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Experimental Disclosure and Its Moderators: A Meta-Analysis

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Disclosing information, thoughts, and feelings about personal and meaningful topics (experimental disclosure) is purported to have various health and psychological consequences (e.g., J. W. Pennebaker, 1993). Although the results of 2 small meta-analyses (P. G. Frisina, J. C. Borod, & S. J. Lepore, 2004; J. M. Smyth, 1998) suggest that experimental disclosure has a positive and significant effect, both used a fixed effects approach, limiting generalizability. Also, a plethora of studies on experimental disclosure have been completed that were not included in the previous analyses. One hundred forty-six randomized studies of experimental disclosure were collected and included in the present meta-analysis. Results of random effects analyses indicate that experimental disclosure is effective, with a positive and significant average *r*-effect size of .075. In addition, a number of moderators were identified.

Keywords: expressive writing, emotional disclosure, meta-analysis, Pennebaker, intervention

When a person experiences an important life event, the tendency to disclose information about that event has long been considered both normal (Jourard, 1971) and healthy (Alexander, 1950). From negative events, such as the loss of a loved one or interpersonal conflicts, to positive events, such as graduation from college or marriage, most people tend to share details of their emotional experiences with others within days or even hours of the event (Rimé, 1995). It is believed that disclosing information may allow people to free their mind of unwanted thoughts, help them to make sense of upsetting events, teach them to better regulate their emotions, habituate them to negative emotions, and improve their connections with their social world, all of which can lead to beneficial effects on health and well-being.

Although disclosure is a naturally occurring process that can be examined in observational and correlational designs, one can truly learn about the consequences of disclosure by manipulating this process using an experimental design. Such an experimental manipulation was first conducted by Pennebaker and Beall (1986), who randomly assigned participants to write either about traumatic events or about neutral topics for several consecutive days. In this first study, Pennebaker and Beall (1986) assigned participants to one of four writing groups: a trauma-fact group, in which participants wrote only about the facts surrounding their trauma; a trauma-emotion group, in which participants wrote only about the emotions surrounding their trauma; a trauma-combo group, in which participants wrote about both the facts and emotions surrounding their trauma; and a control group, in which participants wrote in a nonemotional fashion about some neutral event (e.g., their plans for the day). The results of this study revealed that, several weeks after writing, the trauma-combo group (but not any of the other three) demonstrated a reduction in illness-related doctor's visits. This fascinating finding that disclosing one's thoughts and feelings concerning a traumatic event can lead to objectively measured health improvements was the beginning of a long tradition of research examining a wide range of effects elicited by experimentally induced disclosure.

A Review of Past Research

Early research on experimental disclosure was conducted mainly with healthy college students (or university employees) and either asked them to disclose their most stressful or traumatic experiences or asked them to discuss their (presumably stressful) experience of having recently started a new life at college. The studies used a relatively uniform paradigm that consisted of having participants come to the lab for three to five sessions of 15–20 min each, during which participants were randomly assigned to either write expressively about an upsetting topic or write without emotion about a neutral topic. Researchers measured participants on a variety of health and well-being variables both before randomization and again several days or weeks (or sometimes months) after the disclosure sessions to assess long-term effects of writing (or, in some cases, talking). Some of the more striking benefits of disclosure that were found included improvements in immune functioning (Pennebaker, Kiecolt-Glaser, & Glaser, 1988), a reduction in health center visits (Pennebaker, Colder, & Sharp, 1990), reduced absenteeism rates from work (Francis & Pennebaker, 1992), improved grade point average (Pennebaker & Francis, 1996), and decreased self-reported upper respiratory problems (Greenberg, Wortman, & Stone, 1996).

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After almost a decade of research on participants from a university sample, the effects of disclosure were examined in more varied samples, typically samples of people who were currently experiencing or had previously experienced an upsetting event. These field studies in the community revealed that experimental disclosure could help nonstudents as well. Interesting benefits of disclosure found in these studies included helping unemployed engineers find jobs faster (Spera, Buhrfeind, & Pennebaker, 1994), helping female caregivers reduce posttraumatic stress symptoms (Campbell, 2003), and helping incarcerated men take fewer trips to the infirmary (Richards, Beal, Seagal, & Pennebaker, 2000). It should be noted, however, that not all attempts to demonstrate the beneficial effects of disclosure were successful among nonstudent populations. For example, widowed community members who were assigned to write expressively about their loss did not show any improvement over controls on physical or psychological health outcomes (Stroebe, Stroebe, Schut, Zech, & van den Bout, 2002).

Not long after distressed community members began to be used in studies of experimental disclosure, this paradigm was extended to include testing on people with medical ailments, including those with rheumatoid arthritis, asthma, and migraine headaches. The first published study of this type was conducted by Kelley, Lumley, and Leisen (1997), who examined the effects of experimental disclosure on arthritis-related problems in rheumatoid arthritis patients. Patients who wrote expressively about traumas (compared with a nonwriting control) reported less physical and affective dysfunction in the weeks following writing. However, there were no group differences for arthritis-related pain or objectively measured joint condition. An even more recent study found that experimental disclosure was helpful for both rheumatoid arthritis and asthma patients (Smyth, Stone, Hurewitz, & Kaell, 1999), although some researchers have had trouble replicating the asthma finding (Harris, Thoresen, Humphreys, & Faul, 2005). Other recent findings with medical patients include a reduction in cancer-related doctor visits for breast cancer patients assigned to an experimental disclosure group (Stanton et al., 2002), a reduction in distress for migraine headache suffers who wrote expressively (McKenna, 1997), and a reduction in depressive symptoms for community members with Type I diabetes who disclosed thoughts and feelings about their illness (Bodor, 2002).

The most recent sample of participants with whom experimental disclosure has begun to be tested is participants with psychiatric and psychological problems. Although only a handful of disclosure experiments have been conducted with these samples, the results thus far have been quite mixed. Some studies have found support for this intervention using samples of participants who suffered from psychological problems, such as Russ (1992), who found that disclosure improved psychological and physical health for college students with a history of anxiety. By contrast, other studies have found that disclosure may actually be harmful for certain clinical samples, such as Gidron et al. (2002), who found that disclosure increased illness-related doctor's visits in a small sample of men receiving treatment for posttraumatic stress disorder. In addition, a few studies have found null effects for disclosure, including studies using former psychological patients (Bird, 1992), participants with negative body image (Earnhardt, Martz, Ballard, & Curtin, 2002), and those with suicidal tendencies (Kovac & Range, 2002).

Theories of Experimental Disclosure

It seems appropriate to begin this section with a quote from King (2002), from a chapter in which she attempted to explain some of the mechanisms behind the benefits of experimental disclosure. King wrote, "Two strong conclusions can be made with regard to the benefits of writing. First, expressive writing has health benefits. Second, no one really knows why" (p. 119).

Inhibition Theory

Early explanations of the benefits of experimental disclosure draw from a Freudian explanation of the benefits of catharsis, suggesting that the inhibition of thoughts and feelings regarding an upsetting event is harmful and that, consequently, expression of those inhibited thoughts and feelings can reduce stress and improve a host of physical and psychological health outcomes. In fact, the most recent edited book about experimental disclosure (Lepore & Smyth, 2002) is titled *The Writing Cure,* making a clear (and clever) connection to Freud's (1904/1954) "talking cure." Early tests of experimental disclosure encouraged participants to write about things they had not discussed with others or things they felt guilty about, concurrent with the notion that benefits come from talking of events that are threatening to people, memories that are at least partially hidden in their unconscious. This idea is further echoed in instructions to participants that ask them to really "let go" and "not worry about spelling, punctuation, and grammar," as if the written session was designed to be a type of free association exercise.

As more studies were completed, however, evidence began to emerge that this psychoanalytic–inhibition explanation might not be fully sufficient. First, Francis and Pennebaker (1992) found that participants who were low in dispositional constraint benefited most from an experimental disclosure intervention, whereas those who were high in dispositional constraint benefited less. (If disinhibition is the key to success, one would have expected that those who habitually hold back—those high in constraint—would be in most need of help and would therefore benefit more). Furthermore, researchers were obtaining mixed results regarding the need for a writing topic to be previously undisclosed—some studies found that the benefit was stronger when participants reported writing about previously undisclosed topics, whereas others found no such benefit. To address this issue experimentally, Greenberg and Stone (1992) manipulated previous disclosure by assigning some participants to write about previously disclosed traumas, some participants to write about previously undisclosed traumas, and some participants to write unemotionally about a neutral topic (control). This study found no benefit for writing about previously undisclosed (vs. previously disclosed) traumas. Greenberg et al. (1996) followed up this study with an even more challenging concept: They assigned some participants to write about a trauma that had happened to them, assigned some participants to write about an imaginary trauma (a trauma—which they had not experienced—was briefly described to them, and they were asked to write as if the trauma had been their own), and assigned some to a nonemotional writing control. It was surprising that both real trauma participants and imaginary trauma participants demonstrated a reduction in illness-related doctor's visit over control participants. Participants benefited from writing about made-up emotions sur-

rounding a trauma that had not even happened to them. The idea that writing was helpful solely through a mechanism of letting go of unresolved emotions was being challenged with each result.

Cognitive-Processing Theory

To investigate different avenues of explanation, Pennebaker et al. (1990) asked participants who had reported finding benefit in the expressive writing process to explain why they thought it was beneficial. An overwhelming majority of respondents did not talk of catharsis or a letting-go mechanism; instead, most participants reported that the writing was helpful because it allowed them to gain insight into what had happened to them. To examine this insight idea further, Pennebaker (1993) pooled the results of his first five experimental disclosure studies and developed a computerized text-analysis program to examine the words used during writing exercises. In line with the qualitative data provided by his own participants 3 years earlier, Pennebaker found that participants who had benefited most from experimental disclosure in previous studies demonstrated an increase in the use of causation words (e.g., *because, cause, effect*) and insight words (e.g., *consider, know*) during the course of their writing session. In comparison, those who did not benefit from the experimental disclosure did not show an increase in these types of words. Pennebaker (1993) concluded from these results that the act of making sense of an event, of gaining insight about a trauma, and of organizing and integrating an upsetting experience into one's self-schema is the mechanism by which expressive writing is helpful. Although the Freudian idea of disinhibition–catharsis may be necessary (indeed, later studies found that a moderate use of negative emotion words and a high use of positive emotion words was related to benefit), it is not sufficient. One must also make sense of, organize, and integrate this event for benefits to occur.

Self-Regulation Theory

Although cognitive-processing theory certainly explains the findings of many earlier studies, including the first disclosure study, in which only the group assigned to write about both the facts and the emotions of their traumas benefited from writing (Pennebaker & Beall, 1986), it does not offer a clear explanation for the imaginary trauma study (Greenberg et al., 1996) or other, more recent studies that used a slightly different disclosure approach but still found benefit. For example, King and Miner (2000) recently found that writing about the benefits of a traumatic event was just as beneficial in reducing illness-related doctor's visits as the more traditional disclosure paradigm. Cameron and Nicholls (1998) demonstrated that a self-regulation writing exercise in which students described problems they encountered in college and came up with ways to fix the problems produced the same health benefits as typical expressive writing. King (2001) reported that writing about one's "best possible self" (writing about one's life as if all one's goals were met and everything went right) produced reductions in illness visits that were as strong as (if not stronger than) those produced from writing expressively about a trauma. Writing about the best possible self even improved psychological well-being (e.g., optimism), whereas the traditional expressive writing did not.

This more recent flurry of disclosure tasks that produce the benefits of typical experimental disclosure without eliciting all the short-term negative affect that trauma writing often produces is currently being explained in the context of a self-regulation theory of expressive writing. Lepore, Greenberg, Bruno, and Smyth (2002) explained that experimental disclosure (in the traditional sense or in the recent, more positive variations) can be thought of as a mastery experience. It allows people to observe themselves expressing and controlling their emotions. This may give people a new or stronger sense of self-efficacy for emotional regulation. They may feel that their traumas, stressors, or challenges are more controllable, which should serve to reduce negative affect and lead to other well-being improvements.

King (2002) hypothesized that any task that serves to elicit the process of self-regulation should be helpful for the writer. Focusing more on an explanation of the benefits of the more traditional experimental disclosure task (disclosing traumas), King explained that traumatic experiences can be seen as disrupting the normal self-regulation process. She defined self-regulation in terms of goal attainment—people experience emotion as a result of the status of their goals, as a feedback system that tells them whether they are on the right track or are straying away from the path that will lead them to goal attainment. The well-regulated individual experiences emotions that clearly inform him or her regarding the status of his or her goals. When a trauma occurs, it "might muddy the waters of affective feedback" (p. 120). Experimental disclosure tasks allow the participant to make sense of the event, explore sources of emotion, clarify goals, and get the self-regulation feedback system back on track.

Other Theories

Although the disinhibition, cognitive-processing, and self-regulation theories seem to have been written about the most widely, there are a few other theories of experimental disclosure that have recently received some attention, including the social integration model (Pennebaker & Graybeal, 2001) and the exposure model (Bootzin, 1997). The social integration model argues that experimental disclosure affects the way people interact with their social world, which, in turn, improves their health and well-being. Evidence for this model comes from studies that have found that participants assigned to experimental disclosure were more likely than controls to talk about their traumatic experience in the weeks or months following disclosure (e.g., Kovac & Range, 2000) and were more likely to report having received socially supportive behaviors from friends and family (Heffner, 2002). In addition, recent pilot studies have found that treatment participants make small changes in their friendship networks and even laugh more than control participants in the days and weeks following disclosure (Pennebaker & Graybeal, 2001). This theory, however, is very new and has therefore only been tested in a handful of studies. Furthermore, some studies finding evidence of social changes following disclosure either have failed to find other benefits (e.g., physical health changes) or have not examined other outcomes simultaneously.

The exposure theory of experimental disclosure argues that the expression of thoughts and feelings regarding an upsetting event is akin to exposure (or flooding) therapy, which is used to treat phobias and posttraumatic stress disorder. When a person repeat-

edly confronts, describes, and, in essence, relives the thoughts and feelings about his or her negative experience (as researchers have accomplished by having participants disclose their event over and over throughout several days), this repetition and exposure should eventually lead to extinction of those thoughts and feelings. Support for this theory has been mixed, with some studies finding that disclosure does reduce intrusive and avoidant thoughts about the event (e.g., Klein & Boals, 2001) and others failing to find such a reduction (e.g., de Moor et al., 2002; Lepore, 1997). For a more detailed discussion of the experimental disclosure theories discussed here, see Lepore and Smyth (2002), Slatcher and Pennebaker (2004), and Sloan and Marx (2004b).

A True Effect?

Although experimental disclosure is generally accepted to be a beneficial activity, some researchers have begun to call into question its utility in light of failures to replicate the effect. Article titles such as "A Writing Intervention for Negative Body Image: Pennebaker Fails to Surpass the Placebo" (Earnhardt et al., 2002) and comments such as "[We have] reduced confidence in the ability of written expression to benefit the disease status of patients" (Harris et al., 2005, p. 134) demonstrate the skepticism that currently exists in the field. People are questioning, "Does experimental disclosure really work? If so, how well does it work?"

The best way to address the two previous questions, "Does it work?" and "How well does it work?" is by the use of meta-analysis (R. Rosenthal & DiMatteo, 2002). Meta-analysis, also known as a research synthesis or a quantitative literature review, is a technique in which a researcher reviews the results sections of completed papers in an area and, from them, extracts an effect size (a measure of how well a treatment works or of how strong a relation is between two variables). These effect sizes can then be averaged and subjected to null-hypothesis testing to determine, on the basis of the current available literature, how big the effect is (and whether the effect significantly differs from zero).

With regard to experimental disclosure, two meta-analyses have been published in an effort to answer the two questions posed. In 1998, Smyth meta-analyzed 13 experimental disclosure studies, and in 2004, Frisina, Borod, and Lepore meta-analyzed 9 studies (1 of which had been included in the Smyth analysis). Both articles concluded that, indeed, experimental disclosure does work, with significant and positive average effect sizes of .230 (Smyth, 1998) and .101 (Frisina et al., 2004). Although the analyses of these 21 studies do offer support for the conclusion that experimental disclosure does indeed work, the number of experimental disclosure studies has now reached close to 200. Therefore, it is important to examine how the addition of these new studies affects Symth's (1998) and Frisina et al.'s (2004) findings. It is especially of interest to reexamine these questions in light of the larger body of studies given that many of the studies not included in the previous meta-analyses have demonstrated a number of changes in methodology, including asking participants to write only about the loss of a loved one, having participants write about the positive aspects of their traumatic experiences, and instructing participants to write about their future goals. If one conceives of a more inclusive definition of experimental disclosure, does the process still work? If so, how well?

Another reason that another meta-analysis would be prudent at this time is that the two previous meta-analyses, because of the small number of studies involved in each, were limited in the scope of their generalizability. When answering the question "Does it work?" in a meta-analysis, one can examine the degree to which the average effect size is significantly different from zero. Inferential testing to determine the significance of the overall mean effect size can be conducted in one of two ways: a fixed effects approach, or a random effects approach (Hedges, 1994; Raudenbush, 1994). In a fixed effects approach, the participants in each study are considered to be the unit of analysis. This approach, which was used by both Smyth (1998) and Frisina et al. (2004), is typically used when a relatively small number of studies is to be analyzed. It has the advantage of being a more powerful (less conservative) test of significance but carries the disadvantage of limiting the generalizability of the findings; in a fixed effects approach, one can only generalize to similar participants in the exact studies that are included in the analysis but can say nothing about studies not included in the analysis or future studies. By contrast, a random effects approach is typically used when a relatively large number of studies is to be analyzed. In this approach, the study itself is considered to be the unit of analysis; each study's effect size is a single data point. A random effects approach has the advantage of having a more powerful scope of generalizability; one can generalize to similar studies not included in the analysis or even to future studies. It does carry the disadvantage of being a less powerful test of significance (more conservative), but this is less of a concern when the number of studies being analyzed is sufficiently large.[1] Another meta-analysis of the effects of experimental disclosure that included the current, larger body of studies would be able to use a random effects approach; this is the primary goal of the present study.

When Does It Work, and for Whom?

Even though it is likely that experimental disclosure can have beneficial effects, this activity is not helpful for all people in all

[1] Unfortunately, there is no known cutoff point for what is considered a small versus a large number of studies in regard to the decision to use a fixed versus a random approach (R. Rosenthal, personal communication, November 2005). However, a brief examination of the meta-analyses published in *Psychological Bulletin* over the past year (from September 2004 to September 2005) can give some insight into the typical number of studies found in meta-analyses: Of the 19 meta-analyses published in the past year, the median number of studies included in each analysis was 66.5, with an interquartile range of 35 to 143. Even without hard and fast guidelines of what constitutes a small and large number of studies, the Smyth (1998) meta-analysis (with 13 studies) and the Frisina et al. (2004) meta-analysis (with 9 studies) can safely be considered small in light of recent meta-analyses conducted in this journal. Because there are important advantages and limitations associated with both approaches, some meta-analysts recommend presenting the results of both approaches (R. Rosenthal & DiMatteo, 2002), a suggestion that is used in the present study.

situations. For example, Pennebaker (1993) reported that when he ranked his study participants in order of who improved the least to who improved the most as a result of experimental disclosure, participants scoring in the bottom third of this list did not differ from control participants on outcome measures. Other researchers have also found that, although most people react positively to experimental disclosure, some participants do report that they find the task to be unenjoyable, unhelpful, and even objectionable (Frattaroli, 2001). Methodological differences between studies and individual differences between participants may contribute to differences in the effectiveness of experimental disclosure. For instance, a study with arthritis patients found experimental disclosure to be more effective in reducing disease activity for participants who were given instructions specifically designed to promote cognitive change and insight compared with those who were given more general instructions (Broderick, Stone, Smyth, & Kaell, 2004). In addition, a study with college students demonstrated that optimists significantly reduced illness-related doctor's visits after experimental disclosure, whereas pessimists showed no change (Cameron & Nicholls, 1998). Pennebaker (2004) recently argued that the most important agenda for researchers in the field of experimental disclosure is "to find out when [experimental disclosure] does and does not work and with whom" (p. 141).

In addition to answering the questions of "Does it work?" and "How well does it work?" the technique of meta-analysis can also be used to answer the questions "When does it work?" and "For whom does it work?" Because studies naturally tend to vary on aspects of setting (e.g., inclusion criteria), participant type (e.g., students vs. nonstudents), methodology (e.g., participant payment), and details of the treatment itself (e.g., length of treatment sessions), one can use meta-analysis to test for the relation of these variations to the size of the effect. For example, researchers can compare the average effect size of studies that have participants disclose at home with the average effect size of studies that have participants disclose in a lab to test whether location of disclosure sessions is a moderator of the effect of experimental disclosure.

The Smyth (1998) meta-analysis and the Frisina et al. (2004) meta-analysis examined a handful of moderators. Of note, it was determined that studies with college students had significantly larger psychological health effect sizes than studies with nonstudents, studies with more men had significantly larger effect sizes than studies with fewer men, studies in which participants were assigned to write about current traumas had significantly larger psychological health effect sizes than studies in which participants were assigned to write about their choice of current or past traumas, studies in which participants were assigned to write about their choice of current or past traumas had significantly larger effect sizes than studies in which participants were assigned to write about past traumas, and studies with weekly disclosure sessions (sessions spaced 1 week apart) had significantly larger effect sizes than studies with daily disclosure sessions (sessions on consecutive days). In addition, it was found that experimental disclosure did not affect health behaviors (e.g., exercise and eating habits) and that, among medical patients, experimental disclosure was more helpful for physical health outcomes (e.g., immune function) than for psychological health outcomes (e.g., depression).

