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## Written Emotional Disclosure: Testing Whether Social Disclosure Matters

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### Abstract

Studies suggest that written emotional disclosure can improve health. Unknown, however, is whether the presence or absence of an audience for one's disclosure matters, and whether time management control writing has any effects. Undergraduates ( $N = 165$ ) with unresolved stress were randomized to 1 of 3 groups that wrote for 4 sessions: shared written disclosure (submitted to researchers), private written disclosure (not submitted), or time management control writing; or to a fourth group (no-writing control). At 3-month follow-up, the two control groups were equivalent on outcomes. Both shared and private disclosure resulted in less cognitive intrusion and avoidance than the combined control groups. Yet, shared disclosure reduced depression and interpersonal sensitivity more than either private disclosure or the control groups, and only shared disclosure reduced physical symptoms. Although truly private writing improves cognitive stress effects, shared writing has broader benefits, suggesting that social disclosure for one's writing matters.

### Written Emotional Disclosure: Testing Whether Social Disclosure Matters

The inhibition or avoidance of negative emotions (Gross & Levenson, 1997) and the suppression of thoughts (Wenzlaff & Wegner, 2000) leads to heightened physiological arousal, negative mood, and impaired cognition. In contrast, accessing, expressing, and processing inhibited emotions is thought to be adaptive. Perhaps the clearest demonstration of this stems from the written emotional disclosure or expressive writing paradigm (Pennebaker & Beall, 1986). In the standard version of this paradigm, participants are randomized to write for 15 to 20 minutes daily for several days about either stressful experiences (disclosure) or non-emotional topics (control). Changes in health and adjustment from baseline to follow-up several months later are compared between groups to determine the effects of disclosure.

Studies of emotional disclosure have been conducted on three types of participants (Harris, 2006). Many of the early studies were conducted on unselected, healthy young adults, and a meta-analysis indicated that these studies often showed moderate sized effects across a range of outcomes (Smyth, 1998). More recent studies have been conducted on people with medical disorders, such as asthma, rheumatoid arthritis, cancer, or fibromyalgia, and the results of these studies have been mixed, with weaker effects than found in healthy young adults (Frisina, Borod, & Lepore, 2004). Finally, a third set of disclosure studies has examined people who were selected because they had experienced a stressor or had unresolved stress problems. Although no benefits have been reported for people who are bereaved (Range, Kovac, & Marion, 2000; Stroebe, Stroebe, Schut, Zech, & van den Bout, 2002), studies of people who

report other unresolved stressors have shown benefits of written disclosure (Sloan & Marx, 2004a; Sloan, Marx, & Epstein, 2005).

### Truly Private Disclosure?

It is noteworthy that the disclosure paradigm was developed explicitly as an asocial procedure—the effects of private writing were to be tested unconfounded by the presence or feedback of a listener or therapist (Pennebaker, 1997). Indeed, written disclosure has been described as “solitary” and “anonymous” (Pennebaker, 2002), and parallels are sometimes drawn between written disclosure and private journaling or diary-keeping. We propose, however, that written disclosure in the standard study is not asocial, and the parallel with truly private journaling is inaccurate. Almost all disclosure studies have participants submit their writings to the research team. This is done primarily as a manipulation check to verify that writing occurs as instructed, although the submission of writings also permits secondary analyses on the content of the writings. Yet, the fact that one's writing is given to—and presumably read by—one or more researchers means that there is an audience for the writing and renders written disclosure a social experience that is not truly private. Indeed, writing in these studies is conducted not only with an “implicit audience” or imagined reader in mind (Brody & Park, 2004), but an actual audience—the research team.

It is not known whether and how sharing one's disclosure writing with a researcher affects outcomes. Perhaps it does not matter. A leading theoretical view of the mechanism of emotional disclosure is solely intrapersonal and ignores social factors: resolution of stress occurs when affective memories are accessed, put into language, and integrated into a developing and coherent story of the self and the world (Lepore, Greenberg, Bruno, & Smyth, 2002; Lutgendorf & Antoni, 1999; Sloan & Marx, 2004b). If this is the mechanism of disclosure, then whether or not there is an audience for one's disclosure should be irrelevant. Alternatively, perhaps sharing one's disclosures with another person interferes with the benefits of disclosure. Writers might censor themselves, avoid disclosing embarrassing or shameful experiences, or be more concerned with the potential reactions of the reader than with fully engaging in the exposure and resolution process (Brody & Park, 2004). Indeed, early research on disclosure found that verbally disclosing to an unseen person behind a screen (analogous to a religious confessional) was associated with inhibition of disclosure compared with solitary disclosure to a tape recorder (Pennebaker, Hughes, & O'Heeron, 1987). Furthermore, Pennebaker (2002) has editorialized that one large study that found no benefits following verbal disclosure to a health professional (Schilte et al., 2001) might be due to censoring that does not occur with the “solitary” disclosure of expressive writing.

It is possible, however, that sharing one's disclosures with a researcher enhances the effectiveness of the technique. Surveys show that the vast majority of people who have been through traumatic or even mildly upsetting experiences desire to talk about them with others (Rimé, 1995). It also is possible that knowing that one's writing will be read by others leads to a sense of accountability so that one is more adherent or works harder at the writing task, which could lead to better outcomes. Also, we suspect that some experiences, especially those that are associated with shame or guilt and have led to secrecy, may need to be shared beyond oneself to reverse the secrecy, which would not happen with truly private writing.

