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Illness Labels and Social Distance

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Abstract

The authors examine a key proposition in the modified labeling theory—that a psychiatric label increases vulnerability to negative evaluation and social rejection—using an experimental design wherein female participants interact with a female teammate over a computer. The authors also evaluate a hypothesis derived from the disease-avoidance account of disgust by examining this same process for a nonpsychiatric illness: food poisoning. In addition, they introduce a composite measure of social distance behavior that is easy to implement in a laboratory experiment. The authors find, as predicted, that women seek greater social distance from teammates with a history of psychiatric or food poisoning hospitalization than they do from teammates with no hospitalization history. But, contrary to predictions, a teammate's hospitalization history does not affect participants' ratings of her likability. The results also do not vary significantly by psychiatric diagnosis (depression vs. schizophrenia), suggesting that the stigma of depression may be just as strong as the stigma of schizophrenia when information about symptoms is not available. The authors discuss the implications of these findings for the modified labeling theory of mental illness and for the literature on disgust and stigma. They also outline avenues for future research.

Keywords

stigma, labeling, discrimination

Although psychiatric treatment programs can dramatically reduce patients' symptoms (Link et al. 1997; Rosenfield 1997), the official labeling that accompanies this treatment can negatively affect patients. Numerous studies over the past three decades suggest that when individuals are officially recognized to have a mental illness, they are placed into a cultural category (e.g., "a mentally ill person") that damages their material, social, and psychological well-being (Kroska and Harkness 2006, 2008; Link 1982, 1987; Markowitz 1998; Markowitz, Angell, and Greenberg 2011; Rosenfield 1997; Wright, Gronfein, and Owens 2000).

According to the modified labeling theory of mental illness (Link 1987; Link, Mirotznik, and Cullen 1991; Link et al. 1989, 1997), these negative consequences develop through three interrelated processes. First, when an individual is diagnosed with a psychiatric disorder, negative societal conceptions (e.g., incompetent, dangerous) associated with the new label become personally relevant and foster feelings of demoralization. Second, a psychiatric diagnosis that is publically known increases patients' vulnerability to negative evaluation and social rejection. Finally, patients whose self-concepts have been damaged through the first two

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processes increase their use of defensive, or coping, behaviors aimed at warding off subsequent rejection: concealing treatment history, withdrawing from social interactions, and educating others about mental illness. But rather than helping patients, these defensive behaviors are hypothesized to further harm them by, for example, constricting support networks and reducing employment opportunities. Thus, according to the modified labeling theory, diagnostic labels damage patients by producing a negative self-concept, increasing others' negative evaluations and rejection, and triggering defensive behaviors that impair mental health recovery.

Although the first and third processes have been investigated in several recent studies (Kroska and Harkness 2006, 2008, 2011; Markowitz 1998; Markowitz et al. 2011; Rosenfield 1997; Wright et al. 2000), the second process—the increase in negative evaluation and social rejection after official labeling—has received surprisingly little attention in recent years, particularly with studies that include behavioral measures of social rejection. In fact, we identified only three studies published in the past 20 years that examined this process with behavioral outcomes (Lucas and Phelan 2012; Mehta and Farina 1997; Page 1995), a dearth that is surprising given the debate and disputation regarding this proposition (e.g., see critiques in Crocetti, Spiro, and Siassi 1974; Gove 1980, 1982, 2004; Huffine and Clausen 1979). Furthermore, we found no behavioral studies (from any era) that examine how specific diagnoses (e.g., depression, schizophrenia) differentially affect the rejection process. Our study begins to address these gaps. First, we offer a contemporary examination of a key phase in the labeling process: the causal link between psychiatric labels and social rejection. Second, we introduce a composite measure of social rejection that is easy to implement. Third, we explore the differential effect of specific psychiatric diagnoses (depression and schizophrenia) on negative evaluations and social rejection. Finally, we examine these processes for a nonpsychiatric illness, food poisoning, to explore the hypothesis that some forms of stigma are rooted in the human disgust response (Kurzban and Leary 2001; Oaten, Stevenson, and Case 2009).

Methodological Issues

Most of the recent studies aimed at identifying the effect of psychiatric labels on social rejection have used surveys or experimental vignettes. Although both techniques are important, they do nonetheless have limitations. Survey studies typically compare psychiatric patients with nonpatients who have similar psychiatric symptoms on self-reported rates of rejection and related outcomes, such as income, employment status, and support network size (Link 1982; Link et al. 1989, 1991). Although these studies suggest the social consequences of psychiatric labels, their causal conclusions are rarely definitive, because the studies cannot ensure nonspuriousness and often cannot establish temporal ordering. Establishing nonspuriousness is important, because psychiatric labels are correlated with a host of other attributes (e.g., psychiatric symptoms, low socioeconomic status) that can have the same deleterious effects on social interaction often attributed to psychiatric labels. Establishing time order is important, because mental illness, and hence psychiatric labels, may be reciprocally related to social rejection.

Vignette experiments also have limitations. Recent vignette experiments do not include conditions wherein the vignette character is symptom free but carries a diagnosis (Angermeyer and Matschinger 1996; Corrigan et al. 2003; Martin, Pescosolido, and Tuch 2000; Pescosolido et al. 2010; Phelan 2005; Schnittker 2000; Socall and Holtgraves 1992),[1] a design that makes it impossible to identify a pure labeling effect. Yet even vignette studies that include this condition (e.g., Kirk 1974; Link and Cullen 1983; Link et al. 1987) have limitations, because, like all vignette studies, they rely on respondents' reports of how they expect that they would behave when interacting with psychiatric patients, reports that are often discrepant from actual behavior (Crocker, Major, and Steele 1998). Thus, the hypothetical nature of vignette studies, coupled with their overt measures of rejection, presents limitations. As many analysts emphasize (Crocker et al. 1998; Hebl and Dovidio 2005; Link et al. 2004; Stier and Hinshaw 2007), studies of stigma processes should ideally use unobtrusive measures of rejection that minimize social desirability bias and that measure rejection in the domain in which it occurs: social interaction.

When combining measurement constraints with the challenges to establishing causality, it becomes clear that effectively examining the relationship between psychiatric labels and social rejection is difficult: researchers need to establish the temporal ordering, assess the effect of labels

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independent of factors correlated with the labels, measure social rejection in the context of social interaction, and measure social rejection in a way that minimizes social desirability bias. We attempt to address each issue with our research design: (1) we establish nonspuriousness and time order with an experimental design that varies only the participant's interactant's hospitalization history across conditions and measures social rejection afterward, (2) we measure social rejection after a computer-mediated social interaction, and (3) we reduce social desirability bias by measuring social rejection with a composite measure that provides participants with legitimated excuses for rejecting the interactant.