When examining moderators meta-analytically, one is again faced with the decision to use statistical tests that are based on a fixed effects versus a random effects approach. As discussed earlier, the fixed effects approach is useful and appropriate with a small number of studies, but it is limited in its generalizability. Given the many studies currently available on the topic of experimental disclosure, it would be useful to replicate the findings of the previous analyses, which used a fixed effects approach, with a new analysis that uses a random effects approach. In addition, using a larger number of studies (and using a more inclusive definition of experimental disclosure) would allow for the testing of new moderators that were not previously examined in the first two articles; these are the secondary goals of the present article.

Proposed Moderators

A recent qualitative review of the experimental disclosure literature (Sloan & Marx, 2004b) suggested that the following variables may be important moderators and should be examined in more depth by researchers: the sample used, the disclosure instructions used, the number of writing sessions, the spacing of writing sessions, and the timing of follow-up. These moderators (and several others), along with the hypothesized direction of their relations with effect size, are described in detail in the following sections.

Report Information Variables

When one is examining moderators meta-analytically, it is often worthwhile to investigate properties of the study report itself that may be related to effect size variations, such as publication status and characteristics of the authors. Although these variables are not necessarily thought to directly affect the effect size, examination of these variables can highlight various trends or patterns as they appear in the field. For example, it is typically assumed that, because of publication bias, published studies will have larger effect sizes than unpublished studies (M. L. Smith, 1980); similarly, it has been noted that studies with smaller effect sizes tend to take longer to get published (R. Rosenthal, 1991).

Setting Variables

Another group of characteristics that may vary in meaningful ways among studies is setting variables, such as the use of special populations (Stock, 1994). For example, some studies recruit only participants who have a physical health problem (e.g., Kelley et al., 1997), a psychological health problem (e.g., Kovac & Range, 2002), or a history of traumas or stressors (Greenberg et al., 1996), whereas other studies do not have any inclusion or exclusion criteria (e.g., King & Miner, 2000). Studies that use only participants with physical or psychological health problems may have larger effect sizes; it has been suggested that people who are already healthy may not benefit as much as sick people from experimental disclosure, given that healthy people may not be in need of help or may already have good coping skills (Bootzin, 1997). Similarly, studies that use only participants who have a history of trauma or who are currently going through a stressful situation may have larger effect sizes. Some researchers have noted a distinct subset of their research sample who reported being very lucky to have never experienced any traumas in their lifetime (Frattaroli, 2001). These participants reported not really having

anything to write about and, indeed, often seemed annoyed that they were being asked to write about a trauma over and over again when they had experienced no trauma; those who have never experienced a trauma may not benefit from this type of exercise. Finally, as previously mentioned, Smyth (1998) found that studies that used only college students had larger effect sizes than studies that recruited from a more general population.

The conditions of the disclosure session itself may also be related to the impact of the intervention. Some studies have had a lot of control over the disclosure sessions, with sessions taking place in a psychology laboratory or medical office (e.g., Range, Kovac, & Marion, 2000), whereas other studies have had less control over the setting, with sessions taking place at the participants' home (e.g., Rosenberg et al., 2002). Although home sessions may have higher external validity, they are also more susceptible to compliance problems and allow more room for error. Similarly, there has been variation among studies in the degree of privacy afforded to study participants during their disclosure sessions; some studies have participants disclose in a room alone, offering a great deal of privacy (e.g., Klein & Boals, 2001), whereas other studies have participants disclose in a room with other study participants who are also disclosing, offering less privacy (e.g., Anopchand, 2000). Some researchers believe privacy is a critical component of the disclosure paradigm (K. Klein, personal communication, February 27, 2005); the presence of other people might inhibit participants, in effect watering down the strength of the treatment.

Participant Variables

Individual differences among participants may also account for some of the differences in the effect of experimental disclosure. These differences can take the form of demographic variables (e.g., gender, age, ethnicity, and education), of well-being variables (e.g., stress level, mood, or physical health status), or of personality variables (e.g., neuroticism, alexithymia, optimism, and emotional inhibition).

As previously mentioned, Smyth (1998) found that studies with a higher proportion of men had larger effect sizes than studies with a higher proportion of women; he has proposed that men may benefit more from experimental disclosure because they tend to be less likely than women to naturally disclose information as a result of traditional sex roles. Western culture typically discourages men from the interpersonal expression of emotion; men may benefit more because this paradigm provides them with a context to express themselves that they normally would not have. This logic can also be applied to ethnic groups that tend to have a less expressive culture, such as Asian Americans (Rivkin, 2000), as well as to people who have an emotionally inhibited personality; one might expect Asian Americans and other people who tend to be emotionally inhibited to benefit from experimental disclosure more than Caucasians and those with a more expressive personality.

Individual-differences variables of stress, health, mood, neuroticism, and optimism are somewhat related in the sense that if a person is high on stress, poor in health, negative in mood, high on neuroticism, or low in optimism, he or she is similar to groups of participants described earlier who are specifically selected because of a health or psychological problem in that the person's coping skills may be poor and he or she may be more in need of an intervention than other, healthier people (Bootzin, 1997). Sick, stressed, unhappy, neurotic, negative people are also likely to have experienced more traumatic events (Magnus, Diener, Fujita, & Payot, 1993), and, as discussed earlier, participants with a history of trauma may have more to disclose and, hence, more to gain from participating in this type of intervention.

The personality variable of alexithymia recently has been receiving attention in the disclosure literature as a potential moderator. Alexithymia has been described as "a deficit or lack of ability to process and regulate emotional states through the use of cognitive mechanisms such as introspection, imagination, and fantasy" (Lumley, Tojek, & Macklem, 2002, p. 83). People who are high in alexithymia have trouble introspecting about the causes of their feelings or experiences and tend to be relatively expressionless. It has been argued that those who are high in alexithymia would not benefit as much from experimental disclosure because their inability to understand their own feelings and psychological states would interfere with the task of identifying a stressor to disclose, labeling their emotions, and gaining insight into and understanding about their feelings and about the event that they are disclosing.

Methodological Variables

In most experimental disclosure studies, treatment participants are asked to discuss an upsetting or traumatic event from their life. In some studies, participants are specifically warned in advance (during an introductory meeting or during the scheduling process) that if they participate, they may be asked to write about traumatic events (e.g., Kovac & Range, 2002), whereas in other studies, participants are not forewarned or are warned only in the consent form signed right before disclosure (e.g., Dickerson, Kemeny, Aziz, Kim, & Fahey, 2004). It has been proposed that although warning participants that they might disclose a traumatic event should be helpful for the treatment participants (as they come to the study ready to disclose), it should have a negative effect on the control participants (as they come in expecting to disclose upsetting feelings but are instead forced to inhibit those feelings by being asked to discuss a neutral topic; Cole, 2003). Studies that warn participants may also be problematic because control participants, on receiving neutral instructions (instead of the instructions they were warned about), may become aware that they are in a control group, which would create unequal expectations between the two groups.

Another methodological variation among studies that may have important consequences for the effect size is the timing of the follow-up period. Some studies measure change since disclosure as early as only 1 day after the experimental intervention (Booth, Petrie, & Pennebaker, 1997), whereas other studies measure change since disclosure as late as 15 months after the intervention (Gidron et al., 2002). Because disclosing traumatic events can be a difficult and upsetting process, there are typically short-term costs to experimental disclosure, in that treatment participants have an increase in negative mood and a decrease in positive mood shortly after disclosure (Smyth, 1998). Although it is generally assumed that these negative effects will wear off in an hour or 2 (Pennebaker, 2000), both the Smyth (1998) and Frisina et al. (2004) meta-analyses excluded studies with follow-up periods of less than 1 month because of concerns of the impact of short-term

negative effects. Do the benefits of experimental disclosure take some time to kick in? Do researchers need to wait at least a month to measure salutary effects?

Studies also vary on methodology with regard to the payment of study participants; some researchers pay participants with money or course credit (e.g., Rentfrow & Keough, 1999), whereas others do not pay participants at all (e.g., Wetherall, in press). People who volunteer to participate in psychological studies without any sort of payment tend to have different characteristics than those who refuse to volunteer (R. Rosenthal & Rosnow, 1975); in particular, women are more likely to volunteer, as are people who are more sociable. To the extent to which being a woman or being sociable may moderate the effects of experimental disclosure, this variable may predict variation in study effect sizes.

The final methodological variable that is examined in the present study is the raw number of participants who were included in the study. Studies with a smaller number of participants should have a greater likelihood of sampling error (Shadish & Haddock, 1994), producing results that are farther from the mean of the true effect. However, small studies should be equally as likely to have larger than average effect sizes as they are to have smaller than average effect sizes, especially when an effort is made to include unpublished studies (which are more likely to have the small sample size–small effect size combination) in addition to published studies (which are more likely to have the small sample size–large effect size combination).

Treatment Variables

The final group of variables that may differ in meaningful ways among studies is variables that are specific to the actual implementation of study treatment. The first set of variables in this category contains variables related to the "dose" of treatment offered in each study: how many sessions are scheduled, how long the sessions last, and how the sessions are spaced out. On his Web site, where he gives guidelines to readers who are interested in trying expressive writing for themselves, Pennebaker (2000) recommended that people write for at least 15 min and for at least three sessions, suggesting that writing would not be as helpful if done for less than 15 min and for fewer than three sessions. Very short sessions of writing (or talking) about upsetting events may be unhelpful (or even detrimental) because, in this short time period, participants activate negative thoughts and feelings about the event but are not given time to perform the cognitive work necessary for obtaining benefits from disclosure (Paez, Velasco, & Gonzalez, 1999). Fewer than three sessions may be less helpful because participants may be less likely to obtain the insight and understanding about the event that they are expected to reach across several writing sessions (Walker, Nail, & Croyle, 1999).

As mentioned previously, Smyth (1998) found that studies with longer intervals between disclosure sessions (e.g., weekly sessions) had larger effect sizes than studies with shorter intervals between sessions (e.g., daily sessions). Although this finding was the result of a fixed effects analysis and so cannot be generalized to future studies, some researchers since the Smyth (1998) meta-analysis have purposely arranged weekly sessions in their studies on the expectation of increased benefit from this procedure (e.g., Harris et al., 2005). Spacing out the disclosure sessions may reduce the risk of fatigue on the part of the participant and may provide the extra time that the participant needs to fully integrate, understand, and gain insight about the issue being disclosed, although these suggestions have not been explicitly tested.

The second set of variables related to the actual implementation of study treatment is concerned with the topic or event being disclosed: valence of the event, time since the event, prior disclosure of the event, and topic switching. Standard instructions for experimental disclosure studies ask treatment participants to write about "the most traumatic and upsetting experiences of their entire life" (e.g., Pennebaker et al., 1988) or about specific stressful or upsetting experiences, such as death of a loved one (e.g., Kovac & Range, 2000) or an interpersonal conflict (e.g., Allard, Freyd, & Momiyama, 2004). More recently, however, researchers have begun to examine the effects of disclosing more positive topics, such as one's best possible self or future goals (Vaughn et al., 2003) or intensely positive experiences (Burton, 2005). It has been argued that disclosing positive events may allow for similar opportunities for self-examination and for the formation of a coherent life story as may be involved in disclosing negative events and, therefore, should also have beneficial effects (Burton & King, 2004). However, other researchers have argued that disclosing positive events, especially in written form, may cause the participant to analyze and deconstruct the positive event in such a way that the positive event no longer seems as positive and, therefore, should have detrimental effects (Lyubomirsky, Sousa, & Dickerhoof, 2005).

The events that participants choose to write about (or are assigned to write about) tend to vary widely with respect to how long ago they occurred. Whereas some participants write about current events (e.g., an ongoing illness), other participants write about events from long ago in their childhood (e.g., abuse). The time since the event may be an important factor in the effectiveness of the intervention; a recent study found that disclosing one's thoughts and feelings about an event for which one has already gained a sense of closure *(closed events)* does not have any beneficial effects (Naufel & Beike, 2004). One might expect older events to be more likely to be closed. Participants writing about older events might have already processed and integrated these events into their self-schemas, which would reduce the utility of the experimental disclosure exercise (Fidler, Dittoe, Quartana, & Zakowski, 2004). Also, older events may be judged to be less severe, and severity of the event has been found to positively correlate with obtaining benefit from disclosure (Greenberg & Stone, 1992).

Similar to the reasoning that more recent events are better candidates for disclosure, it has been argued that previously undisclosed events may also be better candidates for disclosure (Paez et al., 1999). One could assume that undisclosed events, like recent events, are also less likely to be closed, precisely because the participant has not had the opportunity (or desire) to translate these events into language, process them fully, and integrate them into his or her life story; these open, undisclosed events should be good candidates for experimental disclosure. Studies have differed in their instructions to participants regarding previous disclosure: Some studies have instructed participants to write about undisclosed (or minimally disclosed) events (e.g., Bodor, 2002), whereas other studies have not included this instruction (e.g., Broderick et al., 2004).

As stated earlier, one of the theories of experimental disclosure purports that this activity is helpful because it allows the partici-

pant to gain insight into and understanding of his or her event over the course of several disclosure sessions. Another topic-related difference among studies is that in some studies, participants are told that they are welcome to switch topics between or within sessions (e.g., Pennebaker et al., 1990), whereas in other studies, participants are asked not to switch topics (e.g., Guastella & Dadds, in press) or are given no instruction regarding topic switching (e.g., Francis & Pennebaker, 1992). The ability to switch topics from session to session—or even to write about a variety of topics in one session—may inhibit the writer's ability to form a complete story, gain insight, and increase his or her understanding of causal factors of any one particular event. If one theorizes that the forming of a story (i.e., increasing the use of insight and causation words from the first writing session to the last) is at least partly responsible for the positive outcomes of writing, allowing participants to switch topics from session to session may reduce the effectiveness of the intervention.

The theory that topic switching should decrease intervention effectiveness is based on the assumption that participants' actual switching behavior logically corresponds to the instructions given: Instructions not to switch should result in the least participant switching, followed by giving no instructions, further followed by instructions that it is okay to switch (which should result in the most switching). However, a study that compared the actual switching behavior of treatment participants under these three conditions found that, although the group assigned not to switch did have significantly less topic switching than the other two groups, the group that was told switching was okay and the no-instructions group were virtually indistinguishable from each other in their level of topic switching (Frattaroli, 2001). This suggests that participants who do not receive instructions regarding topic switching disclose in similar ways to participants who are told it is okay to switch topics. In light of this finding, one might expect that studies that instruct participants not to switch would have larger effect sizes than the other two types of studies (grouping instructions that it is okay to switch and no instructions into one group of studies).

A third consideration regarding topic-switching instructions is the issue that it may be the specificity of the instructions, not the content of the instructions, that has an impact on the effect of the intervention. Whether the instructions say that it is okay to switch or say that it is not okay to switch may not matter as much as the fact that the instructions say something about the issue of topic switching. Because disclosing one's deepest thoughts and feelings about a highly personal event in an experimental context is likely a novel and potentially daunting task, giving specific details about the expectations of the disclosure session or clear rules about what is to be disclosed may put anxious participants at ease. Being free from the worry that they are "not doing it right" that they may have with more vague instructions (instructions that do not mention anything about switching), participants may be more able to become actively engaged in the disclosure process and consequently benefit more from it.

The third set of variables related to the actual implementation of study treatment concerns the wording or administration of the disclosure instructions themselves: the focus of the disclosure instructions, the presence or absence of directed questions or specific examples of what to disclose, and the mode of instruction administration. Traditional instructions given to treatment participants in experimental disclosure studies are relatively general in scope; participants are asked to think of an upsetting or traumatic event (either an event of their choice or an event that was chosen by the experimenter) and to discuss their deepest thoughts and feelings about the topic. Since the popularity of cognitive-processing theories of experimental disclosure (which argue that experimental disclosure is helpful in that it allows participants to understand and gain insight about an event), a few studies have modified disclosure instructions to promote cognitive processing, insight, and understanding throughout the course of the disclosure sessions. For example, as in Gidron et al. (2002), participants might be asked to describe their thoughts and feelings at the time of the event (to enhance cognitive processing and verbal labeling of sensory and affective responses) and to describe whether the event affected their life (to enhance self-reflection; p. 163) on the 2nd day of disclosure. They then might be asked to write how they currently thought and felt about the event (to enhance perspective) and to describe what they would do in the future, should they encounter similar events (to enhance self-regulation; p. 163) on the final day. Because the newer, cognitive-processing instructions were theoretically derived in an attempt to maximize the processes thought to be responsible for the beneficial effects of experimental disclosure, they should be more effective (assuming that the cognitive-processing theory is correct).

As mentioned previously, it is possible that some participants may be somewhat intimidated or overwhelmed by the experimental disclosure process, in that it is a novel activity and they are unsure of precisely what to disclose or exactly what is expected of them. Giving directed questions and examples of appropriate disclosure topics may help alleviate this discomfort and allow the participant to become more fully involved in the task, enhancing its benefit. Studies in the literature have varied in the degree to which disclosure instructions included directed questions and specific examples; some researchers have said little more than, "Write about one of the most traumatic and upsetting experiences of your whole life" (Booth et al., 1997, p. 24), whereas other researchers have given directed questions, such as, "How did you feel then, at the time of the experience? How do you feel now about the experience?" (Barry & Singer, 2001, p. 291) or specific examples of what to write about, such as, "Common examples including writing about a death of a loved one, breakup of a relationship, failure, and so forth" (Kloss & Lisman, 2002, p. 34).

Two additional treatment-related differences among studies that may be important for the impact of the intervention are whether participants believe that their disclosure will be read (or heard) by anyone and whether participants disclose via hand writing, typing, or talking. In many experimental disclosure studies, participants turn in their written essays (or a tape-recorded version of the oral disclosure) to the experimenter at the conclusion of the study. This is done so that the experimenter can content analyze the stories that were written (or spoken). Whether or not all experimenters explain to their participants why they are collecting the essays is unclear, but it is relatively safe to assume that participants are aware that their essays will be read. A number of experimenters have asked participants to turn in their writing samples but have allowed them not to turn the samples in if they so desire (e.g., Levey-Thors, 2000). In these cases, most participants (usually more than 90%) have turned them in.

Pennebaker and Seagal (1999) reported that there were no audience effects in play during experimental disclosure. They argued that the results of expressive writing would be the same whether or not the participants expected that someone would read their essay. However, there are reasons to suspect that the presence or absence of an audience may make a difference. In a recent experimental disclosure study (Frattaroli, 2003), in an effort to make bilingual participants feel more at ease, students were informed that they were welcome to write in any language of their choosing. Although many participants reported preferring to write in a non-English language, all but 2 (out of more than 300) participants wrote in English. Although the notion is somewhat speculative, one might imagine that participants chose to write in English because they wanted the English-speaking experimenter to read what they wrote. Perhaps they felt their experience of disclosure would seem futile if not appreciated by another person, or perhaps they would not be motivated to continue writing if they suspected no one would care about what they actually wrote. Perhaps the presence of an audience increases the effect. Conversely, the choice to write in English might have been due more to demand characteristics of the setting than to participants' desire to share their thoughts with others, as the experiment took place in an American university, where there likely were very strong social pressures to write in English, even in spite of the instructions to write in any language. Indeed, one could also make the opposite prediction—that the presence of an audience decreases the effect—as participants may be concerned with self-presentation during disclosure if they are aware that the disclosure will be read or heard (J. Hall, personal communication, July 20, 2005). Given that inhibition of one's true thoughts and feelings can have negative effects (Pennebaker, 1989), inhibiting for the purposes of self-presentation may reduce or counteract the benefits of disclosure, hence lessening the effect.

Finally, it has been suggested the mode of disclosure, either hand writing, typing, or talking, may be an important variable in the effectiveness of experimental disclosure. Regarding handwritten versus typed disclosure, hand writing may be superior to typing because, unless the participant is a skilled typist, typing will likely use up some of the participant's cognitive resources, causing him or her to be distracted from the main task of disclosure. This distraction may serve to lessen the task involvement and emotional arousal, which may ultimately reduce the effectiveness of the task (Brewin & Lennard, 1999). Regarding written versus oral disclosure, speaking may be superior to writing because, similar to the argument for hand writing over typing, speaking is easier than writing and makes fewer cognitive demands. Speaking also has the added benefit of allowing an additional mode of emotional expression—vocal expression—that may arouse more emotions and increase task involvement more than verbal expression alone (Murray & Segal, 1994).

Hypotheses of Proposed Moderators

The ways the aforementioned proposed moderators are expected to be related to effect sizes follow five general guidelines: Higher doses of disclosure (number of sessions, spacing of sessions, length of sessions) should be more beneficial, instructions that are highly specific (telling whether to switch topics, giving examples or specific suggestions) should be more beneficial, disclosing unresolved topics (recent, undisclosed topics) should be more beneficial, participants who typically inhibit their feelings (men, Asians, nonvolunteers) and those who have problems in need of intervention (those with a history of trauma or physical or psychological problems) should benefit more, and settings that provide minimal distractions (private rooms, lab settings, talking as opposed to writing or typing) should be more beneficial. In addition, previous research suggests that studies using college students, those that warn participants that they may be asked to disclose an upsetting event, those that use theoretically derived cognitive-processing instructions, and published studies should be more effective.