Interestingly, of the scores of published disclosure studies, we know of only two that allowed participants to retain their writings rather than share them with the researchers. Broderick, Stone, Smyth, and Kaell (2004) had patients with rheumatoid arthritis write at home and retain their writings, and this study did not replicate the positive effects of disclosure found by this same research team among rheumatoid arthritis patients who wrote under laboratory conditions, including submitting their writings (Smyth, Stone, Hurewitz, & Kaell, 1999). Ames et al. (2005) included written disclosure for half of the participants in an office-based smoking

cessation program, and writing was done at home and retained. There was no effect of writing on smoking-related outcomes. Although many other methodological factors (e.g., home writing, low patient motivation) may have contributed to the lack of disclosure effects in these two studies, it is possible that a failure to share one's writings also attenuated the potential benefits of disclosure.

### **The Effect of the Neutral Control Writing Condition**

The standard disclosure paradigm includes a control condition in which participants write about an emotionally neutral topic in the same format and duration as disclosure writing. Yet, the control condition has been controversial. Some studies that report benefits of disclosure actually have effects that are due more to deterioration of the controls than to improvement of the disclosure group (Lepore & Greenberg, 2002; Lumley & Provenzano, 2003; Park & Blumberg, 2002; Pennebaker et al., 1990; Pennebaker et al., 1988; Taylor, Wallander, Anderson, Beasley, & Brown, 2003). This is particularly true with early studies that often used a “trivial” topic control, such as writing about the contents of one's closet. Of course, worsening among controls could reflect the natural trajectory of declining health and functioning over the course of the study, which is prevented by disclosure. Control group deterioration, however, could result from some noxious aspect of the control condition, such as having to write repeatedly about trivial or meaningless things or having to suppress thoughts and feelings about stressors, particularly if prompted to think about stressors by the recruitment or consent process.

As the disclosure literature has evolved, there has been a shift toward using personally relevant and engaging control topics that are sometimes presented as credible methods to reduce stress. For example, in time management control writing, the rationale is that poor time management is a source of stress, and writing about time management may be helpful. Participants then write about their daily activities in a factual, unemotional manner. In effect, researchers have attempted to create a placebo condition—a credible but theoretically inert writing task—although it has not typically been labeled a placebo. Surprisingly, the credibility of such emotionally neutral, placebo writing conditions has rarely been reported. Lumley and Provenzano (2003) found that “time management and future plans” writing was as credible as disclosure writing. We also do not know whether a placebo control writing condition actually leads to any health benefits, as might be expected if one extrapolates from the psychotherapy placebo literature (Lambert & Ogles, 2004).

One should test whether the neutral control condition is as credible as disclosure writing. More important, however, is testing whether neutral writing is indeed neutral in its effects rather than either iatrogenic or salubrious. The latter can be done by including a no-writing, assessment only condition along with the neutral writing condition. Interestingly, we know of only three studies that included both neutral writing and no-writing conditions. Broderick, Junghaenel, and Schwartz (2005) reported no differences between time management writing and no-writing for people with fibromyalgia, and Gallant and Lafreniere (2003) reported no differences between trivial and no-writing conditions among adolescent children of alcoholics. Richards, Beal, Seagal, and Pennebaker (2000) included trivial writing and no-writing for three days among psychiatric prison inmates and found that the trivial writing group was more anxious than the no-writing group at 6-week follow-up, but otherwise, there were no differences.

### **Goals and Hypotheses of the Current Study**

This study had four goals. First, we sought to replicate previous findings of the benefits of written emotional disclosure in comparison with neutral, control writing by testing it on young adults with unresolved stressors. We evaluated its effects on cognitive manifestations of unresolved stress—intrusions and avoidance—as well as the emotional and physical consequences of stress, depression and physical symptoms. We also included interpersonal

sensitivity as an outcome, which we thought was particularly relevant in this test of the effects of social sharing of disclosure writing, and we thought that the experience of sharing one's disclosures with others might decrease interpersonal sensitivity.

A second and more important goal was to test whether truly private written disclosure that is not shared with anyone yields effects that are smaller, larger, or the same as written disclosure that is shared with the research team. This issue has both theoretical and clinical implications. Theoretically, if the effects of written disclosure are either enhanced or attenuated when one writes without an audience, then the theorized mechanism for the effects of disclosure writing must consider the social component. Yet, if there is no difference in effects between shared versus private disclosure, then the mechanism would appear to be independent of social considerations. Clinically, knowledge of the effects of private versus shared writing can inform recommendations regarding whether one should write completely privately or share one's writings with another person. To accomplish this goal, we added an experimental group in which participants kept their disclosure writing completely private.

Our third goal was to test the credibility of the commonly used time management control writing condition against written emotional disclosure. This was done by assessing expectations of the stress-reducing effects of writing after participants learned about the rationale and procedures of their assigned writing task. The fourth goal was to test whether time management writing was indeed neutral or whether it had either positive or negative effects. We did this by including an additional control group—a no-writing, assessment only condition.

## Method

### Participants

Participants were undergraduate psychology students who reported having a moderately serious unresolved stressful experience. At the start of the semester, students completed a web-based screening measure that included the items, “have you ever experienced a traumatic, stressful, or bothersome event?” and, if so, “how much does the event still bother you?” The latter was rated “not at all,” “somewhat,” “moderately,” or “very.” Students who reported experiencing a stressful event that continued to bother them at least “moderately” were targeted as participants. To increase generalizability, we minimized exclusion criteria to these two: prior participation in an emotional disclosure study or not being able to read and write English.

