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Social contagion of mental health: Evidence from college roommates

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Abstract

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 no significant overall contagion of mental health and no more than small contagion effects for specific mental health measures, with no evidence for happiness contagion and modest evidence for anxiety and depression contagion. 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. We also find that similarity of baseline mental health predicts the closeness of roommate relationships, which highlights the potential for selection biases in studies of peer effects that do not have a clearly exogenous source of variation. 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.

Keywords

peer effects; social interactions; mental health; natural experiment

1. 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, like spillover effects more broadly, it implies potential market failures

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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 related to the prevention, treatment, or transmission of the illness.

This paper examines the contagion of mental health. In a sense, this is one of the most meaningful forms that a spillover effect could take, because mental health is a fundamental indicator of wellbeing. Among children and young adults in developed countries such as the United States, mental disorders account for nearly half of the estimated burden of disease, measured as lost disability-adjusted life years (DALYs) (Michaud *et al*., 2006). Aspects of mental health are important in the development of human capital among children (Heckman *et al*., 2006, Currie and Stabile, 2007), and mental disorders are important negative predictors of economic and social outcomes in adulthood such as employment and earnings (Ettner *et al*., 1997) and marital stability (Kessler *et al*., 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 *et al*., 2011) and happiness (Fowler and 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 interaction effects 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 apply a well-established econometric identification strategy to produce 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.

Our findings are consistent with no significant overall contagion of mental health and no more than small contagion effects for specific mental health measures. Happiness does not

exhibit significant contagion, whereas poor mental health—measured as general psychological distress, depression, and anxiety—exhibits some evidence of contagion, although the depression contagion is only significant for men. These results, particularly for subgroups, should be viewed as exploratory rather than confirmatory, given the potential for type I errors in the presence of multiple, related hypothesis tests. 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. Also, similarity of baseline mental health predicts the closeness of roommate relationships, which highlights the potential for selection biases in studies of peer effects that do not have a clearly exogenous source of variation. 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.

2. 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.

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 *et al*., 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 and

[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.

Katz, 1999). In addition, the other person may not be enjoyable to be around, which may in turn decrease one's mental health (Hokanson *et al*., 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 and 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 and 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, the direction and magnitude of these contagion effects are open empirical questions.

As we explore in our empirical analysis, contagion effects may be heterogeneous across types of individuals. 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 and 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 and Watson, 2001).

Women are generally more likely than men to discuss their emotions (Kahn and Garrison, 2009) and less likely to suppress emotional expression (Gross and John, 2003). Thus, 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. Some psychological studies suggest that women exhibit higher emotional contagion in terms of immediate reactions (Doherty, 1997), but these studies do not address contagion over a longer time period with sustained interactions as in the present study.

Contagion may also depend on one's own mental health. People with poor mental health may be more susceptible to contagion, if they have less ability to cope with the stress of being around someone else with poor mental health. 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 *et al*., 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 of biases due to self-selection, common shocks, and the reflection problem, as described by Manski (1993). 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. Collectively these studies generally find modest evidence for academic peer effects, although some find heterogeneous effects by initial academic ability (Sacerdote, 2001, Zimmerman, 2003) and by gender (Stinebrickner and Stinebrickner, 2006). Although academic peer effects are likely to operate very differently than mental health contagion, previous results from this

[3]Although many 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), this is very different from demonstrating that more enduring states of mental health, such as depression, are contagious.

[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.

literature have potential implications for our study. In particular, social behavior helps explain academic peer effects among roommates, which is notable for our context because social behavior is often correlated with mental health. For instance, 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 (Kremer and Levy, 2003), and assignment to a roommate who brings video games to college causes less studying and lower grades (Stinebrickner and Stinebrickner, 2008).

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 the college admissions process deliberately restricts the distribution of academic ability at each institution. Two previous studies in the psychology literature 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.

3. 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 school with approximately 6,000 first-year students (hereafter "university A"), and one private school with approximately 4,000 first-year students ("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. The study was approved by Institutional Review Boards at both universities.

First-year students are required to live in campus housing at the universities except in unusual circumstances. They 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.