For a handful of the moderators to be examined, there is no specific prediction about directionality, either because the literature suggests opposing hypotheses or because previous research and theory do not necessarily suggest one direction or another. The following moderators are examined in an exploratory fashion: age of participant, educational level of participant, timing of follow-up visits, number of participants, valence of disclosure topics, and audience of disclosure (e.g., whether products of disclosure are turned in to the experimenter or kept by the participant).

In summary, the purpose of the present study is twofold: (a) to estimate the overall effect size of experimental disclosure using all available published and unpublished studies, to evaluate the precision of this effect size estimate by the confidence interval around the estimate, and to subject the obtained effect size to null hypothesis significance testing using a random effects model; and (b) to examine the potential impact of the aforementioned study variations using a random effects moderator analysis.

Method

Literature Search

Prior to conducting a formal literature search, I composed a list of 26 published articles and 3 unpublished papers that I was already familiar with (from having conducted previous work in this area). In addition, I examined the reference sections of the Smyth (1998) meta-analysis and a recent qualitative review of the literature (Baikie, 2003) for additional studies, which resulted in another 37 papers (33 published, 4 unpublished).

Once the initial list was composed, I conducted a more formal literature search. Following advice from Reed and Baxter (1994) and M. C. Rosenthal (in press), I used four methods to locate relevant studies: a keyword search, a backward search, a forward search, and a conference program search. Using the first method (the keyword search), I conducted an initial computer search of the PsycINFO and Medline databases using combinations of the following search terms: *coping*, *coping behavior*, *creative writing*, *depression* (*emotion*), *disclosing*, *disclosure*, *emotions*, *emotional adjustment*, *emotional control*, *emotional expression*, *emotional states*, *emotional*, *trauma*, *emotions*, *expression*, *expressive writing*, *grief*, *health*, *intervention*, *major depression*, *mental health*, *oral communication*, *Pennebaker*, *physical health*, *psychological health*, *posttraumatic stress disorder*, *randomly*, *self-disclosure*, *stress*, *stressful experiences*, *therapeutic*, *trauma*, *well-being*, *writing*, *written*, and *written communication*. This search included articles published between 1986 (the year that the original Pennebaker and Beall, 1986, expressive writing study was published) and March 2004 (the month and year that the literature search was conducted). For searches conducted in PsycINFO, I used the advanced search feature, limiting results to only articles written in English and those using an empirical study methodology.

Because some of the search terms were very broad in nature (e.g., *disclosure*, *emotion*), I followed guidelines to maximize the efficiency of the search:

1. In the first round of searching, a single term was entered into the keyword function of PsycINFO. If the term produced more than 250 hits (titles or titles with abstracts), it was determined to be too broad, and the hits were not examined. If the term produced fewer than 250 hits, the titles (and abstracts, if available) were scanned for acceptability. If the title or abstract suggested that the article might be a randomized controlled trial of experimental disclosure, it was flagged for later examination. This process was then repeated with the Medline database. Successful search terms in this round were *expressive writing* (10 articles), *stressful experiences* (2 articles), and *Pennebaker* (17 articles). All other terms either were too broad or did not lead to any potentially acceptable papers.

2. In the second round of searching, all remaining search terms that had been too broad in the first round were paired with each other (in all possible combinations) so that combinations of two terms were searched simultaneously (e.g., *disclosure* and *written*). As in the first round, only results from combinations of words that produced fewer than 250 hits were examined. Successful search combinations in this round were *coping behavior* and *emotional expression* (one article), *creative writing* and *emotional states* (one article), *creative writing* and *health* (two articles), *creative writing* and *stress* (one article), *depression* (*emotion*) and *writing* (one article), *disclosure* and *randomly* (one article), *disclosure* and *well being* (one article), *disclosure* and *written* (one article), *emotional adjustment* and *expression* (one article), *emotional adjustment* and *self disclosure* (one article), *emotional adjustment* and *written communication* (two articles), *emotional control* and *writing* (one article), *emotional expression* and *trauma* (one article), *emotional expression* and *written* (one article), *emotional trauma* and *disclosure* (one article), *emotions* and *oral communication* (one article), *emotions* and *self disclosure* (two articles), *emotions* and *written communication* (one article), *expression* and *randomly* (one article), *expression* and *writing* (one article), *health* and *disclosing* (one article), *major depression* and *written* (one article), *mental health* and *emotional states* (one article), *mental health* and *self disclosure* (one article), *physical health* and *psychological health* (three articles), *posttraumatic stress disorder* and *grief* (three articles), *stress* and *writing* (five articles), *stress* and *written communication* (three articles), *well being* and *expression* (one article), *writing* and *intervention* (two articles), *writing* and *randomly* (two articles), *written communication* and *therapeutic* (one article), *written communication* and *coping behavior* (one article), *written communication* and *depression* (*emotion*) (one article), *written communication* and *emotional states* (two articles), *written communication* and *emotional trauma* (one article), *written communication* and *health* (three articles), *written communication* and *major depression* (one article), *written communication* and *mental health* (three articles), *written communication* and *randomly* (one article), and *written communication* and *self disclosure* (one article).

3. In the third round of searching, all search terms that had been considered too broad in both the first and the second round were put into three-term combinations (all possible combinations). As in the first two rounds, only results from combinations of words that produced fewer than 250 hits were examined. Successful combinations in this round were *coping* and *emotion* and *expression* (one article) and *emotional trauma* and *major depression* and *stress* (one article).

A total of 56 published articles and 62 unpublished dissertations (listed in PsycINFO via *Dissertation Abstracts International*) were identified by this keyword method. When I had obtained the papers identified in the first step (keyword search), the next step was to complete a backward search, in which I located papers by searching reference lists of relevant papers and books. I examined the reference sections of all papers obtained from the first step (as well as two additional qualitative literature reviews that I was aware of; Sloan & Marx, 2004b, and Slatcher & Pennebaker, 2004) for potential additional studies. Forty-three published studies and 18 unpublished papers were identified by this method. In particular, the reference pages of the following articles included new and possibly useful references: Batten, Follette, Rasmussen Hall, and Palm (2002); Bower, Kemeny, Taylor, and Fahey (2003); Burton and King (2004); Esterling, Antoni, Fletcher, Marguiles, and Schneiderman (1994); Gallant and Lafreniere (2003); Greenberg et al. (1996); Mann (2001); Lepore and Smyth (2002); Norman, Lumley, Dooley, & Diamond (2004); Pennebaker and Seagal (1999); Slatcher & Pennebaker (in press); Smyth, Hockemeyer, et al. (2002); and Stroebe et al. (2002).

After the backward search, a forward search was conducted (in which one browses through titles and abstracts of articles that cite important works in the area of interest); the two important works in this study were Pennebaker & Beall (1986; the first experimental disclosure study) and Smyth (1998; the first experimental disclosure meta-analysis). This identified three published articles.

Finally, I conducted a manual search of abstracts from relevant conferences to locate additional unpublished works; I identified 22 unpublished papers in this way. In particular, I examined the following conference programs: American Psychological Association (2004), American Psychological Society (2002–2004), European Health Psychology Society (2003), Midwestern Psychological Association (2004), Society for Personality and Social Psychology (2003–2004), and Western Psychological Association (1998–2004). I chose these particular conference programs in part on the basis of their relevance to this particular search and in part on the basis of their ease of accessibility.

Once a preliminary list of papers was constructed on the basis of the aforementioned search steps, the primary authors of the papers that had been identified were contacted via e-mail and asked to identify sources that might still be missing; 12 published articles and 31 unpublished papers were identified using this method. Two active researchers in the domain of experimental disclosure (J. Pennebaker and M. Lumley) were especially useful at this stage, each providing a good number of additions to this list.

Inclusion and Exclusion Criteria

Once all studies were identified and obtained (a total of 250 studies), I examined the papers to determine eligibility for inclusion in the meta-analysis. To be included in this analysis, studies must have met the following criteria:

1. The study must have included some variation of the original experimental disclosure task developed by Pennebaker and Beall (1986). This task involves writing (or talking) about a real or imagined significant life event or personal topic. Studies in which experimenters gave either oral or written feedback regarding the disclosure were excluded, as this type of intervention closely resembles psychotherapy and is outside the scope of this meta-analysis. Fourteen studies were excluded according to this criterion.

2. The study must have been a randomized experiment that included a neutral control group. Participants in the control group must have either participated in a neutral activity (e.g., described what they had done in the past 24 hr) or have abstained from participating in any experimental activities other than completing pre- and posttest measures (e.g., empty control). Thirty-three studies were excluded according to this criterion.

3. The study must contain statistical information sufficient to compute an effect size. Six studies were excluded according to this criterion.

4. The study must contain outcome variables that were measured at least 1 day after the writing intervention was completed. Studies that measured only immediate reactions to writing were excluded. Eighteen studies were excluded according to this criterion.

5. The paper must present new data not already reported in an earlier source; it must not be a review of the literature. Six papers were excluded according to this criterion.

7. Assignment to treatment group must not have been confounded by any other variables (e.g., all treatment participants were concurrently receiving

an additional treatment that the control participants were not receiving). Two studies were excluded according to this criterion.

Altogether, 79 studies were excluded according these criteria.[2] Of the 171 remaining studies, 20 of the unpublished studies could not be obtained either because of an inability to contact the authors of the papers (14 papers) or because the authors reported that the papers were not ready yet for distribution (6 papers). An additional five papers were recognized as reports of additional data from a primary study that was reported elsewhere (e.g., two publications from the same data set); these papers were grouped with their accompanying primary paper, as data from the two papers were not independent. Therefore, 146 studies were included and coded in this meta-analysis.

Coding Procedure

As in the original meta-analysis by Smyth (1998), the following information was extracted from each study: (a) report information (authors, year of study, source of study), (b) setting information (sampling scope and population type), (c) participant information (sample size, age, education, gender, minority representation), (d) treatment information (number of disclosure sessions; length of disclosure sessions; spacing of disclosure sessions; trauma past, current, or mixed; location of disclosure sessions), (e) methodological information (attrition, outcomes), and (f) effect size information (statistic type, value, significance, direction). In addition, the following new variables were extracted from each paper: (a) funding status, (b) participant payment status, (c) participants' expectation of study benefit, (d) valence of disclosure topic, (e) mode of disclosure (hand writing, typing, speaking), (f) presence or absence of instruction to write about an undisclosed topic, (g) presence or absence of topic-switching-related instructions, (h) presence or absence of directed questions or examples in the writing instructions, (i) presence or absence of instructions designed to enhance cognitive processing or change, (j) privacy conditions of disclosure sessions, (k) participants' expectation of disclosing an upsetting topic, (l) months since event or disclosure topic, (m) type of control group (standard vs. empty), (n) audience of disclosure, (o) method of instruction administration (oral vs. written), and (p) timing of posttest or follow-up. I coded all studies using a precise coding book.

Calculating Effect Sizes

This study used the *r*-effect size. Most studies contained more than one outcome measure or effect size. In light of this, effect sizes were first coded into one of six outcome types: psychological health (e.g., depression, anxiety, stress), physiological functioning (e.g., immune parameters, heart rate, liver functioning), reported health (e.g., doctor's visits, self-reported physical symptoms, illness behaviors), health behaviors (e.g., eating behaviors, medication adherence, exercise), general functioning (e.g., school outcomes, work outcomes, interpersonal relationship outcomes), and subjective impact of the intervention (e.g., ratings of study enjoyment, perceived effectiveness of disclosure, and willingness to participate again). The first five outcome categories are identical to those used in the previous meta-analyses (Frisina et al., 2004; Smyth, 1998) for comparison purposes; the last outcome (subjective impact of the intervention) is an additional category.

Once the outcomes were coded into one of the six categories, effect sizes (of a single study) were first averaged within outcome type (e.g., a depression measure and an anxiety measure were averaged together for a composite of psychological health) and then averaged across outcome type (e.g., the composite for psychological health was averaged with the composite for physiological functioning) for an overall effect size for each study. This procedure was preferred over a more straightforward averaging of all the effect sizes of a single study because it prevents effect sizes of a certain type from receiving unequal weighting as a function of how many measurements of that type are present in a single study (e.g., one study may contain five measures of psychological health and only one measure of reported health). Furthermore, this procedure was preferred over other, less conservative methods (e.g., R. Rosenthal & Rubin, 1986) in an effort to keep the methodology similar to those used in the previous meta-analyses in this area.

In some cases, the study report indicated that a focused test had been conducted (e.g., *t* test, *F* test with one degree of freedom in the numerator), but rather than reporting any statistical information, it stated only that the test was significant or nonsignificant. In these cases, if the result was reported as significant, the *p* value was assumed to be one decimal place smaller than the alpha value (e.g., assumed to be .049 if the test was significant at the .05 level), and the *r*-effect size was calculated according to the procedures described by R. Rosenthal and Rubin (2003). If the result was reported as nonsignificant, the effect size was assumed to be zero. Assuming a nonsignificant result to have an effect size of zero is considered conservative (R. Rosenthal, 1991) and is consistent with Smyth's (1998) synthesis, in which the same procedure was applied.

In some other cases, an omnibus test (e.g., multivariate *F* test, univariate *F* test with more than one degree of freedom in the numerator) was given in the report without sufficient data to estimate the mean standard error term or group means, or it was stated only that the results of an omnibus test were significant or nonsignificant. In these cases, the effect size was coded as missing.

Significance Testing

Significance testing was conducted according to a random effects approach, except as otherwise noted. When a random effects approach was used, a one-sample *t* test (with $k - 1$ degrees of freedom) on the unweighted means was conducted. When a fixed effects approach was used, the *p* value was calculated according to the Stouffer method described by Mosteller and Bush (1954). Effects were considered significant when the *p* value was less than .05 in either the predicted or the unpredicted direction; effects were considered marginally significant when the *p* value was less than .10 in either the predicted or the unpredicted direction.

Coding Reliability

In some cases, information being coded for a particular study either was not obtainable from the study report (e.g., gender or ethnicity of the study participants was not reported) or was somewhat ambiguous (e.g., it was reported that participants wrote in a private room; with no mention that the participants were given paper and pens or were asked to type essays into a computer, it was unclear whether the disclosure was handwritten or typed). In these instances, the primary author of the study was contacted via e-mail and asked to provide the additional data or to clarify the information. Eighty authors were contact for this purpose; 41 answered the questions, 3 responded to the request but reported that they were unable to provide the information requested, and 36 never responded to the request.

When requested information was not provided and there were no clues in the report to support a reasonable estimate, the information was coded as missing data (e.g., gender or ethnicity of participants). When requested information was not provided but there were clues in the report to support a reasonable estimate, an estimation was made (e.g., it was reported that participants wrote in a private room, with no mention that the participants

[2] The criteria described in this section are the only guidelines by which a decision was made about whether to include or exclude the paper from the present analysis. All 250 papers were first independently screened for inclusion or exclusion by an advanced graduate student and were then screened by me. Disagreements regarding the inclusion or exclusion status of a particular paper (of which there were very few) were resolved by discussion.

were given paper and pens or were asked to type the essays into a computer; because most studies have participants hand write essays and because computer equipment would typically be described in a *Method* section if it were used, it was assumed that the participants hand wrote their essays).

Because the coding process did involve some degree of subjectivity, a small number of studies (11) were randomly selected from the total body of studies (146) and were independently coded by a second coder (an advanced graduate student) for both moderator variables and effect size estimates. For all but one of the variables, the reliability of the two coders was acceptably high (all rs $> .800$). The only item that did not have high reliability ($r = .418$) was the variable regarding the audience of disclosure (whether products of disclosure were turned in to the experimenter). Original study reports often were not clear on the issue of whether the participants turned in their disclosure essays, which made this variable difficult to code consistently. Therefore, moderator analyses using this audience variable should be interpreted with caution.

Ratings of Study Quality

Although the exclusion criteria used in the present study (excluding studies that did not use randomization as well as those with clear confounding variables) ensure that the studies included in the analysis pass at least a minimum standard of quality, all included studies were not equal in terms of quality. The four threats to study quality that were observed in some studies were high study attrition,[3] a failure to mask or blind experimenters to condition assignment, the use of an empty control group, and the creation of unequal expectations regarding treatment effectiveness (e.g., leading treatment participants—but not control participants—to expect to benefit from study participation).

I calculated study attrition by dividing the number of participants who had initially begun the study but for whom no data were available at any of the longer term follow-up time points divided by the total number of participants who initially began the study. Because almost all studies reported equal proportions of attrition among treatment groups (or failed to provide this level of detail), overall attrition was examined in lieu of differential attrition. Studies were labeled as having low attrition (good quality) when attrition rate was less than 20%; high-attrition (low-quality) studies had an attrition rate of at least 20%.

I estimated experimenter blinding and masking by coding for the way group-specific disclosure instructions were administered. Group-specific disclosure instructions (i.e., directions on exactly what the participant should write or talk about) can be administered verbally by the experimenter (e.g., Range et al., 2000) or in writing, with minimal experimenter involvement (e.g., Sheffield, Duncan, Thomson, & Johal, 2002). Allowing experimenters to verbally administer group-specific disclosure instructions makes it impossible for the experimenter to remain blind to condition. Therefore, when instructions were given verbally, it was assumed that the experimenter was not blind to condition (poor quality); when instructions were given only in writing, it was assumed that the experimenter was blind to condition (good quality). Although there might have been cases in which experimenters were not blind to condition even when instructions were administered in writing only, such scenarios are unlikely; indeed, when care was taken to administer instructions in writing only, the paper often explicitly stated that the researchers were blinded to randomization.

The use of an empty control group was defined as failure to provide the control participants with any task that would allow for them to have equal involvement in the study or equal contact with experimenters. Many studies in this research area use a control group in which participants are asked to write (or talk) about neutral topics without expressing their emotion; this allows for participants in all groups to have the same involvement in the study and the same amount of contact with experimenters. These studies were coded as having an active control group (good quality). By contrast, in other studies, control participants are only asked to fill out pre- and posttest questionnaires (or otherwise provide data) without completing any sort of neutral experimental task; these were coded as empty control group studies (poor quality).

The final quality variable concerns the equality of participant expectation. In many studies, all participants are given only a vague explanation of the purpose of the study, often being told that "this is an extremely important project looking at writing" (e.g., Francis & Pennebaker, 1992); in these instances, neither the treatment nor the control group is presumed to expect any benefit from study participation. In some other studies, all participants are given an indication that the writing (or talking) exercise they are asked to perform might be beneficial; in these instances, both treatment and control participants are presumed to expect to benefit from study participation (e.g., Beckwith, Greenberg, & Gevirtz, 2005). The problem arises in the instances when treatment participants are told that they might benefit from study participation but control participants, because they are made aware that they are in the control group, are not given this expectation (e.g., Barry & Singer, 2001). In the first two cases, participant expectation was coded as equal (good quality); in the last case, participant expectation was coded as unequal (poor quality). Each study was given a score of 1 (good quality) or 0 (poor quality) for each of the four variables, and the scores were then summed for a total quality score that ranged from 0 *(poorest quality)* to 4 *(best quality).*

Results

The Effects of Experimental Disclosure

Overall effects. One hundred forty-six separate studies were included in the overall meta-analysis. Seven different effect sizes were computed for each study: an effect size for each of the six outcome types described earlier (e.g., psychological health, physiological functioning) and an overall effect size. The number of participants, p values, all seven r-effect sizes, participant type, and writing topic are listed for each of the 146 studies in Table 1; the number of participants, number of studies, effect sizes, confidence intervals, p values, fail safe sample sizes, and tests of heterogeneity of the six different outcome types (and their subcategories) are presented in Table 2. Because it was predicted that experimental disclosure would have a positive effect (i.e., the treatment group would improve more than the control group), all p values are one-tailed in these tables (unless otherwise noted). In addition, a negative effect size indicates that the results were in the opposite direction from predicted (i.e., that the control group improved more than the treatment group).

A stem-and-leaf display of all 146 effect sizes is shown in Figure 1. Overall effect sizes ranged from $-.291$ to .592, with an unweighted mean effect size of .075, a weighted mean effect size of .063 (weighted by $N - 3$, per procedures described in R. Rosenthal, 1991), an unweighted median of .043, a weighted median of .044, and a standard deviation of .143. This effect was highly significant, with a one-sample t test (with 145 degrees of freedom) of 6.32, a p value of 3×10^{-9}, and a 95% confidence interval (based on the one-sample t test) of .052 to .098. Thirty-six

(*text continues on page 841*)

[3] An anonymous reviewer suggested that the presence or absence of intention-to-treat analyses be considered in the judgment of study quality. Unfortunately, almost no studies in this research area have included such analyses, which means that the inclusion of this variable on a quality rating scale has only limited utility. However, a measure of study attrition rate is included in the quality rating, which does address a similar component of study quality.