Nearly 2000 undergraduates completed the screening, and 253 met inclusion criteria and were contacted. Of these, 165 (65.2%) agreed to participate, completed baseline measures, and were randomized. The sample was primarily female (81.8%), averaged 20.9 years old (18 to 53 years), and was either European American (53.9%), African American (29.7%), Asian (7.3%), Hispanic (3.6%), or other (5.5%).

### Constructs and Measures

**Intervention credibility**—After participants learned about their writing assignment and its rationale, they completed the 6-item Credibility Scale (Borkovec & Nau, 1972) to assess how much they thought their assignment was credible as a stress management technique. Example items were, “How logical does this type of technique seem to you for helping people manage stress in their lives?” and “How confident would you be in recommending this technique to a friend?” Items were rated from 0 (not logical at all) to 6 (very logical) and averaged. Higher scores indicate greater credibility. We found an alpha of .91 for this measure.

The following outcome measures were completed at both baseline (pre-randomization) and follow-up.

**Cognitive intrusions and avoidance**—We used the 15-item Impact of Events Scale (IES; Horowitz, Wilner, & Alvarez, 1979) to assess cognitive intrusions and avoidance related to a specific stressor. Items were rated from 0 to 3 for the frequency of occurrence during the past month, and mean scores for each of the two subscales were calculated. The two subscales are independent enough to justify separate analysis (Sundin & Horowitz, 2002). We found alphas of .86 and .77 for the baseline avoidance and intrusion scales, respectively. The IES is designed to be completed with respect to a specific stressor. Therefore, in this study, all participants were instructed at baseline to “spend a few moments to identify one particularly stressful experience. Some stressors happen only once to a person, whereas other stressors happen repeatedly or continue for a long time and may even be happening right now. Please try to identify a stressful experience that continues to bother you. This may be a stressful experience that that you have not talked about or shared much with other people, or it may be one that you don't like to be reminded of.” Participants then completed the IES with respect to that stressor. At follow-up assessments, participants were reminded to complete the IES with respect to the same stressor to which they referred at baseline.

**Depression and interpersonal sensitivity**—These two constructs were assessed using 6-item depression and the 4-item interpersonal sensitivity subscales of the Brief Symptom Inventory (BSI; Derogatis & Melisaratos, 1983), which assesses a person's emotional, behavioral, and interpersonal functioning over the past 2 weeks. Participants rated each item on a 5-point scale, and items were averaged. We found alphas of .82 and .80 at baseline for depression and interpersonal sensitivity, respectively.

**Physical symptoms**—The Physical Health Symptoms Scale (Greenberg et al., 1996) lists 36 different health problems ranging from a runny nose to abdominal pain, and participants endorsed the severity of each item over the last 2 weeks. Items were rated from 0 (not at all) to 4 (extremely severe) and totaled for a physical symptom score. The alpha at baseline was .90.

## Procedure

From October 2002 to November 2003, potential participants were contacted by email, informed that they were being recruited because they reported having experienced a stressor that still bothered them, and informed that the study involved questionnaire completion several times during the semester and the possibility of being asked to engage in a 4-session writing exercise. Upon arrival in the laboratory, participants provided written informed consent to the first of two IRB-approved consent forms. This first consent was for the assessment procedures of the study, which applied to all participants, and it noted that only some participants would be randomly invited to participate in an additional writing exercise. No details of the writing exercise were given at this time. After completing baseline questionnaires, including the IES on which all participants reported the effects of an unresolved stressor, participants were then assigned to groups. Group assignment had been conducted and coded prior to the study by a person not involved with running of the study who used a random numbers table and assigned participants within blocks of four within each gender into one of the four experimental groups. A research assistant, who had been blind to group assignment during the baseline assessment, then accessed the next assignment code for that participant's gender and determined whether the participant was to continue into the writing phase or not. The 25% of participants who were randomized to the no-writing control group were simply scheduled for follow-up assessment and dismissed. The 75% who were randomized into one of the three writing groups were given a second consent form about the writing procedures, which noted that “you will be asked to write about personal experiences or about daily events,” and it described the format of the writing exercise. No one decline to participate in the writing portion of the study.

Those participants who were writing then were escorted to a private room, and the research assistant gave them a written sheet that contained the rationale and instructions appropriate to their assigned group, along with an audiotape player that had the identical language recorded on it. Participants were left alone to read the sheet and simultaneously listen to the audiotape. We presented the rationale and instructions both in writing and on audiotape to assure that the participant obtained the information thoroughly and did not just skim through the written sheet. We chose not to have the research assistant verbally instruct the participants in order to eliminate experimenter demand effects, variability in presentation of the instructions, and the possibility of emotional disclosure to the assistant. After reading and hearing the group information, participants completed the Credibility Scale, and they answered a manipulation check question about whether they were going to submit their writing to the research team. The research assistant then entered the room, examined this manipulation check answer to verify that the participants correctly understood whether they were going to submit their writings, and corrected participants if they were incorrect or unsure.