Social Consequences of a Psychiatric Label

According to the modified labeling theory, a psychiatric diagnosis functions as a stigmatizing marker that increases an individual's vulnerability to negative evaluation and social rejection (Link et al. 1997). Importantly, the label alone (separated from the symptoms) is hypothesized to contribute to the negative evaluation and social rejection. Yet several researchers have largely rejected this hypothesis (e.g., Crocetti et al. 1974; Gove 1980, 1982, 2004; Huffine and Clausen 1979), concluding that the negative effects of psychiatric labels are short lived and/or inconsequential. Gove (2004), for example, argued that "in the absence of a continuation of disturbed behavior, [the] stigma [of psychiatric hospitalization] is almost always transitory" (p. 365). He also interpreted the literature as showing that "when persons are actually interacting with someone who manifests a pattern of normal behavior, they tend not to reject that person just because the person has had a mental hospitalization" (p. 370).

Despite the debate, however, the proposition that psychiatric labels increase negative evaluation and social rejection has considerable support. Experimental studies suggest that individuals identified as psychiatric patients are evaluated less positively, rejected more readily, and treated less cordially and more aggressively than nonpatients, particularly by men (Farina and Felner 1973; Farina, Felner, and Boudreau 1973 [study 2]; Farina, Holland, and Ring 1966; Farina and Ring 1965; Farina, Thaw, et al. 1976; Gillmore and Farina 1989; Loman and Larkin 1976; Lucas and Phelan 2012; Mehta and Farina 1997; Page 1977, 1983, 1995; Sibicky and Dovidio 1986), and some vignette experiments designed to assess a pure labeling effect suggest similar conclusions (e.g., Link and Cullen 1983; Phillips 1964). But the empirical support is not unequivocal: some experiments fail to support the proposition, particularly among female participants (Farina et al. 1973 [studies 1 and 3]; Farina and Hagelauer 1975; Farina, Murray, and Groh 1978; Farina, Thaw, et al. 1976; Lehmann et al. 1976), while others show somewhat mixed results (e.g., Farina, Hagelauer, and Holzberg 1976; Piner and Kahle 1984). Likewise, some vignette experiments fail to support this proposition (e.g., Kirk 1974; Link et al. 1987).

We investigate this question using an experimental format. We measure participants' evaluations of their interactants with a composite measure of perceived likability, and we operationalize their social rejection with a composite measure of social distance behaviors. Despite the somewhat mixed results in previous studies, we draw on the more common pattern in the literature and expect a psychiatric label to reduce likability ratings and increase social rejection.

Hypothesis 1: Participants will seek more social distance from psychiatric patients than from nonpatients.

Hypothesis 2: Participants will rate psychiatric patients as less likable than nonpatients.

Variation by Diagnosis

Vignette studies generally show that individuals are more reluctant to interact with characters displaying symptoms of schizophrenia than with those displaying symptoms of depression (e.g., Angermeyer and Matschinger 1996; Martin et al. 2000; Pescosolido et al. 2010; Schnittker 2000; Socall and Holtgraves 1992; but see Phelan 2005). In addition, survey research shows that individuals who describe mental illness in ways that include psychosis (a symptom of schizophrenia but not of depression) are more likely than others to also mention violence in their descriptions of mental illness (Phelan et al. 2000). Together these patterns suggest that rejection experiences may be more frequent and/or more extreme for symptomatic schizophrenic patients than for symptomatic depressed patients.

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Yet it is not clear that this pattern holds for asymptomatic patients. Only two studies in recent decades (Francis and Heise 2006; Penn et al. 1994) have examined the way that specific diagnoses (separated from symptoms) affect social rejection and/or evaluation, and both suggest that the effects of schizophrenia and depression labels are similar. Penn et al. (1994) found no differences in the self-reported desire for social distance, perceptions of dangerousness, and affective reactions to asymptomatic depressed and asymptomatic schizophrenic vignette characters. And although they identified a difference by diagnosis in the perceived skills of these characters, the difference favored schizophrenic rather than depressed patients. Francis and Heise's (2006) semantic differential data suggest a similar pattern. Their 2003 U.S. *Interact* dictionary provides evaluation, potency, and activity ratings of hundreds of identities rated by college students in Indiana. The evaluation ratings, which range from infinitely bad (–4.3) to infinitely good (4.3),[2] may be the most relevant to social rejection. Women's evaluation of "a schizophrenic" is slightly bad (–1.21), which is highly similar to (and even a bit less negative than) their evaluation of the only nonpsychotic psychiatric identity in the dictionary, "a neurotic" (–1.27). It is also similar to their evaluation of the amalgamated identities of "a depressed adult" (–1.15), "a depressed undergraduate" (–1.11), and "a depressed woman" (–1.03). Together, Penn et al.'s vignette study and Francis and Heise's semantic differential research suggest that evaluations and rates of social rejection may be similar for depressed and schizophrenic patients whose symptoms are not visible. Given these limited and mixed trends, we examine these processes—the effect of diagnosis (depression vs. schizophrenia) on likability ratings and rate of rejection (seeking social distance)—without advancing a priori hypotheses regarding differences by diagnosis.

Social Consequences of an Illness Label

A growing body of literature suggests that xenophobia and some types of stigma are rooted, at least in part, in disgust (Faulkner et al. 2004; Fincher and Thornhill 2012; Kurzban and Leary 2001; Oaten et al. 2009; Park, Faulkner, and Schaller 2003). According to the disease-avoidance account of disgust (Oaten et al. 2009), which undergirds this perspective, stimuli that are even remotely associated with disease transmission are likely to elicit avoidance behaviors, particularly when the stimuli are found on strangers. The social avoidance appears to be rooted in a disgust response to pathogen risks: numerous studies show that stimuli perceived to pose a pathogen risk, including individuals who are ill (Crandall and Moriarty 1995; Curtis, Aunger, and Rabie 2004) and those who violate hygiene norms (Curtis and Biran 2001; Soo and Stevenson 2007), evoke a disgust response and avoidance behavior (also see Curtis 2007; Rozin, Haidt, and McCauley 2008). Individuals from countries thought to have unusual food preparation and hygiene practices also evoke avoidance responses (i.e., opposition to their immigration) when the fear of disease has been elevated through a video (Faulkner et al. 2004).

According to this perspective, the human reaction to disgust is designed to be overly sensitive (i.e., prone to false alarms) to avoid fatal misses, and it is automatic and fairly impenetrable to cognition (Oaten et al. 2009). Consequently, disgust reactions are often evoked in response to signals of disease that are highly remote and cannot be rationally understood as a disease threat. These features explain individuals' refusal to eat appetizing foods (e.g., fudge chocolate) when they are shaped in the form of disgusting objects (e.g., dog feces) despite knowledge that the food is not that object (Rozin, Millman, and Nemeroff 1986). These features also explain disgust reactions evoked in response to stimuli not directly linked to disease, including people who are physically disabled (Park et al. 2003) or obese (Harvey et al. 2002). Some analysts have also proposed that disgust underlies the social avoidance of the mentally ill (Oaten et al. 2009:312). In line with that proposal, 42 percent of the undergraduates Wheeler, Farina, and Stern (1983) surveyed felt that most people would be unwilling to eat food prepared by a psychiatric patient, 31 percent felt that most people would wash their hands after touching someone who was mentally ill, 26 percent felt that most people would not swim in a pool used by psychiatric patients, and 26 percent felt that most people would be bothered by drinking from a water fountain in a psychiatric hospital.