Table 1
Quality Score, Sample Size, Significance, Effect Size, Outcome Type, Population Type, and Writing Topic for Each Study

Study	Quality score (0–4)	*n*	*p*	Combined *r*	Correlation for different outcome types						Participant type	Writing topic
					A	B	C	D	E	F		
Allard et al. (2004)	4	65	.500	.000[a]			.000[a]				College students with high physical symptoms/ somatization	Upsetting interpersonal childhood experience
Anopchand (2000)	3	86	.880[b]	−.017						−.017	Incoming teaching preinternship students	Adjustment to teaching preinternship
Baikie (2003)	4	88	.031	.198	.001		.054		.499		College students	Upsetting experience
Barry & Singer (2001)	0	38	.013	.364	.364						Female caregivers of sick loved one	Sick loved one
Batten et al. (2002)	4	60	.280[b]	−.139	−.148		−.131				Female rape/sexual assault survivors	Rape/sexual assault
Beckwith et al. (2005)	3	38	.380	.053		.053					Hypertensive patients and community members	Upsetting experience
Bell-Pringle (2001)	4	84	.320	.052	.100		.075			−.020	College students with upsetting-family events/issues	Upsetting event regarding relationship with family
Birch (1998)	4	33	.700[b]	−.084	−.194		.108		−.161		College students	Upsetting experience and philosophy of life
Bird (1992)	2	26	.310	.103	.103						Former psychological patients	Philosophy of life, values, coping, and various other topics
Bodor (2002)	2	22	.045	.375	.450	.236	.620	.267	.242		Community members with Type I diabetes	Illness
Booth et al., (1997, Study 1)	3	37	.740[b]	−.057		−.057		−.057			College students	Upsetting experience
Bower et al. (2003)	3	43	.370	.054		.004			.140	.016	Bereaved women at perceived high risk for breast cancer	Bereavement
Broderick et al. (2004)	2	122	.210	.074	.050	.099					Rheumatoid arthritis patients	Upsetting experience
Broderick et al. (2005)	3	83	.140	.119	.189		.046				Female patients and community members with fibromyalgia	Only negative emotions regarding upsetting experience or only positive emotions
Burton & King (2004)	4	85	.035	.200			.200				College students	Positive events
Burton (2005)	3	61	.110	.159	.157		.160				College students	Positive event or benefits of positive event
Cameron (1998)	3	44	.470	.013			.061		−.034		College students with migraine headaches	Ways to cope with most-upsetting experience
Cameron & Nicholls (1998)	4	122	.069	.135	.210		.160			.033	Incoming college students	Coming to college or ways to cope with college
Campbell (2003)	3	25	.123	.236	.478		.000			.200	Female caregivers of a sick loved one	Sick loved one
Candell (2003)	3	68	.040	.209	.213		−.091		.470		Caregivers of a sick loved one	Upsetting experience or benefits of upsetting experience
Caplan (2001)	3	90	.840[b]	−.022	.001		−.046				Retirement community residents	Recent experience of loss
Cole (2003)	3	110	.780[b]	−.027	−.100		−.002		.021		College students	Upsetting experience
Crow (2000)	3	52	.260	.092	.018	.098	.177			.075	City employees	Upsetting experience
D'Souza et al. (2003)	3	90	.580[b]	−.060	−.064		−.058				College students with headaches	Upsetting experience
de Moor et al. (2002)	3	37	.130	.193	.036		.340				Cancer patients	Illness

(*table continues*)

Table 1 (*continued*)

Study	Quality score (0–4)	*n*	*p*	Combined *r*	Correlation for different outcome types						Participant type	Writing topic
					A	B	C	D	E	F		
Deters & Range (2003)	3	57	.190	.119	−.028		.000		.370		College students with a history of trauma	Upsetting experience
Dickerson et al. (2004)	3	51	.980[b]	−.004	−.021	−.032	.041				College students	Shame or guilt-inducing upsetting event
Donnelly & Murray (1991)	2	49	.500	.000[a]	.000[a]		.000[a]				College students	Upsetting experience
Earnhardt et al. (2002)	2	48	.480	.009	.009						Female college students with negative body image	Negative body image
Eells (2003)	4	127	.018[b]	−.209	−.370		−.084			−.162	College students	Upsetting experience or philosophy of life
Epstein et al. (2005)	3	94	.001	.376	.352		.400				College students	Upsetting experience
Erickson et al. (1998)	4	101	.960[b]	−.006	.041		−.045			−.013	Incoming college students	Coming to college
Esterling et al. (1994)	3	57	.001	.490		.490					EBV seropositive college students	Upsetting experience
Evans (2000)	4	48	.140	.159	.230					.086	Emotionally disturbed children	Upsetting experience
Fergusson (1994)	3	90	.470	.007	.075		.003		−.115	.065	College students	Upsetting experience or write in third person about upsetting experience
Fidler et al. (2004); Zakowski et al. (2004)	3	104	.410	.024	.027					.020	Cancer patients	Illness
Forston (1992)	3	23	.960[b]	−.013	.098	.008	−.145				Depressed, anxious, or bipolar patients	Upsetting experience
Francis & Pennebaker (1992)	3	41	.370	.053	.000	.153				.004	University employees	Upsetting experience
Frattaroli (2001)	4	119	.097	.122			.100		.180	.089	College students	Upsetting experience
Frattaroli (2003)	4	271	.210	.049	−.062		−.026		.230	.051	College students	Upsetting experience
Gidron et al. (1996)	3	14	.320[b]	−.291	−.091		−.469				Posttraumatic stress disorder patients	Upsetting experience
Gidron et al. (2002)	3	38	.057	.260			.260				Frequent clinic attendees	Upsetting experience
Gillis (2002)	2	67	.098	.163			.163				Patients, support group, and community members with fibromyalgia	Upsetting experience
Gortner (2004)	3	90	.230	.080	−.085		.150		.173		Formerly depressed college students	Upsetting experience
Greenberg & Stone (1992)	3	50	.450	.019	.023		.015				College students	Upsetting experience
Greenberg et al. (1996)	3	98	.968[b]	−.004	−.094		.086				Female college students with a history of trauma	Real or imaginary upsetting experience
Guastella & Dadds (2005); Guastella & Dadds (in press)	4	81	.180	.102			.102				College students	Upsetting experience or benefits of upsetting experience

Table 1 (*continued*)

Study	Quality score (0–4)	*n*	*p*	Combined *r*	Correlation for different outcome types						Participant type	Writing topic
					A	B	C	D	E	F		
Habbal (1999)	3	53	.940[b]	−.012	−.008		−.016				Support group and community member women with a family history of cancer	Upsetting experience
Haraway (2003)	4	49	.056	.231	.231						Incoming middle school students	Coming to college
Harris et al. (2004); Harris et al. (2005)	3	115	.410	.021		.021					Patients and community members with asthma	Upsetting experience or positive event
Heffner (2002)	2	31	.064	.278	.460	.121	.192			.320	Community seniors and assisted living residents	Upsetting experience
Hemenover (2003)	4	47	.024	.288	.288						College students	Upsetting experience
Hughes (1994)	3	104	.470	.004	−.071		.011	−.012	.072	.019	Incoming college students	Coming to college
Kelley et al. (1997); Lumley et al. (2001, Study 2)	3	72	.230	.088		−.084	.255				Rheumatoid arthritis patients	Upsetting experience
Kim (2004)	4	93	.191	.092	.134	.032	−.048		.244		College students	Upsetting experience
King (2001)	4	72	.011	.270			.270				College students	Upsetting experience and/or best possible self/future
King & Miner (2000)	3	85	.001	.592			.592				College students	Upsetting experience and/or benefits of upsetting experience
Klapow et al. (2001)	3	43	.520[b]	−.102					−.102		Medical patients	Upsetting experience
Klein & Boals (2001, Study 1)	4	72	.250	.082	.000				.239	.004	Incoming college students	Coming to college
Klein & Boals (2001, Study 2)	4	101	.099	.129	.078				.230	.079	College students	Upsetting experience or positive event
Kloss & Lisman (2002)	3	129	.480	.006	−.012		.024				College students	Upsetting experience or positive events
Kovac & Range (2000)	2	30	.140	.199	−.018		.340		.263		Bereaved college students	Bereavement
Kovac & Range (2002)	4	97	.270	.063	.032		.086		.070		College students with history of suicidal ideation	Suicidal/depressed time in life
Krantz & Pennebaker (1995)	2	37	.330	.050	.000				.000	.150	College students	Upsetting experience
Kunkel (2001)	3	206	.460	.007	.002		.012				College students	Upsetting experience
Kuntz (2003)	3	46	.500	.000[a]	.000[a]	.000[a]	.000[a]				Male college students	Upsetting experience
Laguna (1998)	3	59	.880[b]	−.021	−.090					.049	College students with high distress and a recent romantic breakup	Romantic breakup or only the facts about romantic breakup
Leake et al. (1999)	3	42	.210	.133	.134						Patients with end-stage renal disease	Illness
Lepore (1997)	3	74	.190	.105	.105						College students who were taking a major exam	Upcoming exam
Lepore & Greenberg (2002)	4	145	.170	.080	.070		.091			.081	Students with a recent romantic breakup	Romantic breakup

(*table continues*)

Table 1 (*continued*)

Study	Quality score (0–4)	*n*	*p*	Combined *r*	Correlation for different outcome types						Participant type	Writing topic
					A	B	C	D	E	F		
Lepore et al. (2000)	2	128	.016	.166	.166						College students	Upsetting film
Lepore et al. (2004, Study 1)	2	59	.999[b]	−.001	.044	−.046					Female college students	Upsetting film
Lepore et al. (2004, Study 2)	2	60	.180	.121	.114					.130	Female college students	Upsetting film
Levey-Thors (2000)	2	72	.500	.000[a]			.000[a]				Healthy and sick (upper respiratory symptoms) college students	Upsetting experience
Lumley et al. (2001, Study 1); Lumley & Provenzano (2003)	3	72	.300	.063						.063	College students with high physical symptoms/ somatization	Upsetting experience
Mann (2001)	2	40	.620[b]	−.079	.000		.018	−.250			Female HIV patients	Best possible self/future
Marston (2003)	3	47	.800[b]	−.039	−.079		.000[a]				HIV-positive patients and community members	Illness
McKenna (1997)	2	24	.380	.067	.143		−.010				Migraine headache sufferers	Illness
Murray & Segal (1994)	3	120	.500	.000[a]	.000[a]		.000[a]				College students	Upsetting experience
Murray et al. (1989)	2	14	.210	.226			.226				College students	Upsetting experience
Naufel & Beike (2004)	3	62	.500	.000[a]			.000[a]				College students	Upsetting experience or only the facts about upsetting experience
Njus et al. (1996)	4	48	.230	.110		.110					College students	Upsetting or funny video
O'Connor et al. (2005)	3	29	.500	.000[a]	.000[a]	.000[a]					Bereaved community members	Bereavement or benefits of bereavement
O'Heeron (1993)	3	56	.440	.020	.002		.010	.042	−.007	.050	At-risk children	Upsetting experience
Paez et al. (1999, Study 1)	3	50	.460	.015	.015						College students	Upsetting experience
Paez et al. (1999, Study 2)	3	53	.200[b]	−.180	−.180						College students	Upsetting experience
Pantchenko et al. (2003)	4	42	.390	.044	.035		.052				College students	Upsetting experience
Park & Blumberg (2002)	2	57	.380	.041	.085		−.003				College students	Upsetting experience
Pennebaker & Beall (1986)	3	46	.081	.209			.261		.160		College students	Upsetting experience or only the facts or only the emotions
Pennebaker & Francis (1996)	3	72	.052	.127	.000		.260			.116	Incoming college students	Coming to college
Pennebaker et al. (1988); Suedfeld & Pennebaker (1997)	3	50	.021	.286	.154	.086	.280		.570		College students	Upsetting experience

Table 1 (*continued*)

Study	Quality score (0–4)	*n*	*p*	Combined *r*	Correlation for different outcome types						Participant type	Writing topic
					A	B	C	D	E	F		
Pennebaker et al. (1990)	3	125	.170	.088			.031	.000[a]	.335	−.028	Incoming college students	Coming to college
Petrie et al. (1995); Booth et al. (1997, Study 2)	3	40	.900[b]	−.021		−.021					College students	Upsetting experience
Petrie et al. (2004)	3	37	.076	.240		.240					HIV patients	Upsetting experience
Range et al. (2000)	2	44	.310	.076	−.022		.034		.212		Bereaved college students	Bereavement
Raval (2000)	3	78	.150	.124	.220		.025				College students	Upsetting experience
Rentfrow & Keough (1999)	4	148	.760[b]	−.026						−.026	College students	Ways of coping with upcoming exam and/or only emotions about upcoming exam
Reynolds et al. (2000)	3	191	.500	.000[a]						.000[a]	Children under 18	Upsetting experience
Richards et al. (2000)	3	94	.160[b]	−.146	−.170		−.121				Psychiatric prison inmates	Upsetting experience
Rivkin (2000)	2	68	.420	.025	−.041		.000		.116		College students with intergenerational family conflict	Intergenerational family conflict
Rivkin et al. (2004)	2	62	.410	.030	−.024	.000[a]		.143	.000		HIV-positive community members	Illness
Romero (2004)	3	92	.150	.110						.110	Students and community members who have been hurt/wronged	Benefits of forgiveness or a time when hurt/wronged
Rosenberg et al. (2002)	1	30	.230	.143		.090	.195				Male cancer patients	Illness
Russ (1992)	1	55	.007	.327	.327						College students with a history of anxiety	Anxiety or generally about self
Scanlan (2000)	3	57	.460	.014	−.028		.064			.005	Incoming college students with a history of depression	Coming to college
Schantz (2001)	1	82	.420	.022	.022						College students with a current interpersonal problem	Interpersonal conflict
Schoutrop et al. (2002)	2	48	.037	.250	.250						College students with a history of trauma	Upsetting experience
Schwartz & Drotar (2004)	2	54	.440[b]	−.107	−.067		−.145				Caregivers of sick loved one	Upsetting experience
Sharsky (1997)	4	87	.420[b]	−.087	−.087						College students	Upsetting experience or positive event
Sheese et al. (2004)	4	535	.079	.061	.000		.058		.089	.099	College students	Upsetting experience
Sheffield et al. (2002)	3	46	.240[b]	−.178	.124		−.260			−.380	College students	Upsetting experience
Shulman et al. (2005)	3	134	.290	.049	.049						College students	Gratitude/what they are thankful for
Slatcher & Pennebaker (in press)	4	86	.013	.239					.375	.093	College students	Romantic relationship
Sloan & Marx (2004a)	3	49	.001	.524	.595		.446				Female college students with posttraumatic stress symptoms	Upsetting experience

(*table continues*)

Table 1 (*continued*)

Study	Quality score (0–4)	*n*	*p*	Combined *r*	Correlation for different outcome types						Participant type	Writing topic
					A	B	C	D	E	F		
Sloan et al. (2005)	4	79	.094	.151	.172		.130				College students with posttraumatic stress symptoms	Upsetting experience
J. L. Smith (2003)	1	99	.460	.009	.002		.016				College students	Upsetting experience
Smyth et al. (1999); Stone et al. (2000); Smyth, Anderson, et al. (2002)	4	107	.600[b]	−.052	−.054		−.024	−.012		−.117	Community members with asthma or rheumatoid arthritis	Upsetting experience
Smyth et al. (2001)	4	118	.480	.005	−.180		.190				College students	Upsetting experience
Smyth, Hockemeyer, et al. (2002)	3	42	.390	.044	.031		.057				College student hurricane survivors	Hurricane
Solano et al. (2003)	2	40	.007	.387	.390		.383				Inpatients scheduled for papilloma resection	Illness
Solano et al. (2005)	2	40	.280	.098	.098		.097				Patients with an upcoming urology surgery	Illness
Somers (1998)	3	85	.420	.021	.015					.027	College students	Upsetting experience
Spera et al. (1994)	3	41	.120	.186						.186	Unemployed outplacement clients	Job loss
Spurlock (2001)	3	40	.720[b]	−.060			−.060				College students	Upsetting experience
Stanton et al. (2002)	3	60	.230	.100	.070		.121		.107		Female cancer patients	Illness
Stetler et al. (2004)	4	44	.340[b]	−.145		−.145					Minority college students and employees	Racism or discrimination
Stroebe et al. (2002)	1	87	.540[b]	−.067	−.013		−.120				Bereaved community members	Problems to cope with because of bereavement and/or only emotions about bereavement
Swanbon (1999)	2	62	.200	.107	.119		.130			.079	Male homosexuals	Homosexuality
L. A. Taylor et al. (2003)	1	39	.400	.044	−.038	.053	.115				Cystic fibrosis patients	Upsetting experience
Thomas & Frattaroli (2005)	3	48	.210	.120	.068					.173	College students who were taking a major exam	Upcoming exam
Tromp (1998)	4	36	.330	.078	.010		.122			.101	Female prison inmates	Upsetting experience
Truxillo (2001, Study 1)	4	40	.700[b]	−.064	−.009	−.015	−.180	−.050			College students	Fitness goals
Truxillo (2001, Study 2)	3	80	.920[b]	−.012	−.100	−.019	.120	−.051			Female college students	Fitness goals
Ullrich & Lutgendorf (2002)	3	122	.860[b]	−.015	.120		−.150				College students	Upsetting experience or only emotions about upsetting experience
Valusek & Berrenberg (2004)	3	33	.390	.052	−.120					.220	College students	Upsetting test-taking experience
Van Middendorp (2004)	4	68	.480	.006	.042	.069	.004		−.018	−.066	Rheumatoid arthritis patients	Upsetting experience
Vaughn et al. (2003)	4	84	.120	.130	.130						College students	Best possible self (or ways to reach it)

Table 1 (*continued*)

Study	Quality score (0–4)	*n*	*p*	Combined *r*	Correlation for different outcome types						Participant type	Writing topic
					A	B	C	D	E	F		
Wagner (2001)	3	148	.400	.021	.021						College students with a history of trauma	Upsetting experience or positive event
Walker et al. (1999)	1	39	.800[b]	−.043	−.043						Female cancer patients	Illness
Warner et al. (2004)	3	50	.080	.202	.317	.159	.126				Asthma patients	Upsetting experience
Wetherall (in press)	2	34	.460	.016	−.015	.026	.038				Rheumatoid arthritis patients	Upsetting experience
Williams-Avery (1999, Study 1)	3	35	.640[b]	−.084							College students	Upsetting experience or only the facts about upsetting experience
Williams-Avery (1999, Study 2)	3	218	.960[b]	−.004	−.011		−.070		.070		College students	Upsetting experience
Wilson (2000)	3	87	.430	.019	.081		−.017		.000[a]	.012	Incoming college students	Coming to college
Wonacott (2001)	3	200	.470	.002	.032		−.012	−.012		−.002	Incoming college students	Coming to college
Yanko (2001)	3	117	.500	.001	−.034		.034				Female college students with a history of trauma	Real or imaginary upsetting experience

Note. All *p* values are one-tailed unless otherwise noted. For the Quality score, a higher score indicates better study quality. A = psychological health; B = physiological functioning; C = reported health; D = health behaviors; E = subjective impact of the intervention; F = general functioning/life outcomes; EBV = Epstein-Barr Virus.

[a] The report did not contain enough statistical information to compute a precise effect size. Effect size was estimated as zero because the effect was reported to be nonsignificant. [b] The *p* value is two-tailed.

studies (25%) had a negative effect size, eight studies (5%) had an effect size of zero, and 102 studies (70%) had a positive effect size; according to a vote-counting approach, the difference between the number of studies with a null or negative effect (44) and the number of studies with a positive effect (102) was also significant, $\chi^2(1, N = 146) = 23.04$, $p = .00000079$, $r = .397$. The total number of participants in all 146 studies was 10,994, with a median of 60 participants per study.

Psychological health. The first of the six outcome types, psychological health, was measured by 112 different studies, with a mean unweighted effect size of .056 and a mean weighted effect size of .034. The unweighted effect was significant in a random effects analysis with a *p* of .00014. Because psychological health is a rather broad domain, it was further broken up into 13 subcategories: anger (2 studies), grief/bereavement (4 studies), distress (33 studies), anxiety (9 studies), positive human functioning (61 studies), stress (45 studies), coping/coping strategies (17 studies), cognitive schemas/core beliefs (4 studies), posttraumatic/stress-related growth (4 studies), eating-disorder-related problems (3 studies), and dissociative experiences (1 study). Of these 13 subcategories, three were significant in a random effects analysis: distress ($r = .102$), depression ($r = .073$), and positive functioning ($r = .045$). The findings for anger and anxiety are also worth mentioning; because only 2 studies examined anger-related outcomes and 9 studies examined anxiety-related outcomes, it is less prudent to examine these effect sizes using a random effects approach. When these two subcategories were reexamined with a fixed effects approach, both average effect sizes were significant ($r = .183$ for anger; $r = .051$ for anxiety). No other psychological health outcomes approached significance in either the random or the fixed effects approach.

As noted in Table 2, many of the subcategories of psychological health contained even more detailed subcategories of their own (e.g., positive functioning included measures of mood, satisfaction with life, happiness, and optimism). Although it is beyond the scope of this article to present mean effect sizes for all the lower level categories, there are two smaller scale outcome types that are important to examine, in light of theories of experimental disclosure—posttraumatic stress symptoms (a subscale of stress) and self-regulation (a subscale of coping/coping strategies)—as these two were expected to improve in light of exposure theory and self-regulation theory, respectively. Analyses revealed that the unweighted average effect size for posttraumatic stress symptoms (with 38 studies measuring this outcome type) was .032 with a random effects significance level of .130, and the weighted average effect size was .022 with a fixed effects significance level of .072. In addition, the unweighted average effect size for self-regulation (as measured by 3 studies) was −.077 with a random effects (two-tailed) significance level of .683, and the weighted average effect size was −.186 with a fixed effects (two-tailed) significance level of .124.