Participants then were left alone and wrote by hand (no computer use was allowed) on their assigned topic for the first of four, 20-minute sessions. Although many disclosure studies have used consecutive writing days in the laboratory, we sought to balance experimental control with feasibility for our participants, most of whom commute to the university only a few days per week. Thus, we decided to conduct four sessions of writing over a 1-week period and to have writing sessions 2 and 3 conducted at home. At the end of the first writing session, participants were given writing journals and instructed to find a private place to write twice during the next few days where they would not be interrupted for 20 minutes. Writing day 4 occurred in the laboratory, one week after the first writing session, and participants brought their writings from days 2 and 3 back with them. All participants (including the no-writing control group) were scheduled for follow-up questionnaires in the laboratory 1 month and 3 months after randomization. Participants received up to four hours of course credit for participating, or \$10 per hour up to \$40 if course credit was not needed.

## Experimental Groups

There were 4 experimental groups:

**Shared disclosure**—This group and the private disclosure group (below) were given the following rationale for the disclosure writing exercise: “The goal of this project is to see whether writing for 4 days about a stressful event in your life will reduce stress and therefore improve your health and functioning. If you are like most people, you have had some stressful experiences or events during your life. Research has found that when people write about stress, they feel less stressed.” These participants were then instructed to write about the stressful experience that they had just identified and about which they had completed the IES. Specifically, there were instructed:

As you write over the 4 days, your task is to do the following: a) try to make your memories of the stressful experience as vivid as possible, including mental images, emotions, and sensations in your body; b) try to describe both the facts about the experience, and also write about your deepest feelings about it; c) try to write as much as you are able, even if there is some part of the experience that you are reluctant to write about. Over the 4 days of writing, you should try to work on and resolve the stressful experience, and this means that you should write about the same stressful experience over all 4 days. As you write, try to make sense of your stressful experience—to understand its meaning. This might include trying to answer questions about why the experience occurred, how it has affected you—such as your beliefs, your relationships, or your actions—and ways that you might cope with the experience now.”

Importantly, this group was informed at the beginning of the study and on the cover sheet for each day's writing that they would have to turn their writing in to the research team after writing; all participants did so.

**Private disclosure**—This group received the identical rationale and writing instructions as the shared disclosure group. However, they were also informed that “all of your writings will remain private. The researchers will not read them or collect them, and you should not show your writings to anyone. You will take your writings with you each day, and you should either store them somewhere safe and private, or you should destroy them. Again, no one is going to read what you have written.” Cover sheets for each day reminded these participants of the private nature of the writing. It was important from an experimental control perspective, however, to confirm that writing actually occurred. So, at the end of day 1 and on day 4 (when participants brought also back to the laboratory the writings from days 2 and 3), the participant quickly showed the journal pages to the research assistant, who visually verified that writing had occurred, but participants did not give the writings to the assistant or allow the assistant to read them. All of these participants retained their writings.

**Placebo writing control**—The rationale for this writing was stated in a parallel fashion to that of the two disclosure groups: “The goal of this project is to see whether writing for 4 days about how you manage your time will reduce stress and therefore improve your health and functioning. If you are like most people, how you spend your time can be a source of stress. Research has found that when people plan their time or organize their activities better, they feel less stressed.” Participants then engaged in time management writing, focusing on what they did with their time during over the last week (day 1) and last 24 hours (day 2), and what they planned to do with their time over the next 24 hours (day 3) and the next week (day 4). Participants were instructed, “As you write, you should try to stick to your actual behaviors or your planned actions. Try not to write about your feelings about what happened or what is going to happen, and try to avoid giving your opinions. Write only about the facts—what happened, perhaps day by day or hour by hour—or what you plan to do in the next day or week, but not about your feelings or opinions.” These participants were informed that they would submit their writings to the research team, which they did after day 4.

**No-writing control**—This group was not informed about any writing options, did not participate in writing, and only completed the baseline and follow-up assessments.

### Approach to Data Analysis

A sample of 165 participants were randomized (shared disclosure:  $n = 42$ ; private disclosure:  $n = 41$ ; placebo-writing control,  $n = 42$ ; no-writing control,  $n = 40$ ). However, 9 participants from the three writing groups dropped from the study after day 1, and another 17 participants from all four groups failed to complete any follow-up assessment. Thus, 139 participants (84.2% of those randomized) conducted the intervention and provided follow-up data, and these participants constitute the “completer” sample.

Our primary analyses were conducted on the completer sample because they both completed the writing and had outcome data available. Of these 139 participants, 91 completed both 1-month and 3-month follow-up assessments, whereas the other 48 completed only one of the two time points, typically the 1-month follow-up. (In most cases of a missing follow-up assessment, the 3-month follow-up point occurred after the end of the semester, at which point some participants were no longer available or motivated to complete the study.) We were interested in the effects at the most distal follow-up point available. Thus, we used the last follow-up assessment point that was available, which was typically the 3-month point, but the

1-month point if necessary. Thus, the average time to follow-up for all participants was 2.72 months. Because of this variability, time until follow-up was covaried in all outcome analyses.

For our primary analyses, we conducted analyses of covariance (ANCOVA) in which the follow-up value was predicted from group, covarying both the baseline level of the outcome and time to follow-up. We had two primary sets of analyses. We first determined whether the placebo writing group was indeed neutral by comparing it to the no-writing group. Second, we compared the shared and private writing groups with each other and with the controls. In addition to indices of statistical significance, two types of effect sizes were computed for each outcome measure for each 2-group comparison. First, we present the partial eta squared ( $p\eta^2$ ) statistic, which estimates the proportion of variance in the outcome related to the group factor while holding constant baseline scores and time to follow-up. Values of  $p\eta^2$  of .01, .06, and .14 are considered to be small, medium, and large, respectively. Second, we computed Cohen's *d*, which is calculated as the difference between the two groups on the change in the outcome measure (follow-up minus baseline) divided by the pooled standard deviation of the change scores. (Note that the *d* statistic in this case does not consider the time to follow-up.) Values of *d* of .20, .50, and .80 standard deviations are considered small, medium, and large, respectively.