In a preliminary effort to explore these ideas, we examine evaluative and social distance responses to individuals who previously suffered from a potentially disgust-inducing but nonpsychiatric illness: food poisoning. The disease-avoidance account of disgust suggests that the

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participants paired with a partner with a history of hospitalization for food poisoning may fear, perhaps subconsciously, that contact with the partner poses a pathogen risk due to either a lingering illness or the partner's lax hygiene or food-cleaning practices. Thus, we expect a history of food poisoning hospitalization to reduce likability ratings and increase social rejection.

Hypothesis 3: Participants will seek more social distance from food-poisoned patients than from nonpatients.

Hypothesis 4: Participants will rate food-poisoned patients as less likable than nonpatients.

Our Study

Our work builds on the existing experimental work in this area (i.e., Lucas and Phelan 2012; Sibicky and Dovidio 1986). In Lucas and Phelan's (2012) study, participants in the psychiatric patient condition learn that their partner was hospitalized in the past 12 months for "psychological problems." Those in the control condition learn that their partner reported no hospitalization in the past 12 months. Participants do not learn their partner's gender. After working with their partner on 25 rounds of a task, they are asked to select a topic that will be discussed with a partner in a follow-up study. Participants can see which of the two follow-up topics their current partner selected, but they think the researcher does not know that they can see this, a design that allows participants to seek social distance without worrying about the researcher's judgment of this behavior. As predicted, participants in the psychiatric partner condition were more likely than those in the control condition to seek a different partner for the follow-up study.

We examine social distance behavior using a similar method. As with Lucas and Phelan (2012), we examine social distance behavior after participants have worked on a task over the computer with another person. But we go beyond their design in four ways: (1) we use a five-item composite measure of social distance rather than a single-item measure; (2) participants know the gender of their partner, so partner gender is not ambiguous; (3) we specify the specific type of psychiatric hospitalization (depression or schizophrenia), adding further clarity to the meaning of the findings and allowing us to explore differences by diagnosis; and (4) we explore these processes for a nonpsychiatric illness. Unlike Lucas and Phelan, however, we examine interactions only between women, so we cannot determine if these results generalize to men. We did collect some data from men (42 nonsuspicious cases), but we do not have enough cases to examine these processes separately by gender. However, the findings are similar when those cases are folded into the analysis. We briefly describe those findings in the final portion of the results section.

METHODS

Sample

We collected data at a public university in the South during the 2010–2011 academic year from 136 female undergraduates. In the debriefing, 24 of these participants reported a suspicion that there was no partner, and 2 elected to have their data destroyed, a standard option on the debriefing form, leaving 110 nonsuspicious participants willing to have their data retained. The 24 suspicious cases were dropped on the basis of a recommendation from the lab worker (a research assistant ran all but four of the cases), who made recommendations using information gained during the debriefing and drop criteria established during the pilot for this study. The overall rate of exclusion is 17.9 percent, and rates by condition are 15.6 percent in the schizophrenia condition, 15.6 percent in the depression condition, 15.4 percent in the food poisoning condition, and 25.8 percent in the nonpatient condition. As we explain below, the partner in the three patient conditions provided an extra detail about "taking time off," which may have added believability to those three conditions, lowering the rate of suspicion. However, the difference does not reach significance when nonpatient condition (vs. patient) is used to predict drop status with logistic regression ($p = .196$).

Furthermore, when all 134 cases are included, the results are similar. We report most of the social distance and likability results for both the 110 nonsuspicious cases and the full sample of 134 cases, and we report the descriptive statistics for both groups in Table 1. The other descriptive details in the text (e.g., α reliability) are from the restricted sample of 110 cases, but those details for the full sample are available from the first author on request.

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Table 1. Descriptive Statistics for Variables in Analyses

	Nonsuspicious Cases (n = 110)				All Cases (n = 134)			
Variable	*M*	*SD*	Minimum	Maximum	*M*	*SD*	Minimum	Maximum
Dependent variables								
Social distance	3.09	1.07	0	5	3.06	1.18	0	5
Does not agree to meet after	0.25	0.44	0	1	0.26	0.44	0	1
Does not give full name	0.43	0.50	0	1	0.44	0.50	0	1
Does not give e-mail address	0.56	0.50	0	1	0.54	0.50	0	1
Does not agree to get to know socially	0.87	0.33	0	1	0.85	0.36	0	1
Does not say yes to both coffee and online	0.97	0.16	0	1	0.96	0.19	0	1
Partner likability[a]	65.06	16.33	27.00	100	64.78	16.42	26.33	100
Independent variables								
Condition								
Schizophrenia	0.25	0.43	0	1	0.24	0.43	0	1
Depression	0.25	0.43	0	1	0.24	0.43	0	1
Food poisoning	0.30	0.46	0	1	0.29	0.46	0	1
Nonpatient	0.21	0.41	0	1	0.23	0.42	0	1
Class standing[b]	0.53	0.84	0	3	0.54	0.81	0	3
Percentage of stays weighted inversely by popularity of initial choice	0.26	0.09	0	.45	0.27	0.09	0	.55
Partner status	64.40	14.52	34.60	100	63.96	14.18	34.60	100
Partner task performance	59.60	14.47	18.75	100	59.05	15.78	12.75	100
Partner evaluation	1.10	1.37	–4.3	4.3	1.13	1.41	–4.3	4.3
Partner potency	0.27	1.34	–4.3	3.0	0.27	1.36	–4.3	3.0
Partner activity	0.08	1.34	–3.0	4.3	0.12	1.34	–3.0	4.3

a. Also used as an independent variable in some social distance models.
b. Attribute of participant and partner.

Partner's Hospitalization History

We use self-reported hospitalization history to operationalize psychiatric and nonpsychiatric illness labels. At the beginning of the computerized instructions, participants learned that they would be working with a teammate on 25 "meaning insight tasks" (MITs). The instructions then asked them to fill out an electronic information sheet that would be exchanged with the partner. The instructions explained that "the educational, employment, and demographic information you exchange will be similar to the information you might obtain from co-workers at a job." The instructions also instructed participants, "Please answer the following questions about yourself carefully and accurately." The form asked participants for their gender, age, year in college, years of work experience, type of work experience, whether they had had to take a leave of absence from school or work, and, if so, the reason. The partner's responses to the last two questions served as the manipulation of the partner's hospitalization history, and these responses were randomly assigned by the computer program. In the nonpatient condition, the partner response to the leave of absence question was simply "No." In the schizophrenia, depression, and food poisoning conditions, the response was "Yes," with the following reason: "Last year I was hospitalized for schizophrenia/depression/food poisoning, so I took a little time off." The manipulations created four conditions: nonpatient partner, partner hospitalized for schizophrenia, partner hospitalized for depression, and partner hospitalized for food poisoning.