Our main empirical approach builds on the framework of previous studies of peer effects among college roommates, by estimating linear regressions of the form:[5]

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

The subscript *t* denotes a measurement in the baseline survey, and *t+1* denotes a measurement in the follow-up survey. *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 β_2, which represents the effect of roommate mental health on the individual's mental health. Heteroskedasticity-robust standard errors are corrected for clustering 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,[6] 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 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.[7],[8] 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

[5]In sensitivity checks we also estimate probit regressions with binary mental health variables (e.g., positive screen for depression, as defined by the standard PHQ-9 algorithm). Note also that we chose this specification (with the outcome measure at follow-up as the dependent variable and the outcome measure at baseline as a covariate), rather than one with change scores as the dependent variable, in order to remain consistent with most previous roommate studies. Applied statisticians have debated these two alternative approaches for many years, and a common (though not universal) view seems to be that controlling for the baseline dependent variable is preferable in randomized trials, which is essentially what we have. In any case, our main estimates of contagion effects remain essentially the same when we look at change scores (results available on request). In addition, given that the dependent variables have somewhat skewed distributions, we estimated generalized linear model (GLM) regressions (e.g., log link function and gamma family) but did not find a notable change in point estimates or precision (results also available on request).

[6]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 *et al.*, 2010). Also, the "pre-incentive" has the advantage of not requiring an additional letter or email for delivery of the incentive.

[7]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.

[8]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.

responders with at least one roommate baseline responder, 1,641 (63%) completed the follow-up survey.[9]

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. As shown in Appendix 1, we find minimal evidence of differential attrition. Despite our reasonably large sample size, the only statistically significant difference in attrition is a slightly higher proportion of women in the final analytic sample (0.53) as compared to the initial sample (0.50). Also, conditional on gender, whether a student responds at follow-up is not significantly associated with own or roommate mental health at baseline (results available on request).

Additional characteristics of the primary analytic sample are shown in Table I. 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. The full wording of these items is shown in Appendix 2. Depressive symptoms are measured by the PHQ-2 instrument (Löwe *et al*., 2005) 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. 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 items are summed for a happiness score of 0-9.

As shown in Table I, 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.

[9]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.

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.[10]

Exogeneity of roommate assignments

For students who do not request roommates, the assignment processes differ somewhat between the two universities in our sample (full details of the assignment processes are available in Appendix 3). 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. This key 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.

If housing assignments are exogenous conditional on variables used by the housing offices to assign roommates, 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).

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

We find, as expected, that the estimates of β_2 are close to zero for all outcome variables, and none of the estimates are significant at $p<0.05$ (results in Appendix 3).[11]

4. RESULTS

Contagion in the overall sample

Table II shows the results of the estimation of equation (1) for the overall sample of assigned roommates; each row shows the key coefficient, β_2, from a separate regression. We

[10] 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 II) 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?).

[11] 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.

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: 0.51 for happiness; 0.57 for psychological distress; 0.42 for depression; and 0.46 for anxiety (all significant at $p<0.01$). As described in the notes below Table II, we would fail to reject a composite null hypothesis of no mental health contagion, when accounting for multiple hypothesis tests.

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 III, 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, because students with poor mental health at baseline may have less ability to cope effectively with being around another person in poor mental health. After further stratifying the sample at a binary cutoff for each baseline mental health measure we find evidence consistent with this for depression contagion among men.[12] 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.[13] It is important to keep in mind, however, that all of these subgroup results should be viewed as tentative, given the lack of unambiguous prior hypotheses and the possibility for type I errors with a large number of hypothesis tests.

[12]We use a cutoff established as an indicator of a probable depressive disorder in validation studies of the PHQ-2 screen for depression (Löwe *et al*., 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).

[13]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.

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. Higher disclosure could augment contagion by making the roommate's depression more salient and perhaps more burdensome, and may lead to co-rumination, but on the other hand, disclosure could reduce contagion by reducing misunderstandings about the depressed mood and associated behavior.[14] For this analysis we limit the sample to students in double rooms—this allows for a cleaner interpretation of the roommate's disclosure measure. [15] Our measure is a single item from the Distress Disclosure Index (Kahn and 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 IV, 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.

Closeness of roommate relationships and mental health

To further enhance the interpretation of our contagion estimates, we examine a number of measures of the closeness of roommates' relationships. We use these measures to learn more about the nature of roommate relationships, 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. The results of these analyses are fully described in Appendix 4, and they can be summarized in three key findings.

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, and contagion effects do not appear to vary by closeness of friendships or time spent together. Second, roommates with similar mental health at baseline are somewhat more likely to become close, which underscores possible selection biases in studies of contagion based on endogenously formed social networks. Third, closeness of roommates appears to be

[14]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.