Although primary analyses were on completers, it is recommended to include in analyses people who dropped from the study. Thus, we repeated the ANCOVAs on the randomized sample of 165 participants, replacing missing outcome data with each participant's baseline value and using the sample mean follow-up time of 2.72 months. For these secondary analyses, we simply report any changes in significance from the findings with the completer sample.

## Results

### Attrition Analyses

The 26 participants who dropped from the study were compared with the 139 who completed. There were no significant differences between drop-outs and completers on age, gender, race, or baseline levels of any outcome measure (all  $p > .05$ ). Importantly, drop-outs came equally from the four groups: shared disclosure ( $n = 6$ ), private disclosure ( $n = 9$ ), placebo writing control ( $n = 5$ ), and no-writing control ( $n = 6$ ),  $\chi^2(3, N = 165) = 1.74, p = .63$ .

### Credibility of the Writing Interventions

The three writing groups were compared on perceived credibility of the writing exercises as a method to reduce stress. Credibility scores for the three groups did not differ,  $F(2,100) = 0.22, p = .80$ . Indeed, the scores (possible range of 0 to 6) of the groups were very similar: shared disclosure ( $M = 3.54, SD = 1.06$ ), private disclosure ( $M = 3.41, SD = 1.18$ ), and placebo writing control ( $M = 3.58, SD = 0.97$ ).

### Effects of the Placebo Writing Control Versus No-Writing Control

The first set of analyses tested the neutrality of the placebo writing control group ( $n = 37$ ) by comparing it with the no-writing control group ( $n = 34$ ) for the completer sample. Table 1 presents the baseline and follow-up means and standard deviations for these two control groups, along with the follow-up adjusted means (and standard errors), which are adjusted for the baseline value and the time to follow-up. At baseline, the two groups were similar on both intrusions and avoidance (both  $p > .45$ ); however, the placebo-writing control group was more depressed, interpersonally sensitive, and had more physical symptoms than the no-writing control group at baseline (all  $p < .05$ ).



Regarding outcomes, ANCOVAs indicated that the two control groups did not differ at follow-up on any of the outcomes: intrusions,  $F(1, 67) = 0.05, p = .83$ ; avoidance,  $F(1, 67) = 0.03, p = .86$ ; depression  $F(1, 67) = 0.21, p = .65$ ; interpersonal sensitivity,  $F(1, 67) = 0.11, p = .74$ ; or physical symptoms,  $F(1, 67) = 0.01, p = .92$ . Indeed, the effects of these two groups were very similar, as shown in Table 1. In secondary analyses of the full randomized sample, the placebo-writing control group again did not differ from the no-writing control on any of the five outcomes (all  $p > .47$ ).

### Effects of Shared and Private Written Disclosure

The second set of analyses examined the effects of both shared and private written disclosure. Because the placebo writing and no-writing control groups were very similar in their effects, we followed the procedure used by both Broderick et al. (2005) and Richards et al. (2000) and combined the two control groups into a single control group ( $n = 71$ ). We then compared each of the two disclosure groups (shared:  $n = 36$ ; private:  $n = 32$ ) and the single combined control group using 3-group ANCOVAs, followed by pairwise ANCOVAs comparing the two disclosure groups with each other and with the combined control group. Table 2 presents the baseline and follow-up data for these three groups, along with the adjusted means from the 3-group ANCOVA. At baseline, the 3 groups were not significantly different on any demographics or baseline levels of the outcomes (all  $p > .13$ ).

For intrusions, there was a significant group effect,  $F(2, 134) = 4.07, p = .02$ . Post-hoc ANCOVAs indicated that shared and private disclosure did not differ  $F(1, 64) = 0.90, p = .35$ . However, shared disclosure led to marginally less intrusion,  $F(1, 103) = 2.90, p = .09$  (small to medium effect), and private disclosure led to significantly less intrusion,  $F(1, 99) = 8.54, p = .004$  (medium effect), than occurred in the combined control group. The results were unchanged for the randomized sample.

For avoidance, there was a significant group effect,  $F(2, 134) = 4.70, p = .01$ . Again, the shared and private disclosure groups did not differ,  $F(1, 64) = 0.32, p = .57$ . Both shared disclosure,  $F(1, 103) = 8.15, p = .005$ , and private disclosure,  $F(1, 99) = 4.34, p = .04$ , resulted in significantly less avoidance than did the combined control group (medium effects). For the randomized sample, the private disclosure group fell to only marginally lower on avoidance than the combined controls,  $F(1, 119) = 2.80, p = .097$ .

For depression, there was a significant group effect,  $F(2, 134) = 7.96, p = .001$ . In this case, shared disclosure led to significantly less depression than did private disclosure,  $F(1, 64) = 7.41, p = .008$  (medium effect). Shared disclosure also led to less depression than the control group,  $F(1, 103) = 17.81, p < .001$  (large effect), whereas private disclosure did not differ from the control group,  $F(1, 99) = 1.53, p = .22$ . The results were unchanged for the randomized sample.