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The partner's responses were matched with the participant's on all the other questions, and we used broad response categories for all these questions except year in college so that the matching responses did not arouse suspicion. After participants were shown the partner's information, the instructions asked them to write the information down on a "partner information sheet" beside the computer, a task that ensured that participants in the psychiatric conditions saw the hospitalization information. We also fostered the idea that there was a partner with other techniques, including the lab worker's script, the use of walkie-talkies, door signs in the lab, and the posting of a fictitious companion study (ostensibly for partners) on the Web site at which students registered to participate in the study.

Social Encounter

After exchanging information with the partner, participants learned more about the MIT. This part of the experiment followed the standardized experimental situation for investigating status-organizing processes (Berger 2007). Participants learned that on each of the 25 MITs, the two teammates would be presented with two words from a reconstructed language (e.g., *yut-ken* or *yan-tek*) and one English word (e.g., *sharp*) and that their task was to determine which of the two words was more likely to be related to the English word. Through an example trial, the teammates learned that they would provide an initial answer that was shared and that each teammate would then privately enter her final answer. In reality, there were no correct answers, and the partner was computerized and programmed to give an initial answer that differed from the participant's on 20 of the trials (all but trials 1, 6, 13, 17, and 22). Participants were told that the teammates' final choices on each trial would be combined and that the team with the highest number of correct answers that semester would split a $100 bonus. This joint reward was designed to create a valued outcome and to motivate participants to work with their partners to find the correct answer. After the 25 trials, participants completed a postexperimental questionnaire.

We measured our outcome variables—social distance and partner likability ratings—after the MIT encounter and after the participant had rated the partner on numerous dimensions. It is possible, therefore, that the participants' MIT behavior and/or partner ratings mediate the effect of condition on social distance and partner likability ratings; that is, condition may affect MIT behavior and/or partner ratings, and it may be that behavior and/or those ratings that then affect social distance behavior and partner likability ratings. Therefore, we include numerous analyses to examine these possibilities.

Dependent Variables

Social distance. Behavior is the sum of five dichotomous items (no = 1) that ask participants if they would like to (1) stay after for 5 minutes to meet their partner, (2) give their partner their name (coded as 0 only if they share their full name), (3) give their partner their e-mail address, (4) get to know their partner socially, and (5) meet the partner for coffee and/or online (asked only of those who responded in the affirmative to question 4 and coded 0 only if they accepted both coffee and online meeting options). We administered these items in the following way: the computerized instructions stated, "The OU Department of Sociology encourages its researchers to give study participants who work on teams the opportunity to meet one another after the study is over. Therefore, if you have time, we want to give you the opportunity to meet your partner. The meeting will take about 5 minutes beyond the scheduled time for the experiment." Participants had the option of selecting "Yes, I have time to meet my partner after the experiment" or "No, I do not have time to meet my partner after the experiment." Beneath the two options, the instructions stated, "If you both have time for the meeting, after the study, the research assistant will introduce you to each other and let you talk for 5 minutes." On the next screen, participants were asked, "Would you like to give your partner your name and email address? If so, please provide that information below and we will give it to your partner after the study is over." Below this question, two lines ("My name is:" and "My email address is:") gave participants a place to type in one or both pieces of information.[3] On the next screen, participants were told,

> In addition to giving you the opportunity to meet your partner after the study, we also want to give you the opportunity to set up a future meeting with your partner. Indicate below if you would like us to tell your

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partner that you would like to get to know him or her socially outside of this study.

If the participant selected "yes," another question popped up below that one that said, "We can facilitate this meeting. Which type of meeting would you like us to arrange? Select all that apply." Below that, three options were listed "conversation on-line," "conversation at a local coffee shop," and "no arrangement, because I changed my mind." Participants could select one of them or both the online and coffee option, but if they selected "no arrangement," they could not also select the coffee or online option and they were coded 1 for both items 4 and 5. The distribution of the composite measure approaches normality (χ^2 for joint test of skewness and kurtosis = 2.05, p = .36), with the following distribution: 0 = 0.9 percent, 1 = 5.5 percent, 2 = 23.6 percent, 3 = 30.9 percent, 4 = 31.8 percent, and 5 = 7.3 percent. Descriptive statistics for this and the other variables in the analyses are presented in Table 1.

Partner likability. Partner likability is the summed average of three items: unlikable or likable, inconsiderate or considerate, and unpleasant or pleasant (α reliability = .86). The items were measured with 101-point semantic differential sliders that were placed below the stimulus "My partner." The order and direction of the adjective pairs were randomized across participants. The instructions introducing the measures emphasized that the ratings would not be shared with the partner. Partner likability is not normally distributed: the χ^2 statistic for the joint test of skewness and kurtosis is 4.86 (p = .09). Although taking the log and its square root improve its normality (logged, χ^2 = 1.20, p = .55; square root, χ^2 = 1.12, p = .57), the results are not substantively different when the transformed versions are used; therefore, we use the nontransformed version given its ease of interpretation.

Independent Variable

Class standing. The partner's class standing is matched to the participant's, so this attribute varies across conditions and is therefore controlled in most models. It is coded as 0 = freshman, 1 = sophomore, 2 = junior, and 3 = senior.

Possible Mediators

Although the instructions emphasized that the teammates would not see each other's final choices on the MIT, it is still possible that participants' social distance behavior and/or their partner likability ratings were, in part, a reaction to their earlier behavior during the MIT (e.g., embarrassment or compensation for resisting their partner's influence). Therefore, we control for participants' resistance to influence during the MIT in most of the models. We also measured social distance behavior after the participants had rated their partner on numerous dimensions. Although our instructions emphasized that teammates would not see each other's ratings, it is also possible that the social distance behavior was partially a reaction to or an affirmation of these ratings. Hence, we also control for the partner ratings when examining the effect of condition on social distance. We do the same for the partner likability models, although some of those controls cannot be mediators, because they were measured after partner likability. We describe the measures for these possible mediators below.