Physiological functioning. The second of the six outcome types, physiological functioning, was measured by 30 different studies, with a mean unweighted effect size of .059 and a mean weighted effect size of .054. This unweighted effect was significant in a random effects analysis with a significance level of .0075. Because physiological functioning is a rather broad domain, it was further broken up into 16 subcategories: HIV viral load (1 study), blood glucose levels (1 study), liver function (1 study), immune parameters (13 studies), erythrocyte sedimentation rate (2 studies),

Table 2
Sample Size, Effect Size, Confidence Interval, Significance, Fail Safe Sample Size, and Test of Homogeneity of Effect Size for Each Outcome Measure

Variable	Sample size		Mean *r*-effect size		95% CI (random effects)		*p*		Fail safe	Test of homogeneity	
	N	*K*	Weighted	Unweighted	Lower limit	Upper limit	Fixed	Random	*N*	$\chi^2(1)$	*p*
Psychological health	8,533	112	.034	.056	.026	.086	.00007	.00014	488	150.17	.008
Anger (e.g., Beck Anger Inventory)	73	2	.211	.183	−.695	.841	.054	.13			
Grief/bereavement (e.g., Grief Experience Inventory)	218	4	.020	.137	−.399	.603	.17	.24		10.58	.014
Distress (e.g., General Health Questionnaire)	2,435	33	.059	.102	.042	.161	.0001	.0016	90	49.94	.023
Depression (e.g., Beck Depression Inventory)	2,098	27	.044	.073	−.011	.156	.024	.043	12	56.02	.001
Anxiety (e.g., Beck Anxiety Inventory)	582	9	.028	.051	−.091	.189	4×10^{-8}	.22	87	16.67	.034
Positive functioning (e.g., Satisfaction With Life Scale)	4,535	61	.031	.045	.009	.081	.0098	.0075	62	52.47	.744
Stress (e.g., Perceived Stress Scale)	279	45	.023	.029	−.020	.078	.11	.12		55.10	.122
Coping strategies (e.g., Emotional Approach Coping Scale)	1,195	17	−.014	.002	−.080	.084	.90[a]	.48		30.10	.018
Cognitive schemas (e.g., World Assumptions Scale)	499	4	.003	−.005	−.044	.035	.36	.74[a]		0.17	.982
Posttraumatic/stress-related growth (e.g., Stress-Related Growth Scale)	489	4	.001	−.009	−.154	.136	.48	.86[a]		2.53	.470
Eating-disorder-related problems (e.g., Eating Disorder Inventory)	144	3	−.030	−.020	−.072	.033	.78[a]	.24[a]		0.03	.985
Dissociative experiences (e.g., Dissociative Experience Scale)	57	1		−.047			.72[a]				
Physiological functioning	1,510	30	.054	.060	.013	.106	.02	.0075	17	21.44	.843
HIV viral load	39	1		.331			.019		1		
Blood glucose levels	22	1		.236			.15				
Liver function (e.g., SGOT, SGPT)	36	1		.287			.043		1		
Immune parameters (e.g., IL-8, CD8)	560	13	.098	.099	−.007	.202	.014	.032	10	16.40	.174
Blood lipids (e.g., cholesterol, triglycerides)	36	1		.071			.46				
Lung capacity/lung function (e.g., FEV, FVC)	186	3	.043	.066	−.134	.262	.24	.15		0.47	.791
Blood pressure	261	5	.044	.038	−.123	.198	.26	.27		2.90	.575
Stress-related measures (e.g., cortisol, adrenaline)	270	6	.025	.034	−.049	.116	.33	.17		1.25	.940
Body composition (e.g., BMI, weight)	104	3	.016	.009	−.469	.534	.32	.42		0.30	.861
Heart (e.g., heart rate)	403	8	.003	−.013	−.105	.078	.47	.74[a]		2.45	.931
Strength (e.g., push-ups)	120	2	−.010	−.018	−.311	.278	.88[a]	.58[a]			
Joint condition/arthritis (e.g., grip strength, ESR)	289	4	.044	.025	−.100	.148	.30	.29		1.40	.706

Table 2 (*continued*)

Variable	Sample size		Mean *r*-effect size		95% CI (random effects)		*p*		Fail safe	Test of homogeneity	
	N	*K*	Weighted	Unweighted	Lower limit	Upper limit	Fixed	Random	*N*	$\chi^2(1)$	*p*
Reported health	7,461	95	.056	.072	.036	.107	4×10^{-7}	.00011	763	143.02	.001
Specific disease outcomes (e.g., HIV Symptom Scale)	631	12	.123	.128	.049	.204	.0012	.002	29	7.66	.743
Illness behaviors (e.g., medication use, doctor's visits)	4,690	54	.062	.073	.015	.131	.000008	.0075	318	103.16	.001
General physical symptoms (e.g., PILL, Fatigue Severity Scale)	4,847	59	.017	.021	−.021	.063	.068	.16		99.17	.001
Health behaviors	829	10	−.005	.007	−.091	.104	.98[a]	.44		5.48	.796
Healthy diet (e.g., ate too much)	189	4	.076	.074	−.063	.209	.16	.092		1.02	.796
Physical activity	298	5	−.009	.072	−.253	.383	.72[a]	.29		6.35	.174
Substance use (e.g., alcohol, caffeine)	260	3	.026	.021	−.064	.106	.35	.20		0.20	.901
Hours of sleep per night	204	2	−.043	−.042	−.524	.460	.54[a]	.50[a]			
Adherence to medical treatment	40	1		−.255			.12[a]				
Subjective impact of intervention	3,032	33	.152	.159	.092	.225	7×10^{-15}	.000035	412	75.52	.001
Positive attitude about intervention (e.g., intervention had positive effect)	2,143	26	.257	.270	.173	.361	3×10^{-24}	.0000083	324	80.90	.001
Attempts to process/ make sense of event (e.g., talked about event)	1,586	21	.122	.132	.055	.207	.000009	.001	122	46.97	.001
Intervention had no effect	43	1		−.031			.80[a]				
Negative attitude about intervention (e.g., intervention had negative effect)	1,123	11	−.017	−.062	−.235	.115	.44[a]	.46[a]		28.58	.002
General functioning/life outcomes	4,147	43	.036	.046	.011	.081	.0075	.0055	51	29.50	.927
Work-related outcomes (e.g., work absenteeism)	268	5	.069	.084	−.032	.198	.11	.058		1.80	.772
Social relationships (e.g., Wade Forgiveness Scale)	1,312	16	.031	.060	.002	.117	.042	.021	2	8.39	.907
Cognitive functioning (e.g., working memory)	405	5	.050	.058	−.002	.118	.12	.027		0.81	.996
School outcomes (e.g., GPA, College Adjustment Test)	2,499	21	.043	.038	−.017	.092	.038	.083	3	15.13	.820
Law/forensic outcomes (e.g., traffic tickets)	192	3	−.021	.011	−.213	.234	.94[a]	.42		0.92	.631
Goals (e.g., Life Goals Inventory)	313	4	−.064	−.034	−.173	.107	.40[a]	.50[a]		2.14	.544
Overall (All outcomes combined)	10,994	146	.063	.075	.051	.098	2×10^{-11}	3×10^{-9}	1,821	169.00	.094

Note. CI = confidence interval; SGOT = serum glutamic oxaloacetic transaminase; SGPT = serum glutamic pyruvic transaminase; IL-8 = interleukin 8; CD8 = cluster of differentiation 8; FEV = forced expiratory volume; FVC = forced vital capacity; BMI = body mass index; ESR = erythrocyte sedimentation rate; PILL = Pennebaker Inventory of Limbic Languidness; GPA = grade point average.

[a] *p* value is two-tailed.

Stem	Leaf
.55	9
.50	2
.45	9
.40	
.35	6, 8, 8, 9
.30	3
.25	5, 6, 7, 8, 9, 9
.20	0, 0, 0, 0 1, 1, 3, 3, 4, 4, 4
.15	5, 6, 6, 6, 7, 9, 9, 9
.10	0, 0, 0, 0, 0, 1, 1, 1, 2, 2, 2, 2, 2, 2, 3, 3, 3, 4, 4
.05	5, 5, 5, 5, 5, 5, 5, 5, 5, 5, 5, 5, 7, 7, 7, 8, 8, 8, 8, 9, 9, 9, 9
.00	0, 0, 0, 0, 0, 0, 0, 0, 0, 0, 0, 0, 0, 0, 0, 0, 1, 1, 1, 1, 1, 1, 1, 1, 1, 1, 2, 2, 2, 2, 2, 2, 2, 2, 2, 2, 3, 4, 4, 4, 4
-.00	1, 1, 1, 1, 2, 2, 2, 2, 2, 3, 3, 3, 4, 4, 4
-.05	5, 6, 6, 6, 6, 7, 8, 8, 8, 8
-.10	0, 1, 4, 4
-.15	8, 8
-.20	1
-.25	9

Figure 1. A stem-and-leaf display of overall study effect sizes.

blood lipids (1 study), C-reactive protein (1 study), lung capacity/lung function (3 studies), blood pressure (5 studies), stress-related measures (6 studies), body composition (3 studies), heart (8 studies), strength (2 studies), and joint condition (4 studies). Of these 16 subcategories, only the immune parameters subcategory was significant in a random effects analysis ($r = .099$). Several other subcategories in this group were only measured in 1 study—although at least 2 studies are required for a meta-analysis, it is worth noting the effect sizes (and significance levels) of the more impressive outcomes, as points of comparison for future research: HIV viral load had an effect size of .331 ($p = .019$), and liver function had an effect size of .287 ($p = .043$). No other physiological functioning outcomes approached significance in either the random or the fixed effects approach.

Reported health. The third of the six outcome types, reported health, was measured by 95 different studies, with a mean unweighted effect size of .072 and a mean weighted effect size of .056. This unweighted effect was significant in a random effects analysis with a significance level of .00011. Because reported health is a rather broad domain, it was further broken up into three subcategories: specific disease outcomes (12 studies), illness behaviors (54 studies), and general physical symptoms (59 studies). Of these three subcategories, two were significant in a random effects analysis: specific disease outcomes ($r = .128$) and illness behaviors ($r = .073$). General physical symptoms ($r = .021$) was marginally significant in a fixed effects approach ($N = 4{,}847$).

Health behaviors. The fourth of the six outcome types, health behaviors, was measured by 10 different studies, with a mean unweighted effect size of .007 and a mean weighted effect size of −.005. This unweighted effect failed to approach significance in a random effects analysis with a significance level of .44. Because health behaviors is a rather broad domain, it was further broken up into five subcategories: healthy diet (4 studies), physical activity (5 studies), substance use (3 studies), hours of sleep per night (2 studies), and adherence to medical treatment (1 study). Of these five subcategories, only one approached significance in a random effects analysis: healthy diet ($r = .074$). No other health behaviors outcomes approached significance in either the random or the fixed effects approach.

Subjective impact of the intervention. The fifth of the six outcome types, subjective impact of the intervention, was measured by 33 different studies, with a mean unweighted effect size of .159 and a mean weighted effect size of .152. This unweighted effect was significant in a random effects analysis with a significance level of .000035. Because subjective impact of the intervention is a rather broad domain, it was further broken up into four subcategories: positive attitude about intervention (26 studies), attempts to process/make sense of event (21 studies), intervention had no effect (1 study), and negative attitude about intervention (11 studies). Of these four subcategories, two were significant in a random effects analysis: positive attitude about intervention ($r = .270$) and attempts to process/make sense of event ($r = .132$). No other subjective impact outcomes approached significance in either the random or the fixed effects approach.

In light of social integration theory, which would predict an increase in the degree to which participants talk about their event with others after the intervention, it is appropriate to examine one of the lower level categories of the attempts to process/make sense of event subcategory, a variable that measures how much participants talked to others about the study, about their writing topic, or about their traumatic event in the weeks and months following disclosure. This variable (talked about study or topic) was mea-

sured by 12 studies, and it had an unweighted mean effect size of .027 (with a random effects significance level of .22) and a weighted mean effect size of .017 (with a fixed effects significance level of .32).

General functioning/life outcomes. The final of the six outcome types, general functioning/life outcomes, was measured by 43 different studies, with a mean unweighted effect size of .046 and a mean weighted effect size of .036. This unweighted effect was significant in a random effects analysis with a significance level of .0055. Because general functioning/life outcomes is a rather broad domain, it was further broken up into six subcategories: work-related outcomes (5 studies), social relationships (16 studies), cognitive functioning (5 studies), school outcomes (21 studies), law/forensic outcomes (3 studies), and goals (4 studies). Of these six subcategories, four were significant (or marginally significant) in a random effects analysis: work-related outcomes ($r = .084$), social relationships ($r = .060$), cognitive functioning ($r = .058$), and school outcomes ($r = .038$). No other general functioning outcomes approached significance in either the random or the fixed effects approach.

Moderating Variables

All of the coded variables described earlier (e.g., length of writing sessions, spacing of writing sessions) were examined as potential moderators of the overall effect of experimental disclosure. Because the effect sizes in the outcome types of psychological health, reported health, and subjective impact of the intervention all had significant within-group variability, moderator analysis was also conducted for these outcomes types. Table 3 displays descriptive information for each potential moderator and the results of the moderator analysis, including the t or F value of the test of the moderator (when appropriate); the r-effect size of the moderator (or correlation between the moderator variable and the effect sizes); and the p value of the statistical test for the overall effect, the psychological health effect, the reported health effect, and the subjective impact effect.

Report information variables. Published studies had significantly higher overall effect sizes (published, $r = .095$; unpublished, $r = .054$) and reported health effect sizes (published, $r = .141$; unpublished, $r = .064$). Publication status also marginally moderated the subjective impact effect size (published, $r = .213$; unpublished, $r = .123$), but there was no significant difference between published and unpublished studies for the psychological health effect size. Across the 146 studies, 52% were published.

Setting variables. Six variables that described the general setting, including the use of special populations (Stock, 1994), were included in this category: physical health selection criteria (whether participants were required to have a physical health problem to participate), psychological health selection criteria (whether participants were required to have a psychological health problem to participate), trauma/stressor history selection criteria (whether participants were required to have a history of a trauma or serious stressor to participate), population type (college students vs. nonstudents), location of disclosure sessions (controlled setting vs. home), and privacy of disclosure conditions (private room vs. room with other participants).

Physical health selection criteria did not significantly moderate the effect of experimental disclosure for the overall effect size, psychological health effect size, or subjective impact effect size. However, studies in which participants were required to have a physical health problem had significantly higher reported health effect sizes than studies in which this was not an eligibility criterion (health criteria, $r = .131$; no health criteria, $r = .054$). Trauma/stressor history selection criteria did not significantly moderate the effect of experimental disclosure for the overall effect size, psychological health effect size, or reported health effect size. However, studies in which participants were required to have a history of a trauma or serious stressor had marginally higher subjective impact effect sizes than studies in which this was not an eligibility criterion (trauma criteria, $r = .226$; no trauma criteria, $r = .129$). College student status did not significantly moderate the effects of experimental disclosure for the overall effect size, reported health effect size, or subjective impact effect size. However, studies that did not draw their sample from college students had marginally higher psychological health effect sizes than studies that included only college students (nonstudents, $r = .092$; students, $r = .036$). Psychological health selection criteria did not significantly moderate any of the effect size categories. Across the 146 studies, 23% included only participants who had a physical health problem, 10% included only participants who had a psychological health problem, 27% included only participants who had a history of trauma or a serious stressor, and 64% included only college students.

The location of the disclosure sessions did not significantly moderate the effect of experimental disclosure on the overall effect size, reported health effect size, or subjective impact effect size. However, studies in which participants disclosed at home had significantly higher psychological health effect sizes than studies in which participants disclosed in a controlled setting, such as a psychological laboratory (home, $r = .122$; controlled setting, $r = .034$). The privacy conditions of the disclosure sessions acted as a moderator for the overall effect size (private room, $r = .085$; public room, $r = .034$) and psychological health effect size (private room, $r = .069$; public room, $r = .028$), such that studies in which participants disclosed in a room by themselves had significantly larger effect sizes than studies in which participants disclosed in a room with other participants. The privacy conditions did not significantly moderate reported health or subjective impact effect sizes. Sixty-eight percent of studies had participants disclose in a controlled setting, and 79% of studies had participants disclose in a private room.

Participant variables. For the participant variables, the analysis of moderators took on two different approaches: a between-studies approach, and a within-study approach. The between-studies approach is the same approach that was used for all the other moderator categories: A value for each variable was extracted from each study report (e.g., average age of participants in each study), and that moderator variable was then correlated with the effect sizes. This was done in the cases in which most studies (more than 50%) provided information on the variable in question; in particular, between-studies analyses were done for the variables of gender, age, ethnicity, and education level. The within-study approach was used in the cases in which most studies did not measure or report information on the variable in question but at least two studies examined the variable as a moderator in the study itself. In those cases, the effect size for that moderator (either the r-effect size associated with the Treatment $\times$ Moderator F or the

Table 3
Descriptive Information and Between-Studies Tests of Moderators (Random Effects)

Variable	Descriptive information	Overall effect size	Psychological health effect size	Reported health effect size	Subjective impact effect size
		Report information			
Publication status	52% of studies were published	***t*(144) = 1.744** ***r* = .144** ***p* (one-tailed) = .042**	*t*(110) = 0.226 *r* = .025 *p* (one-tailed) = .40	***t*(93) = 2.012** ***r* = .204** ***p* (one-tailed) = .024**	***t*(31) = 1.376** ***r* = .238** ***p* (one-tailed) = .091**
		Setting information			
Physical health selection criteria	23% of studies had physical health selection criteria	*t*(143) = 1.080 *r* = .090 *p* (one-tailed) = .14	*t*(110) = 0.791 *r* = .075 *p* (one-tailed) = .21	***t*(92) = 1.838** ***r* = .188** ***p* (one-tailed) = .034**	*t*(31) = 1.232 *r* = −.16 *p* (two-tailed) = .22
Trauma/stressor history selection criteria	27% of studies had trauma/stressor history selection criteria	*t*(144) = 0.544 *r* = .046 *p* (one-tailed) = .29	*t*(110) = 1.061 *r* = .101 *p* (one-tailed) = .15	*t*(93) = 0.471 *r* = −.049 *p* (two-tailed) = .64	***t*(31) = 1.403** ***r* = .244** ***p* (one-tailed) = .086**
College student status	64% of studies used only college students	*t*(142) = 0.108 *r* = −.009 *p* (two-tailed) = .91	***t*(110) = 1.847** ***r* = −.173** ***p* (two-tailed) = .067**	*t*(93) = 0.190 *r* = .020 *p* (one-tailed) = .42	*t*(31) = 0.855 *r* = .152 *p* (one-tailed) = .20
Psychological health selection criteria	10% of studies had psychological health selection criteria	*t*(144) = 0.018 *r* = −.002 *p* (two-tailed) = .99	*t*(110) = 0.775 *r* = .074 *p* (one-tailed) = .22	*t*(93) = 1.011 *r* = −.104 *p* (two-tailed) = .32	*t*(31) = 0.763 *r* = −.136 *p* (two-tailed) = .46
Location of disclosure sessions	69% of studies conducted disclosure sessions in a controlled setting	*t*(128) = 0.378 *r* = −.033 *p* (one-tailed) = .70	***t*(98) = 2.557** ***r* = −.250** ***p* (two-tailed) = .012**	*t*(83) = 0.902 *r* = .093 *p* (one-tailed) = .18	*t*(23) = 0.548 *r* = −.144 *p* (two-tailed) = .58
Privacy of disclosure sessions	79% of studies had participants disclose in a private room	***t*(143) = 1.772** ***r* = .147** ***p* (one-tailed) = .04**	***t*(110) = 1.781** ***r* = .167** ***p* (one-tailed) = .039**	*t*(93) = 0.902 *r* = .093 *p* (one-tailed) = .18	*t*(31) = 1.256 *r* = .220 *p* (one-tailed) = .11
		Participant information			
Proportion of male participants	On average, 34% of study participants were male	*r*(138) = .083 *p* (one-tailed) = .166	*r*(105) = −.005 *p* (two-tailed) = .957	*r*(89) = .056 *p* (one-tailed) = .301	*r*(31) = −.121 *p* (two-tailed) = .503
Age	The average participant was 29 years of age	*r*(142) = .007 *p* (two-tailed) = .93	*r*(108) = .115 *p* (two-tailed) = .23	*r*(91) = .077 *p* (two-tailed) = .46	*r*(31) = −.126 *p* (two-tailed) = .49
Educational level	The average participant completed some college	*r*(125) = −.040, *p* (two-tailed) = .65	*r*(93) = −.157 *p* (two-tailed) = .129	*r*(80) = .036 *p* (two-tailed) = .75	*r*(29) = .267 *p* (two-tailed) = .15
Ethnicity: proportion of Caucasian participants	On average, 72% of study participants were Caucasian	*r*(96) = .075 *p* (two-tailed) = .47	*r*(72) = .032 *p* (two-tailed) = .79	*r*(62) = .146 *p* (two-tailed) = .25	*r*(20) = .145 *p* (two-tailed) = .52
Ethnicity: proportion of Black/African American participants	On average, 12% of study participants were Black/African American	*r*(96) = −.108 *p* (two-tailed) = .29	*r*(72) = .052 *p* (two-tailed) = .66	*r*(62) = −.097 *p* (two-tailed) = .44	*r*(20) = −.139 *p* (two-tailed) = .15
Ethnicity: proportion of Asian participants	On average, 7% of study participants were Asian	*r*(96) = −.013 *p* (two-tailed) = .90	*r*(72) = −.084 *p* (two-tailed) = .48	*r*(62) = −.142 *p* (two-tailed) = .26	*r*(20) = −.024 *p* (two-tailed) = .92
Ethnicity: proportion of Hispanic/Latino participants	On average, 5% of study participants were Hispanic/Latino	*r*(96) = .033 *p* (two-tailed) = .75	*r*(72) = −.015 *p* (two-tailed) = .90	*r*(62) = −.046 *p* (two-tailed) = .72	*r*(20) = .151 *p* (two-tailed) = .50
		Methodological information			
No. participants	The average study had 78 participants	*r*(144) = −.108 *p* (two-tailed) = .20	***r*(109) = −.181** ***p* (two-tailed) = .058**	*r*(92) = −.111 *p* (two-tailed) = .14	*r*(31) = −.030 *p* (two-tailed) = .44
Participant payment	73% of studies paid their participants	*t*(137) = 0.316 *r* = .027 *p* (one-tailed) = .38	*t*(106) = 0.435 *r* = −.042 *p* (two-tailed) = .66	*t*(89) = 0.664 *r* = .070 *p* (one-tailed) = .25	*t*(29) = 2.169 *r* = .374 *p* (one-tailed) = .019
Predisclosure priming	19% of studies warned participants in advance that they might disclose an upsetting topic	*t*(137) = 1.143 *r* = −.097 *p* (two-tailed) = .52	*t*(137) = 1.143 *r* = −.097 *p* (two-tailed) = .26	*t*(89) = 0.547 *r* = −.058 *p* (two-tailed) = .58	*t*(29) = 1.499 *r* = −.268 *p* (two-tailed) = .144