For interpersonal sensitivity, there was a significant group effect,  $F(2, 134) = 5.23, p = .006$ . Shared disclosure was marginally more effective than private disclosure in reducing interpersonal sensitivity,  $F(1, 64) = 3.36, p = .07$  (small to medium effect). Shared disclosure led to significantly less interpersonal sensitivity than did the combined control group,  $F(1, 103) = 9.78, p = .002$  (medium to large effect), but private disclosure was not different than the control group,  $F(1, 99) = 2.04, p = .16$ . For the randomized sample, the shared disclosure group became significantly (rather than marginally) lower on interpersonal sensitivity than the private disclosure group,  $F(1, 79) = 4.70, p = .03$ .

Finally, for physical symptoms, the group effect did not quite reach statistical significance,  $F(2, 134) = 2.20, p = .11$ . Nonetheless, we conducted post-hoc pair-wise tests to explore the differences among the three groups. Although shared disclosure did not differ from private

disclosure,  $F(1, 69) = 2.69, p = .10$ , the shared disclosure group had significantly reduced symptoms compared with the control group,  $F(1, 103) = 4.31, p = .004$  (small to medium effect). In contrast, private disclosure was very similar to the control group in physical symptoms,  $F(1, 99) = 0.10, p = .76$ . The results were unchanged for the randomized sample.

## Discussion

We accomplished four goals in this study. First, we replicated a number of previous studies showing that writing about stressful experiences for several days (and submitting these writings to a research team) leads to a reduction in unresolved stress (cognitive intrusions and avoidance) and improved psychological and physical symptoms. These effects were of moderate size, which is consistent with prior studies in student populations (Smyth, 1998). Second, we tested whether the common practice of sharing one's written emotional disclosures with the research team influences the magnitude of the benefit observed. We found that both shared and truly private disclosure writing led to improvements in intrusions and avoidance. Yet, the benefits of shared writing were broader and stronger—only shared writing led to benefits on psychological and physical symptoms compared with controls, and shared writing led to less depression and marginally less interpersonal sensitivity than did private writing. Third, we demonstrated that the widely-used control condition of time management writing is as credible a technique for reducing stress among college students as is written disclosure. Fourth, by including a no-writing control condition, we demonstrated that time management writing has neither adverse nor positive effects over several months.

### The Effect of Social Sharing of Disclosure

The fact that many controlled studies have demonstrated that writing about stress can improve health in the absence of feedback, therapy, or social support from another person has been a challenge to those who believe that interpersonal help usually is needed for people with stress disorders or health problems. Yet, one cannot conclude from this growing literature on written disclosure that having people write about stress outside of the research context leads to the same benefits. We tested the hypothesis that one aspect of the research setting—knowing that a research team member will receive one's disclosures—appears to contribute to the effects.

This hypothesis was supported in part. Although some benefits occurred even with the knowledge that no one would read the writings, broader benefits occurred when writing was done knowing that a researcher would receive it. Both shared disclosure and private disclosure reduced intrusions and avoidance compared to controls, and the effects of shared or private disclosure on these variables were about the same. However, only shared disclosure led to reduced depression, interpersonal sensitivity, and physical symptoms, whereas private disclosure did not. More impressively, shared disclosure was significantly better than private disclosure in reducing depression, and showed a similar trend for interpersonal sensitivity. These findings suggest that whereas private writing has some benefits, writing that is shared with the research team has more benefits. These findings also suggest that multiple processes may contribute to the effects of disclosure. It is possible that one mechanism, such as emotional exposure and desensitization, reduces intrusions and avoidance independent of the social context. A second mechanism, however, may involve social or interpersonal factors and lead to reduced emotional and physical problems. In addition, the results suggest that a possible contributing factor to the null findings of the disclosure studies of Broderick et al. (2004) and Ames et al. (2005) is that they did not have writers share their disclosures with the researchers.

Yet, key questions remain. First, what specifically is occurring when one writes with an audience in mind, or writes knowing that no one will read one's writing? How do these different conditions lead to different outcome? One possibility is that the content of the writing differed under these two conditions. For example, perhaps writers worked harder at the disclosure task

knowing that someone would evaluate it, and the increased effort led to better outcomes. For example, perhaps shared disclosure writers remained on the same stressor, processed it more fully, attempted to find meaning in it, or resolved to make changes in their lives because they knew that someone would read their writings; that is, the increased accountability of having an audience improves writing content. Perhaps writers revealed important secrets when they knew that a researcher would learn these secrets, but failed to write about these secrets when they knew that the only audience was themselves—someone who already knew the secret. An alternative explanation is that private writers may have chosen to address more difficult, private stressors and conflicts than did those writers who shared, and perhaps these stressors were not fully processed or resolved, leading to reduced benefits. Unfortunately, we cannot answer these questions. Although we visually verified that private writing occurred, we did not have access to the private writings. Perhaps there could have been some clever procedure that would have allowed us access to the private writings, but we did not want to chance contaminating the validity of our experimental manipulation and participants' beliefs that their writings were truly private. Future research, however, should attempt to clarify differences in process between shared and private disclosure writing.