Percentage of stays. Percentage of stays represents the participant's resistance to influence during the MIT. Although the MIT is designed to give participants two equally plausible word options, the initial selections differed from a 50-50 divide for 11 of the 20 disagreement trials ($p < .05$, two-tailed tests), suggesting that the two options were not perceived as equally plausible on all trials. Hence, we created a stay score that weights each stay inversely by the popularity of the initial selection. Specifically, we multiplied the participant's stay score (0 or 1) by the absolute value of the difference between her initial answer (coded 1 or 2) and the average initial answer (1.5 if the selections were equally divided between the two choices). For example, on trial 2, 45 percent of the participants selected the option on the left (*pa-le*; coded 1), and 55 percent selected the option on the right (*se-weh*; coded 2), so the average score for trial 2 was 1.55. Thus, participants who selected the left option (the less popular option) and stayed with it were given a higher stay score (0.55) than those who selected the right option and stayed with it (0.45). The participants' stay scores were the average of these 20 differentially weighted stays. The distribution of the variable approaches normality (the χ^2 value for a joint test of skewness and kurtosis is 2.57, p = .28). We created sample-specific weights for the stay scores, so the weights for the stay scores used in the focal analysis came from only the 110 cases. We used the 110-case sample weights for the

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full (N = 134) sample, because these weights reflect the decision making of participants more fully engaged in the process and because these stay scores explain a bit more variance in both of the social distance models and in one of the two likability models than the stay scores that come from the full 134 cases. We also operationalized the percentage of stays three other ways.[4] The version we present provides the greatest explained variance in both the social distance and the likability models, although the differences across models are small.

Partner status. Partner status is a summed average of five items measured on 101-point sliders: not respected or respected, low status or high status, incompetent or competent, unknowledgeable or knowledgeable, and incapable or capable (α reliability = .87). The direction and order of the items were randomized across participants. Partner task performance is the summed average of four items measured on 101-point sliders that assess perception of the partner's MIT performance: usefulness of the partner's ideas, quality of the partner's contributions, partner's skill at the MIT, and responsible selections during the MIT (α reliability = .81). The direction and order of these items were randomized across participants.

Partner evaluation, power, and activity. Partner evaluation, power, and activity reflect the participants' rating of "my partner" on nine-point semantic differential scales. The evaluation scale was anchored with "good, nice" and "bad, awful"; the power scale was anchored with "powerful, big" and "powerless, small"; and activity was anchored with "fast, noisy, active" and "slow, quiet, inactive." The middle point on all three scales was marked "neutral," and the points between the midpoint and the endpoints were marked "slightly," "quite," "extremely," and "infinitely." The nine points were coded –4.3, –3, –2, –1, 0, 1, 2, 3, and 4.3. The direction and order of the adjective pairs were randomized across participants. The instructions introducing these scales emphasized again that the partner ratings would not be shared with the partner.

RESULTS

Social Distance

Table 2 shows the ordered logistic regression of social distance on condition with various controls. We use ordered logistic regression, because the distances between the attributes of the social distance variable are not uniform. Consistent with hypothesis 1, model 1 shows that participants seek more social distance from partners hospitalized for schizophrenia and depression than from nonpatient partners. The schizophrenia coefficient is larger than the depression coefficient in this and all other models except models 5 and 8. However, this difference does not reach significance in any of the models, suggesting that the stigma of asymptomatic schizophrenia is similar to the stigma of asymptomatic depression. Consistent with hypothesis 3, model 1 also shows that participants seek more social distance from partners hospitalized for food poisoning. This effect is smaller than the psychiatric hospitalization effects in all of the models, but this difference also does not reach significance in any of the models.

Model 2 shows that all three hospitalization effects hold when controlling for class standing. Model 3 shows that the percentage of stays does not reach significance and that the hospitalization effects do not change substantially when the stays are controlled, suggesting that this resistance behavior does not mediate the effect of condition on social distance behavior. Model 4 includes controls for partner ratings. None of the partner ratings reaches significance ($p < .05$), and the hospitalization effects do not decline in size or significance, indicating that the partner ratings also do not mediate the effect of condition on social distance behavior. Models 5 to 8 display the same four models using the full 134 cases: the 110 nonsuspicious cases combined with the 24 suspicious cases. These models show a similar pattern, but the hospitalization coefficients are smaller and less significant.

In Table 3, we examine the effect of hospitalization history on each of the first four items of the social distance composite measure. We exclude the fifth item simply because there is too little variation on it for a logistic regression. (Only three participants, two in the nonpatient condition and one in the depression condition, agreed to both the coffee and the online meetings.) Using logistic regression, we regress each item on condition, class standing, percentage of stays, and the response to the temporally prior social distance items. The share-name and share-e-mail questions were presented simultaneously, so we control each item in the final model of the other. The analyses use only the 110 nonsuspicious cases, but the results with the full 134 cases are similar and are available on request from the first author.

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Table 2. Ordered Logistic Regressions of Social Distance on Condition and Controls among Women

	Social Distance							
	Nonsuspicious Cases (n = 110)				All Cases (n = 134)			
Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Condition								
Schizophrenia (0 = nonpatient)	1.28*	1.43**	1.46**	1.49**	.96*	1.01*	1.09*	1.16*
	(0.52)	(0.53)	(0.53)	(0.55)	(0.46)	(0.46)	(0.46)	(0.48)
Depression (0 = nonpatient)	1.22*	1.23*	1.26*	1.46**	0.98*	0.94*	1.01*	1.29**
	(0.52)	(0.52)	(0.52)	(0.54)	(0.46)	(0.46)	(0.47)	(0.49)
Food poisoning (0 = nonpatient)	1.14*	1.20*	1.19*	1.29*	0.79†	0.80†	0.79†	0.91*
	(0.50)	(0.50)	(0.50)	(0.51)	(0.43)	(0.43)	(0.44)	(0.45)
Class standing[a]		–0.38†	–0.44*	–0.36		–0.33†	–0.45*	–0.42*
		(0.21)	(0.22)	(0.23)		(0.19)	(0.20)	(0.20)
Percentage of stays weighted inversely by popularity of initial choice			1.74	1.00			3.93*	2.92
			(2.06)	(2.26)			(1.76)	(1.97)
Partner likability				0.03				0.02
				(0.02)				(0.02)
Partner status				–0.04†				–0.02
				(0.02)				(0.02)
Partner task performance				0.01				0.01
				(0.02)				(0.02)
Partner evaluation				–0.24				–0.28*
				(0.15)				(0.14)
Partner potency				–0.05				–0.07
				(0.15)				(0.13)
Partner activity				0.03				–0.11
				(0.15)				(0.13)
Pseudo-R^2	.026	.036	.039	.057	.015	.022	.034	.052

Note. Coefficients are unstandardized; standard errors are in parentheses. Given space limitations, we do not present the multiple intercepts.
a. Attribute of participant and partner.
†$p < .10$, *$p < .05$, and **$p < .01$ (two-tailed tests).

