
| Anxiety score (0-8) | | | | | | | | | | | | | | |
| All | 771 | 2.47 | 1.55 | 0.032 | 0.042 | 0.45 | | 867 | 2.81 | 1.54 | ***0.069*** | 0.037 | 0.06 | * |
| Anxious at baseline (score>=4) | 119 | 3.41 | 1.62 | 0.032 | 0.144 | 0.83 | | 223 | 3.70 | 1.60 | 0.109 | 0.100 | 0.28 | |
| Not anxious at baseline (score<4) | 652 | 2.29 | 1.48 | 0.047 | 0.046 | 0.31 | | 644 | 2.49 | 1.39 | 0.052 | 0.043 | 0.23 | |

*Each row corresponds to a separate linear regression--only the estimate for the key coefficient on the roommate variable is shown. All regressions include controls for the variables noted in equation (1): variables used for housing assignments, baseline level of the dependent variable, gender, age, race/ethnicity, parents' education.*

**Table 4: Subgroup analysis of depression contagion by gender and roommates' distress disclosure**

| | Men with disclosing roommate (N=92) | | | | | Women with disclosing roommate (N=149) | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Roommate effect | | | | | Roommate effect | | | |
| | DV mean | SD | $\beta$ | SE | p | DV mean | SD | $\beta$ | SE | p | |
| Depression (PHQ-2) (0-6) | 0.92 | 1.15 | 0.047 | 0.266 | 0.86 | 1.29 | 1.21 | -0.365 | 0.151 | 0.02 | ** |
| | **Men with non-disclosing roommate (N=510)** | | | | | **Women with non-disclosing roommate (N=520)** | | | | | |
| | | | Roommate effect | | | | | Roommate effect | | | |
| | DV mean | SD | $\beta$ | SE | p | DV mean | SD | $\beta$ | SE | p | |
| Depression (PHQ-2) (0-6) | 0.93 | 1.16 | 0.076 | 0.047 | 0.10 | 1.16 | 1.31 | 0.002 | 0.061 | 0.97 | |

*Sample is restricted to students in double rooms (with only one roommate). For each regression only the estimate for the key coefficient on the roommate variable is shown. All regressions include controls for the variables noted in equation (1): variables used for housing assignments, baseline level of the dependent variable, gender, age, race/ethnicity, parents' education.*

## Table 5: Closeness of roommate relationships

| | Primary sample: assigned roommates (N=1641) | | | Requested roommates (N=430) | | |
|---|---|---|---|---|---|---|
| | Men | Women | All | Men | Women | All |
| Close friend with roommate(s) | | | | | | |
| Strongly disagree | 0.13 | 0.14 | 0.13 | 0.01 | 0.04 | 0.02 |
| Disagree | 0.17 | 0.16 | 0.17 | 0.04 | 0.04 | 0.04 |
| Neither agree nor disagree | 0.24 | 0.18 | 0.21 | 0.08 | 0.08 | 0.08 |
| Agree | 0.26 | 0.26 | 0.26 | 0.34 | 0.22 | 0.28 |
| Strongly agree | 0.19 | 0.26 | 0.23 | 0.53 | 0.62 | 0.57 |
| Average time per day with roommate(s) | | | | | | |
| Less than 15 min | 0.32 | 0.29 | 0.30 | 0.04 | 0.07 | 0.05 |
| 15-30 min | 0.13 | 0.11 | 0.12 | 0.06 | 0.07 | 0.06 |
| 30 min - 1 hr | 0.15 | 0.16 | 0.16 | 0.14 | 0.13 | 0.13 |
| 1-2 hrs | 0.18 | 0.17 | 0.17 | 0.24 | 0.12 | 0.18 |
| 2-4 hrs | 0.14 | 0.14 | 0.14 | 0.25 | 0.19 | 0.22 |
| 4 or more hours | 0.08 | 0.12 | 0.10 | 0.28 | 0.42 | 0.35 |
| Enjoys being in room with roommate(s) | | | | | | |
| Strongly disagree | 0.07 | 0.11 | 0.09 | 0.03 | 0.07 | 0.05 |
| Disagree | 0.15 | 0.12 | 0.13 | 0.04 | 0.06 | 0.05 |
| Neither agree nor disagree | 0.26 | 0.21 | 0.23 | 0.16 | 0.12 | 0.14 |
| Agree | 0.33 | 0.30 | 0.32 | 0.36 | 0.27 | 0.32 |
| Strongly agree | 0.19 | 0.26 | 0.22 | 0.42 | 0.48 | 0.45 |
| Discussed own personal problems | | | | | | |
| Never | 0.41 | 0.20 | 0.30 | 0.22 | 0.07 | 0.15 |
| 1-2 times total | 0.28 | 0.22 | 0.25 | 0.28 | 0.13 | 0.21 |
| Once per month or two | 0.13 | 0.16 | 0.15 | 0.17 | 0.09 | 0.13 |
| Once every week or two | 0.1 | 0.16 | 0.13 | 0.14 | 0.12 | 0.13 |
| Couple times per week | 0.06 | 0.13 | 0.10 | 0.11 | 0.23 | 0.17 |
| Almost every day | 0.02 | 0.13 | 0.08 | 0.08 | 0.36 | 0.21 |
| Discussed roommate(s)'s personal problems | | | | | | |
| Never | 0.44 | 0.19 | 0.30 | 0.23 | 0.04 | 0.14 |
| 1-2 times total | 0.27 | 0.24 | 0.26 | 0.23 | 0.12 | 0.18 |
| Once per month or two | 0.12 | 0.15 | 0.13 | 0.18 | 0.10 | 0.14 |
| Once every week or two | 0.09 | 0.15 | 0.12 | 0.18 | 0.11 | 0.15 |
| Couple times per week | 0.06 | 0.14 | 0.10 | 0.10 | 0.25 | 0.17 |
| Almost every day | 0.02 | 0.14 | 0.08 | 0.08 | 0.37 | 0.22 |