Why did private disclosure writing influence cognitive intrusions and avoidance but not psychological and physical symptoms, whereas shared writing affected both sets of measures? One possibility is that these two types of measures are influenced sequentially and by different processes. Cognitive intrusions and attempts at mental avoidance may be indicators of unresolved stress and signs that one is engaging in ongoing emotional inhibition (Horowitz, 1986). Theoretically, volitional and repeated confrontation of traumatic memories and emotions reverses intrusions and avoidance. Thus, either private or shared disclosure would lead to improvement in this domain, given that both types of disclosure promote such confrontation. In contrast, improvement in emotional and physical health may follow decreased intrusions and avoidance, but only after additional cognitive, emotional, or interpersonal changes occur. Such changes may have occurred for the shared disclosure group but not the private disclosure group for reasons noted in the prior paragraph. Clearly this speculation needs testing.

Our results suggest that truly private writing may lead to reduced intrusions and avoidance, but not necessarily to reduced depression, interpersonal sensitivity, and physical symptoms. Do these results, therefore, mean that expressive writers or journal keepers should share their writings with another person rather than keep them private? Our answer is a qualified yes, with two caveats. First, it is important to recognize the type of sharing that we studied and to which our conclusion is limited—a formal context in which the recipients are researchers who do not know the participants and who have little or no future interaction with them. Such a relationship is more akin to that of a fellow passenger on a flight or a stranger in an internet chat room than a therapist, clergy, or family member. Indeed, we suspect that disclosure to a person who has an ongoing relationship with the writer is a very different situation, and the outcomes of such disclosure are likely to be quite variable and dependent on a host of relationship, personality, and stressor-specific factors, as suggested by Zech, Rimé, and Nils (2004). The importance yet unpredictability of significant others' responses might account for the failure of Schilte et al. (2001) to find benefits following patients' disclosures about stress to their own health care providers. Second, our design assigned people to conditions and enforced either sharing the writing or keeping it private. It is possible that the more common process of choosing whether to write privately and retaining the option of sharing it with someone in the future has effects that are as or more powerful than we found in this study.

## The Neutrality of Control Writing

We demonstrated that the rationale for time management writing created expectations for stress improvement that were equivalent to those for disclosure writing. More importantly, we tested the effects of this time management condition by comparing it with a no-writing condition. Like two other studies that have included a no-writing control (Broderick et al., 2005; Gallant & Lafreniere, 2003), we found that time management writing and no writing were not different in their effects. Our effect size comparisons indicated that the two groups were essentially equivalent. Together, these studies suggest that the time management control writing condition does not cause negative reactions. Additionally, there is no lasting “placebo effect” of time management writing—that is, no positive benefit from expectation or participation. However, it should be noted that our outcomes were assessed nearly 3 months post-writing, and any placebo effects from time management could be short-lived. Indeed, a study of written disclosure among people with fibromyalgia found that time management writing led to significant decreases in negative mood and increases in perceived social support at 1-month follow-up, but that these “placebo” effects did not last until 3 months (Gillis, Lumley, Mosley-Williams, Leisen, & Roehrs, in press). Thus, time management writing may lead to short-term positive effects. If studies continue to show that time management writing has no lasting effects relative to no-writing, then a writing control group may not be needed in future studies, which would obviate the challenges surrounding the selection and implementation of a good placebo writing control condition. In the meanwhile, we recommend avoiding “trivial” control writing conditions, which might create a negative reaction, and we recommend including measures of condition credibility to assess how a control condition compares with an active condition.

## Limitations and Future Directions

This study has several limitations. Conclusions about the effects of private versus shared writing are limited to undergraduates reporting unresolved stressors. We do not know how clinical populations might respond to this manipulation, and it is possible that there is a selection by manipulation interaction. That is, the apparent advantage of shared over private written emotional disclosure may be limited to those participants who are willing and motivated to publicly report unresolved stress at screening. There probably are people with unresolved stress who would not report it and, hence, would not be included in a study such as ours, and it is possible that such people would show different effects, such as stronger benefits from private than submitted writing.

We also do not know the stressors that our participants experienced. Of course, we had the writings of the shared disclosure group, and a review of these writings revealed that the majority (42%) were about intimate relationship problems (e.g., past or potential break-ups caused by experiences such as infidelity, abuse, rape, pregnancy, and religious differences), 29% were about family conflict (including divorce, parental mental health problems, communication difficulties, etc.), 13% were about the death of a loved one, 16% about other topics (e.g., health problems, legal problems, racism). However, we do not have the private disclosure writing, and we chose not to ask these or the control participants to report their stressor to us out of concern of contaminating these conditions by, in essence, generating a shared disclosure condition.

In addition, full blinding did not occur because the research assistants learned the participants' group assignments to provide participants with correct instructions and verify the manipulation. However, assistants were blind to group assignment during baseline assessment, their interactions with the participants were minimized during the instructions and writing phase, and the assistant at follow-up often was different from the assistant who started the participant. Another limitation is our reliance on self-report outcomes. The inclusion of endocrine or immune measures, behavioral measures (e.g., clinic visits, school attendance and

performance), or collateral reports of functioning would be illuminating and add validity to the findings. We also recommend using the revised version of the IES (Weiss & Marmar, 1997), which contains a hyperarousal scale. Finally, it should be acknowledged that our decision to have half of the writing done at home and half in the lab was made out of feasibility considerations, but it means that we have not exactly replicated the paradigm in which all writing is done in the lab.