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Table 3. Logistic Regressions of Social Distance Items on Condition, Percentage of Stays, and Earlier Social Distance Items among Women (n = 110)

	Does Not Agree to Meet After	Does Not Give Full Name			Does Not Give E-mail Address			Does Not Agree to Get to Know Socially	
Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Condition									
Schizophrenia (0 = nonpatient)	0.92	1.63*	1.72*	1.54†	0.91	1.13†	0.46	–0.03	–0.65
	(0.69)	(0.67)	(0.68)	(0.79)	(0.59)	(0.62)	(0.72)	(0.84)	(0.91)
Depression (0 = nonpatient)	–0.24	1.94**	1.93**	1.85*	1.17†	1.16†	0.32	–0.22	–0.71
	(0.78)	(0.68)	(0.68)	(0.78)	(0.61)	(0.62)	(0.74)	(0.84)	(0.91)
Food poisoning (0 = nonpatient)	0.89	1.12†	1.19†	0.99	0.71	0.91	0.49	0.41	0.04
	(0.67)	(0.66)	(0.66)	(0.76)	(0.56)	(0.58)	(0.64)	(0.88)	(0.91)
Class standing[a]	–0.29	–0.004	–0.02	0.34	–0.38	–0.45†	–0.61†	–0.55†	–0.45
	(0.31)	(0.25)	(0.25)	(0.31)	(0.24)	(0.25)	(0.32)	(0.30)	(0.31)
Percentage of stays weighted inversely by popularity of initial choice	–2.22	0.89	0.64	–1.82	3.54	3.20	3.58	0.49	–0.40
	(2.65)	(2.32)	(2.34)	(2.78)	(2.37)	(2.42)	(2.81)	(3.29)	(3.29)
Does not agree to meet after			–0.45	0.24		–1.09*	–1.19*		1.09
			(0.48)	(0.60)		(0.48)	(0.59)		(0.86)
Does not give full name							2.72***		0.60
							(0.56)		(0.78)
Does not give e-mail address				2.82***					0.87
				(0.60)					(0.73)
Constant	–0.87	–1.78	–1.64	–3.04	–1.16	–0.86	–1.35	2.13	1.84
	(0.84)	(0.81)	(0.82)	(0.98)	(0.74)	(0.76)	(0.88)	(1.02)	(1.06)
Pseudo-R^2	.053	.073	.079	.288	.054	.089	.293	.045	.099

Note. Coefficients are unstandardized; standard errors are in parentheses.

a. Attribute of participant and partner.

†$p < .10$, *$p < .05$, **$p < .01$, and ***$p < .001$ (two-tailed tests).

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Model 1 shows that the participants in the hospitalization conditions are just as likely as those in the nonpatient condition to decline to meet the partner after the study, although the depression and food poisoning coefficients approach marginal significance (p = .182 for both). Model 2 shows that participants paired with a partner hospitalized for schizophrenia or depression are significantly less likely to share their full name with their partner, an effect that reaches only marginal significance for those working with a food-poisoned partner. Model 3 shows that these effects hold when the response to the earlier query about meeting after the study is controlled. Model 4 shows, however, that when the share-e-mail response is controlled, the schizophrenia coefficient becomes marginally significant (p = .051) and the food poisoning effect becomes nonsignificant (p = .196).

As shown in model 5, participants in the schizophrenia and depression conditions are less likely than those in the nonpatient condition to share their e-mail address, although the differences do not reach significance (schizophrenia, p = .125; depression, p = .054). When the meet-after response is controlled in model 6, the depression coefficient is stable, and the schizophrenia and food poisoning coefficients increase somewhat in size and significance. But when the share-name response is controlled in model 7, all three hospitalization coefficients decline in size and significance. Model 7 also shows that participants who shared their full name were more likely than others to also share their e-mail address, whereas those who agreed to meet the partner after the study were less likely to do so. Models 8 and 9 show that condition is not significantly related to the participants' willingness to get to know the partner socially.

The findings in Table 3 show that participants in the hospitalization conditions were more reluctant to share identifying information than to meet with the partner. Perhaps in this era of concern regarding online safety and identity theft, there is some cultural acceptance for keeping names and e-mail addresses private, a cultural sentiment that may have made the participants paired with former patients feel comfortable declining the offer to share this information. Moreover, these are the two items that could be most effectively used to find a person both physically (a home address) and online (e.g., Facebook page, campus organizational affiliations). By contrast, a meeting in the laboratory in the presence of a research assistant poses few security risks, and a commitment to meet in the future could be left unfulfilled. Participants' greater willingness to meet the partner may also be due to the way we framed the meetings: we described both as contacts that the research team was facilitating, a framing that may have heightened social desirability pressures in the psychiatric conditions.

Partner Likability

Table 4 shows the ordinary least squares regressions of partner likability ratings on condition and various controls. Model 1 shows, contrary to hypotheses 2 and 4, that the partner's hospitalization history is unrelated to perceptions of partner likability. Models 2 to 4 show that this nonsignificance holds when controlling for class standing, percentage of stays, and the remaining partner ratings. Among the hospitalization coefficients, the schizophrenia coefficient comes the closest to marginal significance, approaching it in both models 2 (p = .117) and 3 (p = .113). Models 5 to 8 display the same four equations for the full sample, and the models show a similar pattern, although in model 7, the schizophrenia coefficient reaches marginal significance (p = .088).

Gender Differences

Numerous studies conducted in the 1970s showed that women behaved in a more tolerant way than did men when interacting with a psychiatric patient and that both women and men were more tolerant of female psychiatric patients than male psychiatric patients (see reviews in Farina 1981, 1998). But only two studies since the 1970s have explored these gender differences with behavioral outcomes (Lucas and Phelan 2012; Sibicky and Dovidio 1986), and although neither identified gender differences, features of each study preclude drawing definitive conclusions regarding the role of gender in contemporary stigma processes (Lucas and Phelan did not specify the gender of the participant's partner, and Sibicky and Dovidio examined only cross-sex pairs).[5] Therefore, it is not clear from these studies if or how our results would differ with different combinations of interactants (female-male, male-female, and male-male). But, given the increasing similarity between women and men in other realms—employment rates (England 2010), occupational

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Table 4. Ordinary Least Squares Regressions of Partner Likability Ratings on Condition and Controls among Women

	Partner Likability							
	Nonsuspicious Cases (n = 110)				All Cases (n = 134)			
Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Condition								
Schizophrenia (0 = nonpatient)	–6.44	–7.22	–7.28	–2.37	–6.33	–6.47	–6.97†	–1.55
	(4.65)	(4.57)	(4.55)	(2.78)	(4.13)	(4.10)	(4.06)	(2.39)
Depression (0 = nonpatient)	–1.36	–0.87	–0.98	–0.73	–1.95	–1.28	–1.63	–2.01
	(4.65)	(4.56)	(4.55)	(2.82)	(4.13)	(4.11)	(4.07)	(2.42)
Food poisoning (0 = nonpatient)	–3.40	–3.25	–2.91	0.11	–5.38	–5.06	–5.06	–0.77
	(4.45)	(4.36)	(4.36)	(2.64)	(3.95)	(3.91)	(3.87)	(2.29)
Class standing[a]		4.27*	4.71*	2.15†		3.25†	3.71*	1.43
		(1.84)	(1.87)	(1.20)		(1.75)	(1.75)	(1.05)
Percentage of stays weighted inversely by popularity of the initial choice			–22.77	18.45			–29.88*	16.50†
			(17.38)	(11.47)			(14.94)	(9.43)
Partner status				0.62***				0.70***
				(0.10)				(0.09)
Partner task performance				0.25*				0.19*
				(0.10)				(0.08)
Partner evaluation				1.09				1.22†
				(0.82)				(0.72)
Partner potency				1.19				1.22†
				(0.75)				(0.65)
Partner activity				0.94				0.40
				(0.74)				(0.66)
Constant	68.00	65.77	71.32	3.84	68.32	66.33	74.23	3.00
	(3.42)	(3.48)	(5.48)	(6.77)	(2.95)	(3.11)	(5.00)	(5.96)
R^2	.021	.069	.084	.686	.024	.049	.078	.705
Adjusted R^2	–0.007	.033	.040	.654	.001	.020	.042	.681

Note. Coefficients are unstandardized; standard errors are in parentheses.
a. Attribute of participant and partner.
$^{\dagger}p < .10$, $^{*}p < .05$, and $^{***}p < .001$ (two-tailed tests).