**Table 6: Effects of roommates' mental health on closeness with roommates**

| Dependent variable | Key righthand-side variables | Roommate (RM) effects β | SE | p-value |
|---|---|---|---|---|
| Close friend with roommate(s) | RM happiness (0-9) | 0.009 | 0.026 | 0.72 |
| (1-5 scale, mean = 3.28, SD = 1.35) | \|RM happiness - own happiness)\| | -0.089 | 0.03 | <.01 |
| | RM depression (0-6) | -0.022 | 0.04 | 0.58 |
| | \|RM depression - own depression\| | -0.111 | 0.047 | 0.02 |
| | RM anxiety (0-8) | -0.012 | 0.027 | 0.65 |
| | \|RM anxiety - own anxiety\| | 0.029 | 0.034 | 0.40 |
| Average time per day spent with RM(s) | RM happiness (0-9) | 0.022 | 0.033 | 0.50 |
| (1-6 scale, mean = 3.03, SD = 1.73) | \|RM happiness - own happiness)\| | -0.076 | 0.04 | 0.05 |
| | RM depression (0-6) | -0.003 | 0.053 | 0.95 |
| | \|RM depression - own depression\| | -0.123 | 0.06 | 0.04 |
| | RM anxiety (0-8) | 0.025 | 0.036 | 0.48 |
| | \|RM anxiety - own anxiety\| | 0.017 | 0.047 | 0.72 |
| Enjoys being around RM(s) | RM happiness (0-9) | 0.028 | 0.024 | 0.25 |
| (1-5 scale, mean = 3.44, SD = 1.23) | \|RM happiness - own happiness)\| | 0.074 | 0.027 | 0.01 |
| | RM depression (0-6) | -0.037 | 0.039 | 0.35 |
| | \|RM depression - own depression\| | -0.087 | 0.045 | 0.05 |
| | RM anxiety (0-8) | -0.016 | 0.027 | 0.54 |
| | \|RM anxiety - own anxiety\| | 0.002 | 0.032 | 0.96 |
| Discussed own problems w/ RM(s) | RM happiness (0-9) | 0.007 | 0.028 | 0.82 |
| (1-6 scale, mean = 2.71, SD = 1.60) | \|RM happiness - own happiness)\| | -0.051 | 0.032 | 0.11 |
| | RM depression (0-6) | 0.016 | 0.045 | 0.72 |
| | \|RM depression - own depression\| | -0.052 | 0.053 | 0.33 |
| | RM anxiety (0-8) | 0.029 | 0.032 | 0.36 |
| | \|RM anxiety - own anxiety\| | -0.01 | 0.04 | 0.80 |
| Discussed RM(s)'s problems w/ RM(s) | RM happiness (0-9) | -0.032 | 0.029 | 0.28 |
| (1-6 scale, mean = 2.71, SD = 1.63) | \|RM happiness - own happiness)\| | -0.068 | 0.033 | 0.04 |

| | | | |
|---|---|---|---|
| RM depression (0-6) | 0.05 | 0.046 | 0.27 |
| \|RM depression - own depression\| | -0.069 | 0.056 | 0.22 |
| | | | |
| RM anxiety (0-8) | 0.058 | 0.031 | 0.07 |
| \|RM anxiety - own anxiety\| | 0.008 | 0.040 | 0.85 |

*Sample restricted to students in double rooms (students w/ one roommate). Each pair of rows corresponds to a separate linear regression and only the key coefficients are shown. All regressions include controls for the variables noted in equation (1): variables used for housing assignments, baseline level of the dependent variable, gender, age, race/ethnicity, parents' education.*