A key direction for future research is to understand variations in the social context of disclosure. Research should examine both the explicit and implicit audiences that disclosure writers have in mind (Brody & Park, 2004) and how such audiences affect the process, content, and outcomes of disclosure writing. Research might also investigate the effects of manipulating the intended audience, such as occurs when disclosure letters are addressed to someone even if not sent (Kowalski & Cantrell, 2002). With the exception of a handful of studies (Murray, Lamnin, & Carver, 1989; Pennebaker et al., 1987; Segal & Murray, 1994), we know almost nothing about how disclosure that is private, or conducted alone but later shared with a researcher, or shared live with researcher, or shared with a significant other will affect processes and outcomes. Similarly, studies might examine the effects not only of a researcher receiving the writing, but of providing feedback to the writer. In general, we think that the time is ripe to understand how emotional disclosure fits into the larger context of interpersonal relationships.

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**Table 1**  
Comparison of Placebo Writing Control and No-Writing Control Conditions on Baseline and Outcome Measures

Measure	Placebo Writing ( <i>n</i> = 37)		No-Writing ( <i>n</i> = 34)		partial $\eta^2$	Cohen's <i>d</i>
Intrusion	Baseline M (SD)	15.76 (5.56)	15.44 (5.31)			
	Follow-up M (SD)	12.62 (6.10)	11.26 (5.65)			-.232
	Adjusted M (SE)	12.08 (0.69)	11.85 (0.72)		.001	
Avoidance	Baseline M (SD)	13.65 (4.33)	12.82 (4.80)			
	Follow-up M (SD)	12.62 (4.96)	11.65 (4.77)			-.031
	Adjusted M (SE)	12.25 (0.72)	12.06 (0.76)		.000	
Depression	Baseline M (SD)	1.28 (1.02)	0.71 (0.50)			
	Follow-up M (SD)	1.18 (1.19)	0.75 (0.80)			.196
	Adjusted M (SE)	0.93 (0.13)	1.02 (0.13)		.003	
Interpersonal sensitivity	Baseline M (SD)	1.46 (1.21)	0.94 (0.65)			
	Follow-up M (SD)	1.28 (1.23)	0.96 (0.83)			.251
	Adjusted M (SE)	1.09 (0.13)	1.16 (0.14)		.002	
Physical symptoms	Baseline M (SD)	28.51 (19.22)	20.68 (11.68)			
	Follow-up M (SD)	29.97 (17.97)	23.97 (16.54)			.138
	Adjusted M (SE)	26.94 (2.21)	27.27 (2.31)		.000	

Note: Data presented is for the completer sample. Baseline and follow-up mean and standard deviations are original (raw) data. Adjusted means and standard errors are follow-up values adjusted for baseline values and time until follow-up. Cohen's *d* was calculated as the difference between the two groups on the change score (follow-up minus baseline) divided by the pooled SD of change scores. Positive values of *d* indicate that placebo writing had better outcomes than no-writing control, whereas negative values indicate the opposite.



**Table 2**  
Comparisons of Shared Disclosure, Private Disclosure, and Combined Controls

Measure	Shared (S) (n = 36)	Private (P) (n = 32)	Controls (C) (n = 71)	Pairwise Contrasts	partial $\eta^2$	Cohen's d
Intrusion	Baseline M (SD)	16.84 (3.43)	17.50 (3.84)	S = P	.014	-.265
	Follow-up M (SD)	11.25 (5.94)	10.38 (6.44)	S < C <sup>†</sup>	.027	.390
	Adjusted M (SE)	10.88 (0.83)	9.56 (0.89)	P < C	.079	.663
Avoidance	Baseline M (SD)	13.70 (3.62)	13.15 (3.38)	S = P	.005	.189
	Follow-up M (SD)	9.69 (5.24)	10.16 (4.89)	S < C	.073	.557
	Adjusted M (SE)	9.52 (0.77)	10.26 (0.81)	P < C	.042	.383
Depression	Baseline M (SD)	1.13 (0.96)	1.34 (1.22)	S < P	.104	.416
	Follow-up M (SD)	0.48 (0.53)	1.04 (1.15)	S < C	.147	.854
	Adjusted M (SE)	0.47 (0.12)	0.88 (0.13)	P = C	.015	.329
Interpersonal sensitivity	Baseline M (SD)	1.29 (1.07)	1.52 (0.89)	S < P <sup>‡</sup>	.050	.242
	Follow-up M (SD)	0.72 (0.75)	1.13 (0.96)	S < C	.087	.613
	Adjusted M (SE)	0.72 (0.12)	0.92 (0.13)	P = C	.020	.418
Physical symptoms	Baseline M (SD)	25.07 (16.81)	27.03 (16.24)	S = P	.040	.356
	Follow-up M (SD)	21.72 (17.36)	29.78 (22.85)	S < C	.040	.399
	Adjusted M (SE)	21.97 (0.24)	28.57 (2.59)	P = C	.001	-.026

Note: S = shared disclosure; P = private disclosure; C = combined controls (time management and no-writing control groups combined). Baseline and follow-up mean and standard deviations are original (raw) data. Adjusted means and standard errors are follow-up values adjusted for baseline values and time until follow-up for the 3-group comparison. The Contrasts column shows the results of the three post-hoc pair-wise comparisons among the 3 groups for each measure; the contrasts are not aligned with a given row but are presented together for each measure

<sup>†</sup> indicates marginally significant ( $p < .10$ ) contrasts; otherwise differences are significant at  $p < .05$ . Effect sizes are next to each of the contrasts. Positive values of d indicate that the first group listed in the contrast had a better outcome than the second group in the contrast, whereas negative values of d indicate the opposite.