Table 3 (*continued*)

Variable	Descriptive information	Overall effect size	Psychological health effect size	Reported health effect size	Subjective impact effect size
		Methodological information (*continued*)			
Timing of follow-up/ posttest: Less than vs. at least 1 month	23% of studies had follow-up periods of less than 1 month	***t*(144) = 1.682** ***r* = .139** ***p* (two-tailed) = .095**	***t*(110) = 2.292** ***r* = .213** ***p* (two-tailed) = .024**	*t*(93) = 0.673 *r* = .070 *p* (two-tailed) = .50	*t*(31) = 0.253 *r* = .045 *p* (two-tailed) = .80
		Treatment information			
No. disclosure sessions (less than vs. at least three sessions)	84% of studies had at least three disclosure sessions	***t*(144) = 1.299** ***r* = .108** ***p* (one-tailed) = .098**	***t*(110) = 1.430** ***r* = .135** ***p* (one-tailed) = .078**	*t*(83) = 0.184 *r* = .020 *p* (one-tailed) = .43	***t*(31) = 1.356** ***r* = .237** ***p* (one-tailed) = .092**
Length of disclosure sessions (less than vs. at least 15 min)	94% of studies had disclosure sessions that lasted at least 15 min	***t*(144) = 1.790** ***r* = .148** ***p* (one-tailed) = .03**	*t*(110) = 0.517 *r* = .049 *p* (one-tailed) = .30	***t*(93) = 2.116** ***r* = .214** ***p* (one-tailed) = .018**	Unable to test
Spacing of disclosure sessions (daily vs. weekly)	15% of studies had weekly disclosure sessions	*t*(97) = 0.346 *r* = −.035 *p* (two-tailed) = .72	*t*(72) = 0.886 *r* = −.104 *p* (two-tailed) = .38	*t*(70) = 0.251 *r* = −.030 *p* (two-tailed) = .80	*t*(22) = 0.644 *r* = −.136 *p* (two-tailed) = .52
Disclosure topic assigned (negative vs. positive)	85% of studies had participants disclose a negative topic	*t*(131) = 0.344 *r* = .030 *p* (two-tailed) = .73	*t*(102) = 0.332 *r* = .033 *p* (two-tailed) = .74	*t*(89) = 0.251 *r* = −.030 *p* (two-tailed) = .80	Unable to test
Months since topic/ event	On average, participants wrote about an event that occurred 15 months prior	***r*(60) = −.283** ***p* (one-tailed) = .013**	***r*(47) = −.323** ***p* (one-tailed) = .012**	***r*(36) = −.289** ***p* (one-tailed) = .040**	*r*(17) = .107 *p* (two-tailed) = .68
Prior disclosure of topic	35% of studies instructed participants to discuss an undisclosed topic	*t*(140) = 0.158 *r* = .013, *p* (one-tailed) = .44	***t*(106) = 1.570** ***r* = .151** ***p* (one-tailed) = .06**	*t*(91) = 0.229 *r* = −.050 *p* (two-tailed) = .82	*t*(31) = 1.266 *r* = .222 *p* (one-tailed) = .11
Directed questions or specific examples given	50% of studies gave directed questions or specific examples	***t*(137) = 1.609** ***r* = .136** ***p* (one-tailed) = .055**	***t*(106) = 2.737** ***r* = .257** ***p* (one-tailed) = .0035**	*t*(88) = 0.803 *r* = .085 *p* (one-tailed) = .21	*t*(31) = 0.623 *r* = −.111 *p* (two-tailed) = .54
Topic switching: no switch and OK to switch are better than no instructions	48% of studies did not give participants any instruction regarding topic switching	*t*(140) = 0.826 *r* = .070 *p* (one-tailed) = .20	***t*(107) = 1.390** ***r* = .133** ***p* (one-tailed) = .084**	*t*(90) = 0.024 *r* = −.003 *p* (two-tailed) = .98	*t*(29) = 0.884 *r* = .162 *p* (one-tailed) = .19
Focus of disclosure (standard instructions vs. cognitive change)	4% of studies gave instructions specifically designed to promote cognitive change, cognitive processing, or insight/meaning	*t*(114) = 0.429 *r* = −.040 *p* (two-tailed) = .68	*t*(114) = 0.429 *r* = −.040 *p* (two-tailed) = .66	*t*(72) = 0.449 *r* = −.053 *p* (two-tailed) = .66	Unable to test
Time reference of disclosure instructions (current is better than choice of current or past, which is better than past)	29% of studies instructed participants to disclose a current event	*t*(143) = 0.732 *r* = .061 *p* (one-tailed) = .23	*t*(109) = 1.067 *r* = .102 *p* (one-tailed) = .14	*t*(92) = 1.231 *r* = .127 *p* (one-tailed) = .11	*t*(30) = 0.352 *r* = −.064 *p* (two-tailed) = .73
Audience of disclosure (experimenter vs. no one)	92% of studies had participants turn in disclosure (experimenter was audience)	*t*(138) = 0.631 *r* = .054 *p* (two-tailed) = .529	***t*(106) = 1.795** ***r* = .172** ***p* (two-tailed) = .075**	*t*(88) = 0.980 *r* = −.104 *p* (two-tailed) = .330	*t*(30) = 0.322 *r* = .059 *p* (two-tailed) = .75
Mode of disclosure: writing vs. talking	4% of studies had participants disclose orally	*t*(139) = 0.029 *r* = .002 *p* (one-tailed) = .49	*t*(100) = 0.635 *r* = .063 *p* (one-tailed) = .26	*t*(84) = 0.584 *r* = .064 *p* (one-tailed) = .28	Unable to test

(*table continues*)

Table 3 (*continued*)

Variable	Descriptive information	Overall effect size	Psychological health effect size	Reported health effect size	Subjective impact effect size
		Treatment information (*continued*)			
Mode of disclosure: handwriting vs. typing	77% of studies had participants handwrite their disclosure	$t(127) = 0.042$ $r = .004$ p (one-tailed) = .48	$t(96) = 0.387$ $r = .039$ p (one-tailed) = .35	$t(84) = 0.969$ $r = .105$ p (one-tailed) = .17	$t(27) = 1.420$ $r = -.264$ p (two-tailed) = .17
		Study quality information			
Total quality rating (range = 0–4)	The average study quality rating was 2.94	$r(144) = -.115$ p (two-tailed) = .17	**$r(110) = -.188$** **p (two-tailed) = .048**	$r(93) = -.093$ p (two-tailed) = .39	$r(31) = .091$ p (two-tailed) = .62
Attrition rate (less than 20% attrition vs. 20% or more attrition)	75% of studies had less than 20% attrition	$t(144) = 0.754$ $r = .063$ p (two-tailed) = .45	$t(110) = 1.233$ $r = .117$ p (two-tailed) = .22	$t(93) = 0.149$ $r = .015$ p (two-tailed) = .88	$t(31) = 0.754$ $r = .134$ p (two-tailed) = .61
Mode of instruction administration (orally vs. in writing)	60% of studies administered group-specific disclosure instructions orally	$t(143) = 0.254$ $r = -.021$ p (two-tailed) = .80	$t(109) = 0.262$ $r = -.025$ p (two-tailed) = .80	$t(92) = 0.805$ $r = .084$ p (one-tailed) = .21	$t(31) = 0.904$ $r = -.160$ p (two-tailed) = .37
Control conditions (standard vs. empty)	12% of studies used an empty control group	$t(129) = 0.912$ $r = .080$ p (two-tailed) = .360	$t(97) = 1.161$ $r = .161$ p (two-tailed) = .110	$t(90) = 0.206$ $r = .022$ p (two-tailed) = .84	Unable to test
Participant expectation of study benefit	5% of studies created unequal expectations for treatment and control participants	**$t(143) = 2.428$** **$r = .199$** **p (two-tailed) = .016**	**$t(109) = 3.148$** **$r = .289$** **p (two-tailed) = .002**	$t(92) = 0.802$ $r = .083$ p (two-tailed) = .42	Unable to test

Note. Significant and marginally significant findings are in boldface.

correlation between the moderator and the outcome in the treatment group) was extracted and then averaged in the same way that overall effects were averaged earlier in this article. Because of the small number of studies examined in these within-study analyses (largest $k = 10$), significance was tested with a fixed effects model. Within-study analyses were done for the variables of stress level, physical health status, mood, neuroticism, alexithymia, optimism, and emotional inhibition. In addition, because a few studies had examined age and gender in a within-study fashion, both between-studies and within-study analyses were conducted for these two variables. Between-studies results are presented in Table 3 with the other moderator categories; within-study results are presented in Table 4.

No participant variables significantly moderated any of the effect size categories in the between-studies analysis. Across the 146 studies, the average participant age was 29 years old, 34% of participants were male, 72% of participants were Caucasian, 12% of participants were Black or African American, 7% of participants were Asian, 5% of participants were Hispanic or Latino, and the mean level of education of study participants was some college.

For the within-study analyses, stress was found to significantly moderate the effects of disclosure, such that higher stress participants showed greater benefits for overall effect size ($r = .102$) and reported health effect size ($r = .187$) but not for psychological health or subjective impact. Physical health status marginally moderated the effects of disclosure on reported health outcomes, such that people in poorer health were more likely to benefit from the intervention ($r = .101$), but did not moderate any of the other outcome categories. Optimism was also found to be a significant moderator for psychological health ($r = .340$) and reported health effect sizes ($r = .157$), such that pessimists benefited more from the intervention, but it did not moderate overall or subjective impact effect sizes. The rest of the within-study moderators (age, gender, mood, neuroticism, alexithymia, and emotional inhibition) did not moderate any of the effect size categories.

Methodological variables. Four variables related to the general conduct of the study were included in this category: number of participants, payment status of participants (whether participants were paid), predisclosure priming (whether participants had been warned that they might be asked to disclose an upsetting topic), and the timing of follow-up or posttest. The number of participants was not significantly related to the overall, reported health, or subjective impact effect size. However, the number of participants was marginally related to the psychological health effect size, such that studies with more participants had smaller effect sizes ($r = -.181$). Across the 146 studies, the average number of participants was 78.

Payment for study participation did not significantly moderate the effect of experimental disclosure for the overall, psychological health, or reported health effect size. However, studies in which participants were paid had significantly higher subjective impact effect sizes than studies in which participants were given no payment (paid, $r = .167$; unpaid, $r = -.006$). Predisclosure priming did not act as a significant moderator for any of the effect size types. Participants were paid in 73% of the studies, and participants were warned in advance that they might disclose upsetting topics in 19% of the studies.

Table 4
Within-Study Summary of Moderators (Fixed Effects)

Variable and prediction	Overall effect	Psychological health	Reported health	Subjective impact
Age (no specific prediction)	Mean $r = .014$ $p = .94$[a] $N = 239$ $k = 3$	Mean $r = .083$ $p = .56$[a] $N = 52$ $k = 1$	Mean $r = -.051$ $p = .42$[a] $N = 171$ $k = 2$	Mean $r = -.066$ $p = .32$[a] $N = 187$ $k = 2$
Gender (men should benefit more)	Mean $r = -.020$ $p = .52$[a] $N = 1{,}117$ $k = 10$	Mean $r = .019$ $p = .39$ $N = 218$ $k = 4$	Mean $r = -.033$ $p = .44$[a] $N = 952$ $k = 6$	Mean $r = -.009$ $p = .88$[a] $N = 187$ $k = 2$
Stress (those with high stress should benefit more)	**Mean $r = .102$** **$p = .018$** **$N = 532$** **$k = 8$**	Mean $r = .075$ $p = .13$ $N = 446$ $k = 7$	**Mean $r = .187$** **$p = .0012$** **$N = 296$** **$k = 4$**	
Physical health (those with poor health should benefit more)	Mean $r = .042$ $p = .25$ $N = 512$ $k = 8$		**Mean $r = .102$** **$p = .089$** **$N = 367$** **$k = 5$**	Mean $r = .116$ $p = .14$ $N = 112$ $k = 2$
Mood (those with negative mood should benefit more)	Mean $r = .015$ $p = .38$ $N = 787$ $k = 5$	Mean $r = .000$ $p = .50$ $N = 50$ $k = 1$	Mean $r = .038$ $p = .27$ $N = 660$ $k = 2$	
Neuroticism (neurotics should benefit more)	Mean $r = .062$ $p = .13$ $N = 934$ $k = 6$		Mean $r = -.037$ $p = .46$[a] $N = 896$ $k = 5$	
Alexithymia (those low in alexithymia should benefit more)	Mean $r = .035$ $p = .34$ $N = 349$ $k = 7$	Mean $r = .078$ $p = .15$ $N = 210$ $k = 5$	Mean $r = .029$ $p = .47$ $N = 246$ $k = 5$	
Optimism (pessimists should benefit more)	Mean $r = .081$ $p = .17$ $N = 239$ $k = 4$	**Mean $r = .340$** **$p = .016$** **$N = 40$** **$k = 1$**	**Mean $r = .157$** **$p = .097$** **$N = 162$** **$k = 2$**	
Emotional inhibition (those high in inhibition should benefit more)	Mean $r = -.031$ $p = .82$[a] $N = 317$ $k = 4$	Mean $r = .084$ $p = .11$ $N = 151$ $k = 2$	Mean $r = -.080$ $p = .38$[a] $N = 125$ $k = 1$	

Note. A negative effect size indicates that the moderator was in the opposite direction than predicted. Significant ($p < .05$) and marginally significant ($p < .10$) findings are in boldface.
[a] p value is two-tailed.

The timing of the follow-up or posttest measures (number of months between disclosure and posttest) moderated the effect of experimental disclosure for the overall effect size (less than 1 month, $r = .111$; at least 1 month, $r = .064$) and psychological health effect sizes (less than 1 month, $r = .110$; at least 1 month, $r = .035$), such that studies that followed participants for less than a month after disclosure had larger effect sizes than studies that followed participants for at least a month. The timing of follow-up did not significantly moderate the effect of treatment on reported health or subjective impact effect sizes. The average follow-up time was approximately 3 months after disclosure.

Treatment variables. Twelve variables related to specific differences in the conceptualization and administration of experimental disclosure were included in this category: dose-related variables (number of disclosure sessions, length of disclosure sessions, spacing of disclosure sessions), topic-related variables (valence of disclosure topic, months since trauma or topic, prior disclosure of topic), instruction-related variables (focus of disclosure instructions, time reference of disclosure instructions, presence or absence of directed questions or specific example of what to disclose, and instructions regarding topic switching), audience of disclosure, and mode of disclosure (hand writing, typing, or talking).

The number of disclosure sessions moderated the effect of experimental disclosure for the overall effect size (fewer than three sessions, $r = .040$; at least three sessions, $r = .082$), psychological health effect size (fewer than three sessions, $r = .007$; at least three sessions, $r = .063$), and subjective impact effect size (fewer than three sessions, $r = .019$; at least three sessions, $r = .173$), such that studies with three or more sessions had marginally larger effect sizes than studies with fewer than three sessions. This variable was not a significant moderator for the reported health effect size. The length of disclosure sessions moderated the overall effect size (less than 15 min, $r = -.007$; at least 15 min, $= .080$) and the reported health effect size (less than 15 min, $r = -.132$; at least 15 min, $r = .078$), such that studies with sessions that lasted at least 15 min had significantly larger effect sizes than studies with sessions that

lasted less than 15 min. Length of session did not significantly moderate the effect of psychological health; this variable could not be tested for the subjective impact effect size, because only one study in this group contained sessions that lasted for less than 15 min. Spacing of disclosure sessions was not found to be a significant moderator for any of the effect size types. Of the 146 studies, the average study had four 20-min disclosure sessions, 53% of which were scheduled on consecutive days.

Valence of the writing topic did not significantly moderate the overall, psychological health, or reported health effect size; this variable could not be tested for subjective impact because all studies in this group had participants write about negative topics. Months since trauma or topic significantly moderated the effect of experimental disclosure for the overall effect size ($r = -.283$), psychological health effect size ($r = -.323$), and reported health effect sizes ($r = -.289$), such that studies in which participants wrote about more recent traumas or topics had larger effect sizes. This variable did not significantly moderate the effect for subjective impact of the intervention. Prior disclosure of the topic was not found to be a significant moderator for the overall effect size, reported health effect size, or subjective impact effect size. However, studies in which participants were instructed to discuss previously undisclosed topics did have marginally larger psychological health effect sizes (undisclosed, $r = .092$) than studies in which participants were not given this instruction (no instruction, $r = .042$). Of the 146 studies, 86% had participants write about negative topics that occurred an average of 16 months before disclosure, and 35% of studies specifically instructed participants to discuss an undisclosed topic.

The presence or absence of directed questions in the instructions or specific examples of what to disclose moderated the effect of experimental disclosure for the overall effect size (directed questions, $r = .090$; no directed questions, $r = .052$) and for the psychological health effect size (directed questions, $r = .094$; no directed questions, $r = .011$), such that studies that gave participants directed questions or examples had marginally (for overall) or significantly (for psychological health) larger effect sizes than studies that did not give directed questions or examples. This variable did not significantly moderate the effect for reported health or subjective impact. Instructions regarding topic switching within or between sessions did not significantly moderate the effect for overall, reported health, or subjective impact effect sizes. However, the hypothesis stating that studies giving any instruction regarding topic switching would have larger psychological health effect sizes than studies giving no instruction was marginally supported by a planned contrast, $t(107) = 1.39$, $r = .133$ (with instruction, $r = .075$; without instruction, $r = .038$). The remainder of the instruction-related variables did not significantly moderate the effect for any of the outcome types. Of the 146 studies, 4% of studies gave participants instructions designed to promote cognitive processing or insight, 52% of studies instructed participants to disclose a past event, 50% of studies gave participants directed questions or examples of what to disclose, and 48% of studies did not give participants any instruction regarding topic switching.

Audience of disclosure (no one will hear or read vs. experimenter will hear or read) did not significantly moderate the effect of experimental disclosure for the overall, reported health, or subjective impact effect size. However, studies in which participants did not turn in their disclosure (no one will hear or read) had marginally higher psychological health effect sizes than studies in which participants turned in their disclosure (did not turn in, $r = .178$; turned in, $r = .050$). The mode of disclosure (hand writing, typing, talking) did not significantly moderate the effect for any of the outcome types. Of the 146 studies, 92% of studies had participants turn in their disclosure, and 77% had participants hand write their disclosure.

Effect of Study Quality

Each study was given a quality rating based on study attrition, experimenter blinding, type of control group, and equality of participant expectation; the score given to each study is listed in Table 1. Most studies had at least one quality problem, with the mean quality rating being 2.94 out of 4.00 (range = 0–4). To assess the impact of study quality on effect size, I examined the relation between quality score and the overall, psychological health, reported health, and subjective impact effect sizes (see Table 2). Although there were no significant relations between the study quality rating and the overall, reported health, or subjective impact effect sizes, there was a significant negative relation between quality rating and the psychological health effect sizes, such that lower quality studies tended to have higher effect sizes. To further examine the relations between aspects of study quality and effect size, I examined each of the four aspects of quality separately in relation to its association with effect sizes. *t* tests comparing effects sizes of studies with low versus high attrition, blinded versus unblinded experimenters, standard versus empty control groups, and equal versus unequal participant expectations revealed significant differences only for equal versus unequal participant expectations. Compared with studies in which groups had equal expectations about the benefits of participation, studies in which groups had unequal expectations had significantly higher overall effect sizes (equal expectations, $r = .068$; unequal expectations, $r = .191$) and psychological health effect sizes (equal expectations, $r = .045$; unequal expectations, $r = .240$).