[15]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.

intertwined with the differences by gender in peer effects on depression: the "harmful" contagion among men is concentrated among those who are not close friends, and the "helpful" contagion among women is concentrated among those who are close friends. These latter findings should be regarded as tentative, however, given the large number of subgroups and imprecision of estimates, and warrant further exploration.

Alternative specifications and sensitivity checks

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 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 (results available on request). While it is still possible that other unmeasured characteristics may affect our estimates in either direction,[16] the robustness of our estimates to a broad set of controls suggests that we are largely picking up true contagion effects.

We also estimate several other specification and robustness checks. First, we do not find evidence of a nonlinear effect of roommate's mental health, and our main patterns of results hold when we specify the mental health variables as binary (e.g., positive screen for depression) and estimate probit regressions.[17] Second, our results are nearly identical when we estimate models separately by university, which is at least suggestive that the findings may generalize to other settings. In both schools 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). 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 (available on request) 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.

[16]For example, personality characteristics such as extroversion and neuroticism are known to be associated with mental health (Kendler *et al*., 2006), and would be useful to examine more closely in future studies.

[17]In these specifications the key righthandside variable is still the roommates' mental health score as in the main specifications, but the dependent variable is a binary measure corresponding to the cutoffs described in footnote 12. The pattern of results (available on request) is similar to the main results in Table II, except that the statistical significance is somewhat stronger for the contagion of distress (p=0.02) and anxiety (p=0.006).

5. DISCUSSION

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. We find limited and mixed evidence regarding the presence and strength of contagion. We fail to reject the null hypothesis of no mental health contagion when accounting for multiple hypothesis tests. In terms of specific measures, we obtain relatively precise null results for the contagion of happiness, and 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.

As noted previously, it is important to consider that we evaluated a number of hypothesis tests pertaining to related issues. Adjustments for multiple hypothesis testing are important to consider, 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. 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. 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.

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 important to keep in mind that even small effects can add up to a large overall effect. Suppose, for example, the roommate is one of five people who are in close and frequent enough contact to be significantly affected by a 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 project 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.[18] This example involves crude assumptions and the dynamics of contagion would obviously depend on the structure of social networks; to understand these externalities fully, future studies of mental health contagion will need to generate not only well-identified estimates but also careful characterizations of social networks.[19]

One of our most striking (though, again, tentative) findings is the apparent large contagion effect for men with pre-existing depression. This implies that the overall prevalence of depression among college students could be reduced by avoiding the pairing of male roommates with depression, though this seems neither realistic nor desirable, 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 (and similarly, how interventions might augment the beneficial peer effects that appear to occur among women). Our findings also imply that, to the extent that depressed men cluster in social networks, 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).[20]

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.[21] 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.

Nevertheless, contagion among people who are placed together largely by chance, as in the case of assigned roommates, may be especially relevant for estimating spillover effects that could generate market failures and thereby motivate policy interventions. These spillovers 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.

[18]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.

[19]The studies of the Framingham data offer good examples of this type of detailed modeling of social networks.

[20]The same is true of college students specifically (Blanco *et al*., 2008).

[21]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.

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 (Miguel and Kremer, 2004, Duflo and Saez, 2003).

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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Table I
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 II

Effects of roommate mental health on own mental health

			Roommate effect		
	Mean	SD	β	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.

[#]If the estimated effects in this table are thought of as independent, multiple tests of the related hypothesis that there is any mental health contagion, then the p-value should be adjusted upward. There are effectively three tests that are arguably independent: contagion for happiness, anxiety, and depression (whereas distress is a combination of anxiety and depression, and should be omitted from the analysis of multiple testing). Because of the small number of tests and the fact that only one test is anywhere close to significant, the adjusted p-value is the same whether one uses a Bonferonni, Holm-Bonferroni, or false discovery rate (FDR) adjustment—it is simply three times the standard p-value. So a (conservative) adjusted p-value for the anxiety contagion estimate is 0.15.

Table III

Subgroup analysis by gender and baseline mental health

	Men						Women					
	N	Mean	SD	Roommate effect			N	Mean	SD	Roommate effect		
				β	SE	p				*β*	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, ag e, race/ethnicity, parents' education.

Table IV

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	*β*	SE	p	DV mean	SD	*β*	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	*β*	SE	p	DV mean	SD	*β*	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.