**Table 7: Effects of hallmate mental health on own mental health**

| | Hallmate effect | | | Roommate effect | | |
|---|---|---|---|---|---|---|
| | $\beta$ | SE | p | $\beta$ | SE | p |
| Happiness score (CES-D items) (0-9) | | | | | | |
| Not controlling for RM effect | 0.023 | 0.087 | 0.79 | | | |
| Controlling for RM effect | 0.023 | 0.110 | 0.84 | -0.026 | 0.031 | 0.40 |
| Psychological distress score (K-6) (0-24) | | | | | | |
| Not controlling for RM effect | 0.194 | 0.090 | 0.03 | | | |
| Controlling for RM effect | 0.140 | 0.111 | 0.22 | 0.03 | 0.03 | 0.32 |
| Depression score (PHQ-2) (0-6) | | | | | | |
| Not controlling for RM effect | 0.129 | 0.099 | 0.19 | | | |
| Controlling for RM effect | 0.169 | 0.141 | 0.23 | -0.004 | 0.032 | 0.90 |
| Anxiety score (K-6 items) (0-8) | | | | | | |
| Not controlling for RM effect | 0.162 | 0.085 | 0.06 | | | |
| Controlling for RM effect | 0.065 | 0.109 | 0.55 | 0.044 | 0.028 | 0.11 |

*N=2,217 for regressions that do not control for RM effect, and 1,641 for regressions that do. Each row corresponds to a separate linear regression--only the estimate for the key coefficients on the hallmate and roommate variables are shown. All regressions include controls for the variables noted in equation (1): variables used for housing assignments, baseline level of the dependent variable, gender, age, race/ethnicity, parents' education.*

**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 | 1658 |
| 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 |
| Depression (PHQ-2 screen) | | | | |
| Score (0-6) | | 0.84 | 0.85 | 0.84 |
| Positive screen (score>=2) | | 0.24 | 0.24 | 0.24 |
| Anxiety (two items from K-6 screen) | | | | |
| Score (0-8) | | 2.48 | 2.47 | 2.45 |
| Positive screen (score>=4) | | 0.22 | 0.22 | 0.21 |
| Psychological distress (K-6 score) (0-24) | | 4.18 | 4.19 | 4.13 |
| Happiness (three items from CES-D) | | | | |
| Score (0-9) | | 7.66 | 7.64 | 7.67 |
| Positive screen (score=9) | | 0.48 | 0.48 | 0.49 |

*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)**

| | Roommate coefficient, w/o correction in Guryan et al (2009) | | | Roommate coefficient w/ correction in Guryan et al (2009) | | |
|---|---|---|---|---|---|---|
| | $\beta$ | SE | p | $\beta$ | SE | p |
| Happiness (CES-D items) (0-9) | -0.001 | 0.034 | 0.98 | -0.007 | 0.032 | 0.83 |
| Psychological distress (K-6) (0-24) | -0.003 | 0.034 | 0.93 | 0.003 | 0.031 | 0.91 |
| Depression (PHQ-2) (0-6) | -0.022 | 0.035 | 0.54 | -0.017 | 0.033 | 0.60 |
| Anxiety (two items from K-6) (0-8) | -0.014 | 0.034 | 0.69 | 0.008 | 0.031 | 0.79 |
| Eating disorder risk (SCOFF) (0-5) | -0.047 | 0.032 | 0.15 | -0.038 | 0.031 | 0.22 |
| Suicidal ideation (past 6 mos) (0/1) | -0.011 | 0.026 | 0.68 | 0.003 | 0.022 | 0.90 |
| Non-suicidal self-injury (past 6 mos) (0/1) | -0.006 | 0.032 | 0.84 | 0.005 | 0.029 | 0.86 |
| Parents' education (highest attainment) (1-7) | 0.050 | 0.035 | 0.16 | 0.036 | 0.032 | 0.27 |
| Religiosity (0-3) | 0.006 | 0.033 | 0.85 | 0.016 | 0.031 | 0.61 |
| Binge drinking (frequency in past 30 days) (0-5) | 0.003 | 0.031 | 0.93 | -0.015 | 0.030 | 0.61 |
| Exercise (frequency in past 30 days) (0-3) | -0.003 | 0.035 | 0.92 | -0.004 | 0.032 | 0.89 |
| Studying (time per day in high school) (0-5) | -0.048 | 0.034 | 0.16 | -0.047 | 0.031 | 0.13 |
| Admissions test (standardized z-score, SAT/ACT) | 0.039 | 0.032 | 0.22 | 0.021 | 0.029 | 0.46 |
| GPA in high school (standardized z-score) | 0.034 | 0.031 | 0.28 | 0.05 | 0.03 | 0.09 |

*N=1,053 (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 roommate variable is shown. All regressions include controls for the variables used for housing assignments, as described in the text.*