Although study quality (and expectation equality, in particular) was related to study effect size, the bias introduced by this relation was relatively small. When the meta-analysis was limited to only studies with higher quality scores (higher than 2), the mean effect sizes for overall, psychological health, reported health, and subjective impact remained virtually the same as when all studies were included. In particular, the *r*-effect sizes for the four categories when all studies were included were .075, .056, .072, and .159 for the overall, psychological health, reported health, and subjective impact effect sizes, respectively. When the poorer quality studies were excluded ($k = 35$), the *r*-effect sizes for the four categories changed to .064, .039, .056, and .163, respectively. Although the mean effect for three of the categories did get slightly smaller with the exclusion of poorer quality studies, all four average effects remained highly significant (ps = .000043, .011, .0025, and .00025, respectively). Similarly, computing weighted means (in which the effect sizes are weighted by the quality score) gave only slightly different results than computing unweighted means: overall weighted $r = .070$ (vs. unweighted $r = .075$), psychological health weighted $r = .047$ (vs. unweighted $r = .056$), reported health weighted $r = .068$ (vs. unweighted $r = .072$), and subjective impact weighted $r = .163$ (vs. unweighted $r = .159$).

Multivariate Analysis of Moderators

The intercorrelations among the 22 variables that marginally or significantly moderated the effects of experimental disclosure are displayed in Table 5. Because so many of the moderators were correlated to each other, the 22 moderators were subjected to a principal-components analysis with step-up rotation. This was done for the purposes of data reduction, to create composite variables that could then be tested as moderators. Unfortunately, although the variables could be statistically forced into either a two-factor or a three-factor solution, the variables in each factor did not group together in a conceptually meaningful or interpretable way in either case. Therefore, this line of analysis was not pursued further.

Discussion

The results of this meta-analysis confirm that experimental disclosure does have beneficial effects for participants, with an overall (unweighted) *r*-effect size of .075. Because the analysis was conducted with a random effects approach, one can have confidence in the true existence of this effect even for similar studies that were not included in the present analysis as well as for similar studies that might be conducted in the future. Furthermore and perhaps more important, a number of moderators of experimental disclosure were identified with a random effects approach; effect sizes tended to be larger when studies included only participants with physical health problems, included only participants with a history of trauma or stressors, did not draw from a college student sample, had participants disclose at home, had participants disclose in a private setting, had more male participants, had fewer participants, paid the participants, had follow-up periods of less than 1 month, had at least three disclosure sessions, had disclosure sessions that lasted at least 15 min, had participants who wrote about more recent events, instructed participants to discuss previously undisclosed topics, gave participants directed questions or specific examples of what to disclose, gave participants instructions regarding whether they should switch topics, and did not collect the products of disclosure. Conversely, a number of variables that were originally hypothesized to moderate experimental disclosure were not significantly related to effect size: psychological health selection criteria, participant age, participant ethnicity, participant education level, warning participants in advance that they might disclose traumatic events, spacing of disclosure sessions, valence of disclosure topic, focus of disclosure instructions, time reference of disclosure instructions, and mode of disclosure (hand writing, typing, or talking).

The Overall Effect

The overall average effect size obtained in this analysis, .075 (Cohen's $d = .151$), is somewhat smaller than the average effect sizes of .257 found in the Smyth (1998) analysis and .084 found in the Frisina et al. (2004) analysis. This may be because the present study included a much higher proportion of unpublished studies in the analysis (48% unpublished) compared with the Smyth (1998) analysis (23% unpublished) and the Frisina et al. (2004) analysis (0% unpublished); unpublished studies tend to have smaller effect sizes, thus lowering the overall average effect. Although the present average effect size is somewhat smaller than expected on the basis of previous analyses, the effect should nevertheless still be considered important, and experimental disclosure should still be considered a worthwhile activity. Some may argue that an effect of .075 is considered to be quite small by traditional standards (e.g., conventions by Cohen, 1988), as it accounts for only 0.56% of the variance in the measured outcomes. However, even Cohen (1988) himself stated that "there is a certain risk in offering conventional operational definitions for [the terms of *small, medium,* and *large*] for use in power analysis in as diverse a field of inquiry as behavioral science" (p. 112). Rather than relying on Cohen's conventions, researchers have argued that the practical importance of an effect depends entirely on its relative costs and benefits (Glass, McGaw, & Smith, 1981). When one considers that the act of disclosing has virtually no costs—it is a free, noninvasive, independent activity and is perceived by participants to be helpful—it seems that any effect that is nonzero and in the positive direction is worth noting (see Prentice & Miller, 1992).

The importance of an effect size of .075 can also be understood in the context of a binomial effect size display (R. Rosenthal, 1991). The binomial effect size display can be used to interpret the impact of an effect size in the context of a hypothetical study with 100 treatment participants and 100 control participants. In this hypothetical study, it is assumed that if the null hypothesis were true, half of the participants would improve and half would not improve, regardless of treatment assignment, such that there would be 50 treatment participants who improved, 50 control participants who improved, 50 treatment participants who did not improve, and 50 control participants who did not improve. Given this assumption, when there is an effect size of .075, instead of 50 participants in each cell, there would be 54 participants in the treatment/improved cell, 46 participants in the treatment/not improved cell, 46 participants in the control/improved cell, and 54 participants in the control/not improved cell. In other words, in this hypothetical study with an effect size of .075, 54% of treatment participants would improve, whereas only 46% of control participants would improve.

In addition, when one is evaluating the size of an effect, it is important to compare the effect size with other effect sizes in a related research domain. Because a number of the outcome measures of experimental disclosure are in the domain of physical health, it would be appropriate to compare the effect size of .075 with other influential and important effect sizes in the medical literature. As an example, consider the act of taking a daily aspirin after a heart attack to prevent death from a second heart attack: This treatment is widely regarded in the medical community as extremely valuable, and it has a *r*-effect size of .034 (Rosenthal, 1994), less than half of the effect size found for experimental disclosure. Similarly, because a number of experimental disclosure studies have been conducted on college students and some of the outcomes were measures of scholastic achievement, it would also be appropriate to consider the size of the effect in light of the educational literature. In particular, educators have recently argued that effect sizes as small as .050 are reasonable effects to expect in educational research and, although small, are nonetheless important in terms of improvements in learning and achievement (Lanahan, McGrath, McLaughlin, Burian-Fitzgerald, & Salganik, 2005); our *r*-effect size of .075 is even a bit higher than the reasonable and important effect of .050.

Table 5
Correlations Among (Between-Study) Moderator Variables

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	—																		
2	−.118	—																	
3	−.087	.556*	—																
4	−.016	−.376*	−.408*	—															
5	−.137†	−.204*	.095	−.300*	—														
6	.009	−.099	−.199	−.097	−.059	—													
7	−.086	−.006	−.151†	.284*	−.110	−.129	—												
8	.044	−.328*	−.564*	.276*	−.058	.207*	.001	—											
9	−.069	−.015	−.140†	.346*	−.126	.015	−.046	.147†	—										
10	.088	−.141†	−.166*	.251*	−.113	.021	−.008	.208*	.444*	—									
11	.144†	−.170	−.090	.084	−.156	.002	.052	.121	−.041	−.105	—								
12	−.066	−.103	−.144†	.315*	−.121	.009	.057	.088	.431*	.287*	−.048	—							
13	−.084	−.030	−.140†	.121	−.031	.113	.041	.139†	.267*	.069	−.023	.179*	—						
14	.131	.033	−.169	−.033	.005	−.330*	−.120	−.168	−.075	−.147	−.035	−.001	.040	—					
15	−.321*	.006	−.129	.017	.066	−.022	.128	.105	.050	.016	−.041	.023	.087	−.026	—				
16	−.022	−.063	−.046	.070	−.078	−.020	.132	−.143†	−.058	−.048	.213*	.138	.050	.128	.139†	—			
17	.039	.020	.200*	−.162†	.104	−.051	−.092	−.140	−.092	−.094	.116	−.176*	−.153†	.066	−.016	.060	—		
18	−.060	−.300*	−.077	.014	−.092	.165†	−.268*	.067	.082	.069	.085	.029	−.073	−.225*	−.250*	.091	−.007	—	
19	−.060	.182*	.271*	.057	.128	.030	−.132	−.22*	−.133	−.134	.001	−.100	−.061	.044	−.043	−.065	.042	.026	—

Note. 1 = publication status (0 = no, 1 = yes); 2 = percentage of participants for whom physical health was not an eligibility criterion; 3 = percentage of participants who were drawn from a student sample; 4 = location of disclosure (0 = controlled setting, 1 = home); 5 = percentage of sessions in which participants disclosed in a room with others who were also disclosing; 6 = percentage of male participants; 7 = percentage of participants lost to follow-up (attrition); 8 = percentage of participants who received no reimbursement; 9 = participant expectation of study benefit (0 = equal, 1 = unequal); 10 = percentage of control participants who were part of an empty control; 11 = timing of follow-up (weeks); 12 = number of disclosure sessions; 13 = length of disclosure sessions (minutes); 14 = months since topic/event; 15 = percentage of participants who received examples/directed questions; 16 = instructions regarding topic switching (0 = no instruction; 1 = okay to switch or don't switch); 17 = percentage of participants that turned in their disclosure/had an audience; 18 = percentage of participants for which trauma/stressor history was not an eligibility criterion; 19 = number of participants in the study.
† $p < .10$. * $p < .05$.

Because experimental disclosure is thought of as a psychotherapeutic activity, it also seems important to compare its effect size with that of psychotherapy. M. L. Smith and Glass (1977), in a review of approximately 500 studies on psychotherapy, found the equivalent of a *r*-effect size of psychotherapy to be about .322. Clearly, this is quite a bit larger than the effect size of .075 that was found in the present study for experimental disclosure. However, given that psychotherapy typically takes place for 1 hr per week over the course of several months (sometimes years) and is conducted by a therapist who has had many years of education and training, of course it should be the case that spending only 20 min a day for 3 days on an independent writing (or talking) activity should have an effect size that is quite a bit smaller than months of time-consuming and expensive psychotherapy. To have arrived at any other result should cause the reader to be suspicious. Indeed, it does seem quite impressive that an intervention that is so easy (requires one only to write or talk), so brief (a total of about an hour), so cost efficient (completely free), and so well received by participants (most participants enjoy it or report it to be helpful) can improve so many facets of a person's life (psychological, physical, social, academic), even if it is considered a very small improvement by conventional standards.

Finally, one additional note should be made in the interpretation of this average *r*-effect size of .075. Although I have argued that the size of the effect is indeed meaningful and important in practical terms, an even more important argument to consider is that this effect is, of course, an average. It includes studies in which the intervention was administered in less than optimal conditions and even in conditions under which the disclosure was harmful (e.g., disclosure sessions shorter than 15 min). Indeed, if one considers, for instance, the eight studies that administered the disclosure intervention under a majority of optimal conditions (e.g., high dosage, privacy during sessions, specific disclosure instructions), the average effect size of those eight studies was .200; this offers preliminary evidence that, when administered correctly, experimental disclosure may have an effect that, even by conventional standards, is considered halfway between small and medium. Rosenthal (1994) made a general warning against putting too much emphasis on an average effect when the range of effect sizes includes both positive and negative effects (as it does in this case), and Sloan and Marx (2004b) specifically argued that computing an overall average effect size of experimental disclosure "seems inappropriate given the substantial degree of methodological variation across studies" (p. 133). Instead, when such heterogeneity of studies exists, it is advisable to instead focus on the variables that moderate the effect and the conditions under which the interventions work well, a task that is the focus of most of the remainder of the discussion.

Effects of Outcome Categories

All major outcome types were found to significantly improve as a result of experimental disclosure, except health behaviors. These findings confirm those of Smyth (1998), who also found health behaviors to be the only outcome category with no improvement. Health behavior change may be more dependent on cognitive factors than on emotional factors, such as perceived risk and assessment of costs and benefits (Rosenstock, 1966); this may explain the null findings for this category. Although it is possible that experimental disclosure is a useful component of a behavior change program, it is apparently not sufficient to promote change in the absence of additional, more cognitively oriented intervention components.

Although the psychological health subcategories of distress, depression, subjective well-being, anger, and anxiety were shown to improve as a result of experimental disclosure, there is insufficient evidence at this time to conclude that experimental disclosure has any effect on outcomes related to grief/bereavement, stress, coping strategies, cognitive schemas, stress-related growth (e.g., becoming more spiritual as a result of a stressful or traumatic event), eating-disorder-related problems, or dissociative experiences. Null effects for grief/bereavement might have been due to measurement problems, such as the use of unreliable or poor measures of grief (Range et al., 2000), or due to a moderating variable; experimental disclosure may only be helpful for grief associated with stigma, such as suicidal grief (Kovac & Range, 2000). Null effects for stress are consistent with the suggestion by Lepore (1997) that experimental disclosure may not reduce one's stress (e.g., frequency of intrusive thoughts) but may instead reduce the impact of the stress (e.g., intrusive thoughts will not lead to depression); participants may still view their life as stressful but may be more able to handle the stress in a healthy and productive way. Cognitive schemas and use of coping strategies may be unaffected by experimental disclosure because global worldviews and one's use of coping strategies tend to be relatively stable across time and situations (David, 1998; Janoff-Bulman, 1992) and are therefore likely to be somewhat resistant to change. The failure of experimental disclosure to increase stress-related growth is puzzling in light of the theory that argues that disclosure promotes cognitive processing, insight, and understanding. An examination of differences in the methodology between the one study that did find significant effects for stress-related growth (Ullrich & Lutgendorf, 2002) and the three studies that did not find significant effects (Rivkin, Gustafson, Weingarten, & Chin, 2004; Williams-Avery, 1999; Wilson, 2000) revealed that the successful study had a very short follow-up period (1 day after disclosure), whereas the other studies had longer follow-up periods (5 to 10 weeks after disclosure). It may be the case that experimental disclosure speeds up stress-related growth: Treatment participants obtain it more quickly, but control participants do obtain it eventually. If this were the case, this effect would be missed if it were measured too long after disclosure. Finally, eating-disorder-related problems and dissociative experiences may be too severe to be solved from such a brief intervention. Overall, it appears that experimental disclosure is helpful for psychological health outcomes that are more directly related to emotions (e.g., depression, positive functioning) than to cognitions (e.g., cognitive schemas, body image disorder).

Physiological functioning subcategories of immune parameters, HIV viral load, liver function, and dopamine were improved by experimental disclosure, but there is currently insufficient evidence to conclude that experimental disclosure is helpful for any of the following measures of physiological functioning: blood glucose, blood lipids, lung function, blood pressure, stress-related measures, body composition, heart-related measures, strength, or joint condition. It appears that some measures of physiological functioning are more amenable to experimental disclosure than others; this may be a function of how closely related the outcome

measures are to the immune system. Immune parameters are believed to be affected by psychological health (Vollhardt, 1991); similarly, associations have been made between psychological health and other measures that are closely related to the immune system, such as HIV viral load levels (D. N. Taylor, 1995), liver function measures (Wu et al., 2000), and dopamine levels (Nankova et al., 2000). Therefore, it makes good sense that an intervention that improves psychological health should, in turn, improve these specific, immune-related measures. However, more general measures of disease status, such as blood glucose (diabetes), lung function (asthma), joint condition (arthritis), blood pressure (hypertension), and blood lipids (high cholesterol), were not improved by experimental disclosure; this pattern of results has been found in other psychological interventions, which have influenced the immune system but have had no clinical impact on the development and course of more serious chronic diseases (Cohen & Herbert, 1996; Petticrew, Bell, & Hunter, 2002). The failure of experimental disclosure to affect stress-related measures of physiological functioning (e.g., cortisol) is not surprising in light of the previously mentioned finding that the intervention does not reduce participants' perceived level of stress. Some of the other measures that were unaffected by experimental disclosure (heart, body composition, and strength) may show null effects because these measures require behavior change; for example, resting heart rate, weight, and the ability to do sit-ups are all strongly influenced by exercise habits. Given the finding that experimental disclosure does not elicit health-related behavior change, it is not as surprising that it also does not affect outcomes that require behavior change.

Among the subcategories of reported health, disease-specific outcomes and illness behaviors were found to be significantly improved in a random effects analysis. It is interesting that even though disclosure did not affect many objectively measured indicators of disease status, self-reported measures of disease activity were improved. For whatever reason, perhaps because they have improved psychological health, patients feel that they are doing better, even though lab results might indicate otherwise. Unlike the other two reported health subcategories, the general physical symptoms subcategory, which was measured in 4,847 participants across 59 studies, was only found to approach significance in a fixed effects approach. If there is a true effect of general physical symptoms, it likely either is quite close to zero or is affected by moderator variables (e.g., the baseline health of participants or the actual scale used to measure the symptoms). The baseline health of participants across studies did vary quite widely, as did the choice of scales used to measure symptoms.

Among the subcategories of subjective impact of the intervention, treatment participants in experimental disclosure studies were significantly more likely to have a positive attitude about the intervention and to attempt to process or make sense of the event after the intervention than control participants, as examined in a random effects approach. The fact that participants perceived this activity to be a positive experience and that they believed it to be helpful is important, even aside from any actual benefit the participant received. If people do not believe that an activity is useful or beneficial (even if it is), they will not perform it or recommend it to others. Other results indicate that treatment participants did not differ from control participants in ratings of whether the intervention had no effect or of whether they had a negative attitude about the intervention. This might have been due to a range restriction on these measures; treatment participants likely scored low on these measures (because they thought the intervention was helpful), and control participants also likely scored low on these measures (because, although the intervention was not helpful for them, it was also not harmful).

Finally, among the subcategories of general functioning, outcomes concerning work, social relationships, cognitive functioning, and school were either marginally or significantly improved by experimental disclosure in a random effects analysis. There was insufficient evidence, however, to conclude that the outcomes related to law and forensics or goals were affected by experimental disclosure. Although tests of mediation have yet to be thoroughly conducted in this literature, it seems plausible that the improved psychological and physical health that results from disclosure could lead to improved functioning in the social, work, school, and cognitive domains. Conversely, outcomes related to legal behaviors, such as getting a traffic ticket or inmate behavior, may be more determined by situational or cognitive factors or by stable personality characteristics (Bianchi & Summala, 2004; Jiang, 2005) and may require a more multifaceted intervention. Finally, with respect to goal-related outcomes, it has been suggested that "a few sessions of solitary disclosure may not be sufficient to induce the level of cognitive processing necessary for provoking changes in intrinsic goals and values in a systematic way" (Bower et al., 2003, p. 152).

Moderators of the Effect

A number of moderators of experimental disclosure were identified in this analysis. Before I discuss the specifics of each of the identified moderators, it is important to note the limitations of moderator analyses of this type; because I have not randomly assigned studies to different conditions or to different levels of the moderator, I do not have the ability to infer cause. Although questions of directionality are not an issue in this study (it is clear that—except with some of the report information variables—the moderator variable always came before the outcome of the study), the effect of a third variable cannot be ruled out. The observation that many of the moderator variables were correlated to each other highlights this possibility; in addition, study variations not examined in this analysis also might have acted as third variables. Findings of moderators in these analyses, therefore, should be viewed as a starting point for future testing rather than a known determinant of the effect. Because the best way to test for moderators is to examine them in a single study, a brief description of primary studies that have also tested the moderators examined here is presented and compared with the present results, when applicable.

Report information variables. As predicted, published studies had larger effect sizes; as discussed earlier, this was likely due to a bias in the field to publish studies with large effect sizes and smaller p values.

Setting variables. As expected, studies using participants with a physical health problem had larger reported health effect sizes; studies with healthy participants are likely subject to floor effects for reported health outcomes, as participants in these studies may be scoring at the lowest levels of illness at pretest. Because physically healthy participants are not necessarily likely to be at

the lowest levels of psychological illness and are therefore not subject to floor effects on psychological health outcomes, it is not too surprising that reported health was the only outcome type that was moderated by this inclusion criterion. In the existing literature, there is only one study that has directly compared physically ill and physically healthy treatment participants within a single experimental disclosure study (Levey-Thors, 2000); in this study, physically ill participants were not found to benefit more than healthy participants on doctor's visits (a reported health outcome). However, this study also failed to find a main effect for disclosure, which makes the conclusions unclear. Future researchers should examine this potential moderator in more detail.

As predicted, studies using participants with a history of trauma or stressors had larger subjective impact effect sizes; studies without this inclusion criterion might have contained participants who did not have upsetting events to disclose and who became irritated or bored with the activity, prompting lower scores on the subjective impact measures. It is interesting that trauma history inclusion criteria did not moderate the other outcome types; although participants in the more inclusive studies might not have enjoyed the intervention quite as much, it still appeared to be helping them just as much. To my knowledge, no studies to date have directly compared treatment participants with and without a history of trauma or stressors in a single experimental disclosure study; this may be a valuable avenue for future exploration.

An unexpected finding is that studies that used college student samples had smaller psychological health effect sizes than studies that recruited from a more general population. This is in direct contrast to the analysis by Smyth (1998), who found that studies with college students had larger psychological health effect sizes. This finding might be understood in the context of a potential third variable, location of disclosure sessions. In the 13 studies analyzed by Smyth (1998), there was virtually no variation among studies regarding location of disclosure sessions (all but one study had all participants disclose in a controlled setting). However, in the present set of studies, not only did studies vary on disclosure location, this variable was related to both the use of student samples and psychological health effect sizes. Student samples were more likely to disclose in a controlled setting (vs. at home), and disclosure in a controlled setting was related to smaller psychological health effect sizes. When I controlled for the location of disclosure sessions, use of college students as a moderator no longer approached significance ($r = -.078$, $p = .44$). It appears, then, that use of college students may not offer any differential benefit for psychological health. At this time, however, no known studies have directly compared student versus nonstudent treatment participants in a single experimental disclosure study.