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training (England 2010), wages (Blau and Kahn 2006), and housework time (Sayer 2005)—it is possible that there is greater similarity today in women's and men's behavior toward stigmatized individuals and in the stigma that female and male patients experience. In line with those predictions, when we include in the analysis the 42 nonsuspicious male cases we gathered (male participants paired with a male partner), gender does not moderate the effect of condition on social distance (not shown but available on request from the first author). And the direction of the difference by gender suggests that if there is a difference, women today are less tolerant than men.

DISCUSSION

According to the modified labeling theory, the negative consequences of psychiatric labeling develop through three interrelated processes that begin at diagnosis. First, the personal relevance of negative cultural conceptions regarding mental illness creates feelings of demoralization. Next, the publically known psychiatric illness increases others' tendency to negatively evaluate and socially reject the patient. Then, as the first two processes unfold, the third process takes hold: patients begin to use coping behaviors—secrecy, withdrawal, and education—to ward off rejection, behaviors that, ironically, are expected to harm the patients by reducing support networks and employment opportunities. Although the first and third process have been investigated in recent studies (e.g., Kroska and Harkness 2011; Markowitz et al. 2011), the second process has received little attention in recent decades, particularly with studies that use behavioral measures of rejection. Even fewer studies have examined how a patient's diagnosis differentially affects this process or how the processes differ for those with a history of a nonpsychiatric illness. We examined those questions with an experimental design involving computer-mediated interactions between a female participant and a female teammate. We examined the effect of illness hospitalization and psychiatric diagnosis on both behavioral (seeking social distance) and verbal (perceived likability) reactions to the teammate. In this way, we provided a contemporary assessment of a key proposition in the modified labeling theory and provide a behavioral examination of the role of diagnosis in the rejection process. We also explored these processes for a nonpsychiatric illness.

Psychiatric Illness and Social Distance

We found, as predicted, that individuals sought greater social distance from individuals who reported a history of a psychiatric hospitalization than from those who did not. The computerized partners interacted with the participants in the same way across conditions, so the findings suggest that this information alone is enough to elicit social distance behavior. We also established that the effect was not mediated by other events in the experiment, such as the participants' resistance to influence during the MITs or the participants' assessment of the partner on various indices, results that suggest that the information about psychiatric hospitalization directly increased social distance behavior.

The social interaction at the heart of this study was unusual, because the participants were highly aware that they were being monitored and were, most likely, self-conscious about their behavior. Yet those features of the study—the participants' knowledge of researcher oversight and participants' likely effort to behave in socially desirable ways—make these findings even more striking: despite the oversight and self-awareness, participants sought social distance from psychiatric patients at a higher rate than they did from nonpatients. These findings provide fresh evidence that conflicts with Gove's (2004:365) and others' (Crocetti et al. 1974; Huffine and Clausen 1979) conclusions that, in the absence of disturbed behavior, the stigma of mental illness is "almost always transitory." A single instance of social rejection in the year following a psychiatric hospitalization does not constitute a long-term problem, but the social rejection that occurred in this experiment is indicative of what is likely to happen in a multitude of social interactions, which collectively could be understood to constitute a serious and long-term problem. Even a single year of heightened social rejection could set the stage for longer term mental health problems, given that perceived social rejection and perceived negative evaluation reduce life satisfaction (Markowitz 1998; Markowitz et al. 2011) and feelings of mastery (Markowitz et al. 2011) and increase self-deprecation (Wright et al. 2000) and depression (Link et al. 1997). Indeed, it is these everyday

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rejection experiences that, according to the modified labeling theory, make mental health recovery more challenging.

Diagnosis

Recent vignette studies of mental illness stigma suggest that individuals displaying symptoms of schizophrenia and other psychotic disorders elicit stronger stigma responses than do individuals displaying symptoms of depression (e.g., Pescosolido et al. 2010). Yet surprisingly few studies have examined situations wherein symptom information is not available. We found, in line with the highly limited research on this question (Francis and Heise 2006; Penn et al. 1994), that schizophrenia and depression labels presented without symptom information created similar reactions: a similar social distance response and no significant evaluative responses. These findings provide preliminary evidence that the second process of the modified labeling theory, wherein a publicly known diagnosis increases negative evaluation and social rejection, may in fact be a fairly general process, experienced with similar frequency and force by schizophrenic and depressed patients when information about their symptoms is unavailable.

Food Poisoning and Social Distance

Yet findings from our third hospitalization condition, food poisoning, suggest that psychiatric illness is not the only illness that produces social distancing. According to the disease-avoidance account of disgust, the human reaction to disgust is a fairly automatic and somewhat irrational system designed to avoid contact with stimuli that may be only distantly reminiscent or evocative of pathogens. Drawing on this perspective, we hypothesized that participants would be more likely to avoid social contact with individuals who reported a history of food poisoning hospitalization than from those who do not. Although a history of food poisoning does not pose a real pathogen threat, we expected that the mention of this history could evoke disgust and/or concern about the safety of the individual's food preparation practices and hygiene. In line with these expectations, participants were significantly more likely to seek social distance from partners who reported a history of food poisoning hospitalization than from the nonpatients. As with the patterns for psychiatric illness, we established that the effect was not mediated by other events in the experiment, suggesting that the food poisoning information directly increased social distance behavior. These findings fit with a growing body of literature suggesting that some forms of stigma and xenophobia are rooted in the human disgust response (Fincher and Thornhill 2012; Kurzban and Leary 2001; Oaten et al. 2009).

Likability

We also examined a nonbehavioral outcome, partner likability ratings, and found, unexpectedly, that psychiatric and food poisoning hospitalization did not affect it. The contrast between these results and the behavioral results suggests the verbal results may partially reflect participants' social desirability concerns. The contrast also suggests that these two types of outcomes may be gauging different types of attitudes, with verbal measures tapping overt, or explicit, attitudes and the more subtle behavioral measures tapping unconscious, or implicit, attitudes (Dovidio, Kawakami, and Beach 2001; Greenwald and Banaji 1995). Indeed, inconsistencies between attitudes and behavior, and even between explicit and implicit behavioral measures, are a well-documented pattern in the study of stigma (Crocker et al. 1998; Stier and Hinshaw 2007).

Consistent with this interpretation, our verbal and behavioral measures differ in explicitness in the way that attitude and behavior measures typically do. The verbal measures of likability were straightforward and clearly labeled, keeping participants aware of their own ratings and aware of the researcher's knowledge of their ratings. The social distance measures, however, were more subtle, because they gave participants socially acceptable excuses for seeking social distance. The first question, which asked participants if they had time to meet their partners after the study was over, explicitly gave participants a socially acceptable reason for declining: time constraints. With the time-constraint interpretation available, participants who wanted social distance, either consciously or subconsciously, could seek that distance without worrying that their decision would be viewed negatively. And, as shown in Table 3, participants in the schizophrenia and food poisoning conditions were more likely to do this than those in the nonpatient conditions, although the

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differences only approached significance. The other social distance questions—provide name, provide e-mail address, and meet the partner socially—could be comfortably declined by those who accepted the initial meeting because those participants could tell themselves (and could imagine others thinking) that they would share that information with the partner during the 5-minute meeting. And, as shown in Table 3, that is what many participants did when asked for their e-mail address: if they had agreed to meet the partner after the study, they were significantly less likely to share their e-mail addresses. Participants could also list only their first names on the provide-name option, a strategy that may have made them feel they were behaving in a socially desirable way, even though that information alone is unlikely to be useful for creating any kind of social follow-up after the study. In sum, the social distance measure gave participants multiple ways of maintaining social distance without worrying, and perhaps without recognizing, that they were behaving in socially undesirable ways, suggesting that the results from this measure provide a more accurate picture of the way individuals would actually behave in natural settings that lack researcher oversight.

The discrepancy between the verbal and behavioral results also suggests that studies that assess impressions of specific psychiatric patients with only explicit measures may underestimate participants' negativity and may offer few insights into how they would actually behave when interacting with that person. Future work should continue to examine these patterns with unobtrusive behavioral measures, and measures that are entirely unobtrusive are especially important. Although our social distance measure was more disguised than explicit measures, it was not entirely disguised. By contrast, Lucas and Phelan's (2012) social distance measure, described earlier, was fully disguised: it allowed participants to seek social distance without fear that anyone, including the researcher, would know what they were doing. Designs that include both explicit and unobtrusive measures are also critical, because they can illuminate discrepancies between outcomes while clarifying the relationship between explicit and implicit feelings.

Alternative Explanations

Given that both hospitalization and a leave of absence are unusual for food poisoning, it is possible that some of the participants in the food poisoning condition suspected that the partner's story was a cover for a more serious health problem, perhaps a debilitating condition or a mental illness. If so, these participants' social distance efforts would have been rooted in a desire to avoid contact with individuals with those imagined and more serious conditions. It is also possible that some participants in the food poisoning condition thought their partners' leave-of-absence response to this typically short-term problem (even if it included a hospitalization) was an overreaction, an interpretation that may have led them to see the partners as weak or hypochondriacal. In this case, the participants' social distance would have been aimed at avoiding contact with a hypochondriac.

Given the consistency between the food poisoning results and other studies of disgust, we do not see these interpretations as necessary to understand the food poisoning results. Nonetheless, future experiments could examine these processes in ways that eliminate these interpretations. For example, the partner could disclose a history of food poisoning without mentioning a hospitalization or a leave of absence. Researchers could also operationalize the threat of pathogen exposure with other stimuli, such as information about the partner's hygiene or food preparation practices. And researchers could assess the hypochondriac interpretation by contrasting the results of a food poisoning disclosure with and without information about a leave of absence.

It is also possible that the participants' efforts to seek social distance in all three hospitalization conditions were prompted by the partner's disclosure of personal information rather than by her illness per se. Although this interpretation is possible, we do not see it as likely given that the disclosures were provided in response to a question specifically asking for the information (reason for the absence), with instructions to answer questions "carefully and accurately." Nonetheless, future studies could explore this possibility by contrasting the effects of psychiatric and other conditions when revealed through self-disclosure versus other means. It may be that the self-disclosing patients are stigmatized to a greater extent than patients who do not self-disclose.

Future Research

Our study provides new evidence of the causal influence of illness labels on behavioral measures

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of social rejection, but we see several avenues for future research. We have already noted some: studies that include male participants and that vary both the gender of the participant and the gender of the patient, studies that use more fully unobtrusive measures, and studies that vary the mechanism through which the psychiatric information is disclosed (self-disclosure vs. other). But we see other possibilities as well.

First, researchers could explore these questions in other domains, such as employment and housing. Employment-based discrimination could be examined with audit studies or résumé experiments wherein application materials vary only in hospitalization history or medical status. Housing discrimination could also be examined with audit studies that build on existing work in this area (e.g., Page 1995).

Second, given the methodological challenges to investigating the ways individuals interact with psychiatric or other types of patients, we see benefits to using the computerized version of affect control theory, *Interact*, to simulate these social interactions. The program is based on the empirically derived impression formation equations that underlie affect control theory (Heise 2007), so these simulation results are themselves empirically based. But the results can also be used to generate finely tuned hypotheses for complex social interactions that consider interactant attributes, including gender and psychiatric diagnosis (Kroska and Harkness 2011). These empirically based hypotheses can then be investigated with other techniques, including both surveys and experiments.

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NOTES

1. Penn et al. (1994) did include symptom-free vignette characters with diagnoses, but all of their vignette characters carried a diagnosis, so they could not determine if responses to characters with a diagnosis differed from responses to characters without one.
2. The values are infinitely bad (–4.3), extremely bad (–3), quite bad (–2), slightly bad (–1), neither good nor bad (0), slightly good (1), quite good (2), extremely good (3), and infinitely good (4.3).
3. Students could provide a bogus e-mail address here, so we compared all reported University of Oklahoma (OU) e-mail addresses with students' actual OU e-mail addresses (both their university-issued addresses and their own modified version). We found one student who provided an incorrect OU e-mail address (off by two characters), so we coded her with a 1 for this item. We could not do this kind of check for the six e-mail providers who listed non-OU e-mail addresses.
4. The three ways are (1) the traditional, unweighted approach (percentage of 20 disagreement trials in which participants stay with their initial answer for their final answer), (2) a version that gives a greater weight to stays on trials with a low stay rate (the stay score divided by the percentage who stayed for that trial), and (3) a combination of the two weighted stay scores, created by taking their product. The bivariate correlations among all pairs of the four versions are .94 or higher.
5. Sibicky and Dovidio's (1986) exclusive use of cross-sex pairs could have masked women's greater tolerance: both women and men have historically been more tolerant of female psychiatric patients, so the equality in women's and men's reactions suggests that women actually behaved in a more tolerant way, given that they were interacting with the less tolerated patients (men).

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