Using psychological health as an inclusion criterion did not moderate the effect of experimental disclosure; this was contrary to predictions. This might have occurred for two reasons. First, many psychological health outcomes are not as subject to floor effects as are reported health outcomes; even participants who do not have a diagnosis of a psychological problem could easily be experiencing mild symptoms of depression, distress, anxiety, and so forth, which would still allow room for improvement. Indeed, mood and anxiety problems tend to go undiagnosed quite often (Hendrick, 2003; Schneier, 2003). Psychological health outcomes may also be less subject to floor effects because a number of measures, especially measures related to subjective well-being, allow a good deal of room for improvement even among well-adjusted people (e.g., a psychologically healthy participant can still become more satisfied with his or her life). Second, not all psychological health problems may be equal regarding their interaction with experimental disclosure; this treatment may be especially helpful for people with certain diagnoses, whereas it should be avoided by people with other diagnoses. For example, one study with participants who were selected for being high in distress had a positive effect size (treatment participants improved; Laguna, 1998), but another study with participants who were selected for having negative body image had a negative effect size (treatment participants got worse; Earnhardt et al., 2002).

As previously mentioned, studies in which participants disclosed at home had larger psychological health effect sizes than studies in which participants disclosed in a controlled setting; this is contrary to expectations, as controlled settings were thought to offer greater compliance monitoring and less room for error (which should lead to larger effect sizes). It may be, however, that affording participants the opportunity to disclose at home allows them to be more comfortable and relaxed and ultimately become more engaged in the disclosure process. Engagement in psychotherapeutic treatment does tend to be greater in home-based programs than in office-based programs (Slesnick & Prestopnik, 2004), and greater involvement in the experimental disclosure process has been associated with better outcomes (Lutgendorf, Antoni, Kumar, & Schneiderman, 1994). However, some researchers have cautioned against home-based administration of disclosure, arguing that the uncontrolled exposure to traumatic memories that could occur when participants have the ability to reread essays or have unstructured sessions may cause more short-term distress than is necessary (Sheffield et al., 2002). Although at least one study has had some participants who wrote at home, whereas others wrote in the lab (Hughes, 1994), these disclosure locations have not been randomly assigned, and their impact has not been examined in a single study. A full examination of the costs and benefits associated with using a home-based administration of disclosure should be made before this approach is recommended as standard procedure.

The final setting variable examined was the privacy conditions of the disclosure sessions; studies that afforded participants greater privacy during disclosure had larger overall and psychological health effect sizes, as hypothesized. The rationale for this finding is similar to that of the impact of home disclosure sessions: Participants should feel more comfortable (and hence more able to get fully involved in the disclosure process) when they are disclosing in private; this increased level of involvement should improve effectiveness of the treatment. It is interesting to note that both location and privacy of disclosure sessions were moderators for psychological health outcomes but not for reported health or subjective impact outcomes. Psychological heath outcomes appeared to be more sensitive than other outcome types to feeling at ease and getting fully involved in the disclosure process.

Participant variables. In contrast to the finding by Smyth (1998), studies with higher proportions of men were not significantly more likely to have higher overall psychological health, reported health, or subjective impact effect sizes. The results of this between-studies analysis are echoed by the results of the within-studies analysis, which also did not find gender to moderate the effect of the intervention. Indeed, of the nine studies that have

tested gender as a moderator within their own study, seven of them found no significant Gender × Treatment effects (Booth et al., 1997; Donnelly & Murray, 1991; Kelley et al., 1997; Rivkin et al., 2004; Russ, 1992; Sheese, Brown, & Graziano, 2004; Van Middendorp, 2004), and the other two found that women benefited more from treatment (Crow, 2000; Pennebaker et al., 1990). Future studies should continue to examine the interaction between gender and treatment, given that most studies do include both men and women and given the ease with which this analysis can be performed. If researchers routinely report the effect size of this Gender × Treatment interaction in their reports, a larger meta-analysis to determine the average Gender × Treatment effect size could be readily conducted in the near future.

No other participant variables that were examined in a between-studies analysis were found to marginally or significantly moderate the effect size of experimental disclosure; studies with varying compositions of ethnicities, ages, and education levels had similar effect sizes. It is somewhat surprising that studies with larger proportions of Asians (or Asian Americans) did not have larger effect sizes, as it was expected that ethnicity would act as a moderator for similar reasons that it was expected that gender would act as a moderator. A possible reason for this null finding is that there was relatively little variation across studies with respect to ethnicity. Most participants in these studies were Caucasian (72%), with many studies (34%) containing no Asians, no studies containing all Asians, and only one study containing more than 50% Asians. This lack of variability makes it difficult to find significant effects. In the one study that did examine the differential effects of experimental disclosure for Asians versus Caucasians within a single study, Asians were more likely than Caucasians to reduce negative affect (shame) and physical symptoms after experimental disclosure (Rivkin, 2000). Future studies should make an effort to include more minority participants, allowing for a closer examination of the potential impact of ethnic differences.

Results of within-study analyses revealed that, as predicted, participants who were higher in stress, poorer in physical health, and lower in optimism were more likely to benefit from experimental disclosure. However, an unexpected finding was that mood, neuroticism, alexithymia, and emotional inhibition were not significant moderators of the effect. Null effects for these within-study analyses must be interpreted with caution for two reasons: First, only a handful of studies examined these variables, which makes effects more difficult to detect; second, in many of the study reports, a failure to find a significant interaction was simply reported as "no significant interactions were found," with no additional data to compute an accurate effect size. In those cases, the average effect was estimated to be zero, which is a conservative procedure. It could easily have been the case that each individual study did not have the power to detect a significant interaction even though such an interaction did exist, especially because most studies had fewer than 100 participants. For example, in one study that examined alexithymia as a moderator, the authors reported that alexithymia significantly moderated a subscale of arthritis at one measurement period with a small to medium effect size ($r = .220$) but reported only nonsignificant interactions (estimated to be zero) for each of the three other subscales at two different measurement periods. In the computation of the mean effect of the moderator, then, the effect size of .22 was averaged with seven zeros, which created a very small (and nonsignificant) average effect size of .022. Future studies should report enough information to compute the effect size of every interaction, significant or not (e.g., exact F or p values, or means and standard deviations), so more accurate mean effect sizes can be estimated.

Methodological variables. An unexpected finding was that there was a negative relation between the number of participants in a single study and the psychological health effect size. This finding likely indicates that my sample of 146 studies was slightly biased in that I was missing some studies that had a small effect size and a small sample size. Studies with a small effect size and a small sample size are less likely to get published and circulated; even though a great effort was made to obtain unpublished studies, one cannot completely avoid the problem of unsuccessful studies being hidden away in file drawers. This does suggest that the true effect for psychological health effect sizes may be smaller than is estimated in this analysis, which raises the following question: If I had been able to get all of the unsuccessful studies hiding away in file drawers, would the effect for psychological health no longer be significant? To help answer this question, one can compute a fail-safe sample size (R. Rosenthal, 1991)—the number of studies averaging null effects that would need to exist to make the psychological health effect size nonsignificant. With 112 studies containing psychological health effect sizes and a sum of zs of 40.31, there would need to be 488 studies with null effects hidden away in file drawers to make this psychological health effect size nonsignificant; it seems highly unlikely that such a large number of these studies exists. In addition, it is encouraging that this relation was found only with psychological health effect sizes and not with any of the other effect size categories.

It was predicted that studies in which participants were paid would have larger effect sizes than studies that relied only on unpaid volunteers; this difference was found only with respect to subjective impact effect sizes. It may be the case that payment status does not moderate the effectiveness of the intervention at all (paid participants and unpaid volunteers benefit equally from disclosure)—which would be good news for external validity—but that differences found in subjective impact outcomes are a result of cognitive dissonance reduction in the unpaid control group participants (Festinger, 1957). Experimental procedures performed by the control group—to describe in detail what one has done in the last 24 hr or to describe a neutral object or place—are likely somewhat boring for the control participants. Control participants may be more willing to admit to themselves (and to the experimenters) that the activity was not helpful or enjoyable when they are paid for participating versus when they participate for no payment; unpaid volunteers who participate in an unenjoyable activity day after day without compensation may need to believe that the activity is enjoyable to reduce incompatible cognitions. Therefore, the discrepancy between treatment and control participants may be larger for paid participants because the paid control participants may give the intervention poorer ratings, whereas treatment participants of both types rate the intervention as equally helpful and enjoyable.

An unexpected finding was that predisclosure priming (warning participants in advance that they may be asked to disclose upsetting events) did not moderate the effect of experimental disclosure for any of the outcome types. This is in direct contrast to the one study that experimentally manipulated predisclosure priming

(Cole, 2003), which found that this procedure caused the control group to get worse after the experiment (which should increase the treatment vs. control effect size). The failure to find this moderator meta-analytically may be, in part, due to uncertainty in the meta-analytic coding process for this particular variable. Although most variables could be coded with relatively certainty, whether participants were warned in advance that they would be disclosing upsetting events often was not explicitly stated in the original study report. In the absence of clarification from the study's authors, an educated guess was made on the basis of the rest of the study procedures; however, there might have been a good deal of error in this process. As a precaution, future researchers may want to avoid priming control participants to think about upsetting topics when the participants will not be asked to disclose the topics to avoid even the possibility of inhibition-related detrimental effects.

The final methodological variable examined was timing of follow-up; it was determined that studies with follow-up periods of less than 1 month had larger overall and psychological health effect sizes than studies with follow-up periods of 1 month or more. This finding indicates that although experimental disclosure may cause a temporary increase in negative mood, the negative effects are very quickly replaced by measurable benefits. This finding also indicates that these benefits have a tendency to wear off after some time, especially benefits related to psychological health. This is in agreement with the idea of hedonic adaptation, which states that people's subjective well-being may have a tendency to return to baseline shortly after mood-altering events or interventions (Suh, Diener, & Fujita, 1996). Interventions of this type, therefore, may need to be to readministered periodically to sustain their effectiveness.

Treatment variables. Two of the three dose-related variables, number of disclosure sessions (fewer than three sessions vs. at least three sessions) and length of disclosure sessions (less than 15 min vs. at least 15 min), were found to be related to overall effect size; number of sessions was also related to psychological health and subjective impact effect sizes, whereas the length of sessions was also related to reported health effect sizes. Although no known studies have manipulated the length of sessions within a single study, two studies have varied the number of disclosure sessions. Whereas one study found no significant difference between participants assigned to disclose for one session versus participants assigned to write for three sessions (Walker et al., 1999), another study found that student teachers assigned to disclose for two sessions rated higher on personal teaching efficacy than student teachers assigned to disclose for only one session. These findings, taken together, do seem to indicate that more may be better in terms of disclosure dosage; future researchers may wish to investigate the optimal number of sessions and minutes (as there may be a point at which one begins to find diminishing returns).

Contrary to the findings of Smyth (1998), the third dosage variable, the spacing of disclosure sessions (daily vs. weekly sessions), was not found to moderate any of the outcome types. Two recent studies (Frattaroli, 2003; Sheese et al., 2004) echo these null results; in both of these studies, the spacing of disclosure sessions was experimentally manipulated, and no significant differences were found between daily and weekly treatment groups. From a practical perspective, this is good news for experimenters, in that disclosure sessions may be arranged flexibly in a way that fits the experimenters' and participants' schedules, without much concern for affecting the results.

The first of the three topic-related variables, valence of the disclosure topic, did not moderate any of the outcome types. This was not necessarily unexpected, as it has been argued that disclosing positive events should be just as beneficial as disclosing negative events (Burton & King, 2004). Six studies have experimentally manipulated valence of topic in a single experiment, four of which found no difference between groups assigned to write about positive or negative events (Harris et al., 2005; Njus, Nitschke & Bryant, 1996; Sharsky, 1997; Wagner, 2001), and two of which found more benefit for groups assigned to write about negative events (Klein & Boals, 2001; Norman et al., 2004). However, it should be noted that, for the two studies that found a benefit for negative events, this benefit was only found for two (out of many) outcome variables (study was valuable/meaningful for Klein & Boals, 2001; evaluative pain for Norman et al., 2004), both of which were single-item variables. On the whole, there appears to be preliminary evidence that disclosing positive events is equally as beneficial as disclosing negative events; this finding deserves special attention because disclosing positive events does not carry the short-term negative side effects that are involved with disclosing negative events. One might, therefore, prefer to choose the disclosure of positive events in future studies to spare participants the temporary increase in negative mood that accompanies the disclosure of negative events.

The remaining two topic-related variables, months since topic and prior disclosure of topic, were found to moderate the effect for psychological health effect sizes; months since topic was also related to overall and reported health effect sizes. Although no known studies have manipulated time since topic in a single study, three studies have randomly assigned participants to disclose either a previously undisclosed or a previously disclosed topic, with mixed results; one study found that discussing an undisclosed topic was more beneficial (Paez et al., 1999, Study 1), another study found that discussing a disclosed topic was more beneficial (Paez et al., 1999, Study 2), and yet a third study found that there was no significant effect of topic assignment (Greenberg & Stone, 1992). One thing that is different about these three primary studies is that, in all three cases, the experimenters compared instructions to discuss an undisclosed topic with instructions to discuss a disclosed topic, rather than comparing instructions to discuss an undisclosed topic with no instruction, as is done in the present meta-analysis. It may be that the disclosure status of the topic does not matter as much as the specificity of the experimental instructions; participants may benefit more when they are given more specific (as opposed to general) disclosure instructions.

The importance of the specificity of disclosure instructions is also examined in two of the instruction-related variables, presence or absence of directed questions and specific examples and instructions regarding topic switching. Studies in which participants were given directed questions or specific examples of what to disclose had larger overall and psychological health effect sizes, and studies in which participants were given instructions regarding whether to switch topics had larger psychological effect sizes than studies that provided less specific instructions. No studies have compared the effects of specific instructions or examples versus no specific instructions or examples, but one study has compared the presence or absence of switching-related instructions (Frattaroli,

2001); this study found that groups receiving specific instructions regarding switching (either that they should not switch or that it was okay to switch) improved more on a measure of analytical thinking than a group that was given no instruction regarding topic switching. These findings further support the notion that specificity of experimental disclosure instructions may play an important role.

The remainder of the instruction-related variables, focus of disclosure instructions (cognitive-processing instructions vs. standard), time reference of disclosure instructions (current vs. past vs. choice of current or past), and mode of instruction administration (orally vs. in writing), did not moderate any of the outcome types. Focus of disclosure instructions (giving participants instructions that are specifically designed to promote cognitive processing or insight vs. giving participants standard disclosure instructions) has been experimentally manipulated in 3 studies; 2 studies found no significant effect for focus of disclosure (Habbal, 1999; Kovac & Range, 2002), and 1 found beneficial effects for cognitive-processing instructions (Broderick et al., 2004). Null effects might have been due to inadequate power; both the Habbal (1999) and the Kovac and Range (2002) studies had fewer than 50 participants each, whereas the Broderick et al. (2004) had more than 120 participants. Similarly, in the present analysis, only 6 studies used cognitive-processing instructions, whereas 110 used standard instructions; the lack of variability can make it difficult to detect effects.

Neither the time reference of instructions nor the mode of instruction administration has ever been experimentally manipulated in a single study, which makes our null effects a bit more difficult to interpret. Because the actual recency of the event being disclosed predicted effect size, one would expect that instructions designed to manipulate the recency of the event would also be related to the effect size. However, differences in instructions may be so subtle that they do not affect the recency of the event dramatically enough for effects to be observed. For example, instructions that ask the participants to disclose the most upsetting and traumatic events of their entire life are typically considered to be instructing the participants to disclose a past event; however, experience running experimental disclosure studies with these instructions tell me that participants in these studies often choose to write about a current event. Indeed, in the present data set, there was no significant difference in the recency of events disclosed in studies that assigned participants to disclose a current event compared with studies that assigned participants to disclose their choice of a current or past event ($p = .47$).

The audience of disclosure (no one will read or hear the disclosure vs. experimenter will read or hear disclosure) was found to be a moderator for psychological health outcomes; studies in which participants' disclosure was private had larger effect sizes than studies in which participants' disclosure was turned in to (and read by) the experimenter. This finding is in contrast to the two studies that have manipulated audience of disclosure within a single study; one study found that there were no significant effects of audience (Kunkel, 2001), whereas a second study found that participants with an audience improved more on mood, physical symptoms, and illness-related activity restrictions (Raval, 2000). Because the current finding is in opposition to the results of primary studies, a third variable is suspected to be responsible for the results. Indeed, audience of disclosure was related to the location of disclosure sessions; studies that did not collect products of disclosure were more likely to have participants disclose at home, and disclosing at home was associated with larger effect sizes. When location of disclosure sessions was controlled, the relation between audience and effect size shrank dramatically and became nonsignificant ($r = -.094$, $p = .356$). Therefore, there appears to be insufficient evidence at this time to draw conclusions regarding the effect of having an experimenter collect (and read) the products of disclosure.

The final variable that was examined, the mode of disclosure, was not found to moderate any of the outcome types; studies using handwritten disclosure did not have larger effects than studies using typed disclosure, and studies using oral disclosure did not have larger effects than studies using written disclosure. There might have been null effects for hand writing versus typing, because almost all of the studies requiring typed disclosure (82%) were conducted with college students; it can be expected that college students are relatively comfortable typing on a computer. Furthermore, some studies requiring typed disclosure included the requirement of feeling comfortable typing on a computer for 20 min (e.g., Raval, 2000). It was thought that hand writing would be superior to typing because typing might add additional cognitive demands on the participant; however, this would not be case if the participant was very comfortable with the use of a keyboard. It is still possible that one might find differences between hand writing and typing when using participants who are not familiar with computers or who are not comfortable with typing; however, the only studies that have experimentally manipulated typing versus hand writing in a single study did not collect any long-term outcome measures on the participants (Brewin & Lennard, 1999; Blomquist, Smith, & Gilden, 2002).

There might have been null effects for talking versus writing, because although talking may allow for more kinds of expression (verbal and vocal), it may simply be awkward for participants to talk aloud in the absence of feedback from another person (which may make it difficult to be fully involved in the disclosure process). The benefits obtained by having multiple modes of expression may be offset by the costs associated with feeling awkward during disclosure. Studies in which oral disclosure involved disclosing directly to an experimenter (a less awkward activity) did tend to have a slightly larger effect size ($r = .106$) than studies in which oral disclosure involved disclosing to a tape recorder (a more awkward activity; $r = .048$), although this difference was not significant because the number of studies was so small ($k = 6$). If one does need to disclose orally (because, e.g., arthritis prevents one from disclosing in writing), it might be more effective if the disclosure were directed to a live person (even though this person would not provide feedback or give advice).

Applications to Theory

The primary purposes of the present study were to estimate the average effects of experimental disclosure and to identify characteristics of participants and of disclosure administration that might be related to the effect of the intervention. The aims of this project were somewhat practical or applied in nature, in that having knowledge of how well, when, and for whom disclosure works can inform researchers and clinicians about ways to potentially alter their administration of experimental disclosure and about avenues

for future exploration. Indeed, it has been argued that such an applied approach should be "the most pressing agenda for [experimental disclosure] researchers" (Pennebaker, 2004, p. 141) at this time. That being said, one need not ignore the theoretical implications of these very practical findings, as some of the results from this analysis can be used to inform researchers about the theories about mechanism that were discussed earlier in this article: disinhibition theory, cognitive-processing theory, self-regulation theory, social integration theory, and exposure theory.

Disinhibition theory. As described earlier, disinhibition theory argues that the inhibition of thoughts and feelings regarding an upsetting event is harmful and that, consequently, expression of inhibited thoughts and feelings can reduce stress and improve a host of physical and psychological health outcomes. If disinhibition is the mechanism by which experimental disclosure is helpful, one would expect that traumas that have been inhibited for longer (older and/or undisclosed traumas) would be the best candidates for disclosure. In addition, this theory might predict that people who have a tendency to inhibit their thoughts and feelings (men, Asians, people with inhibiting personalities) should benefit most from a disclosure intervention. Finally, as the theory specifically states that disinhibition causes a reduction in stress, one would expect that disclosure should reduce self-reported and objective measures of stress.

The results of this meta-analysis provide very little support for the disinhibition theory. In regard to the traumas that should be the best candidates for disclosure, the disclosure of recent events, not older events, was positively related to effect size. Also, although there was some evidence that studies that instructed participants to discuss undisclosed trauma were marginally better than studies that lacked this instruction, it may also be the case that the specificity of the instructions, rather than the actual content of the instructions, was responsible for this benefit. In regard to the people who should benefit most from disclosure, there was insufficient evidence to conclude that Asians, men, or people with an emotionally inhibited personality benefited more from disclosure. Finally, in regard to stress reduction, neither self-reported stress or objective measures of the stress response were found to be significant in either a random or a fixed effects model.

Cognitive-processing theory. As discussed earlier, the cognitive-processing theory argues that the act of making sense of an event, of gaining insight about a trauma, and of organizing and integrating an upsetting experience into one's self-schema is the mechanism by which expressive writing is helpful. If this is the case, there are two predictions relevant to this theory that can be tested with the current data: Studies that use theory-driven cognitive-processing disclosure instructions should be more beneficial, and studies with greater spacing in between sessions (to give participants time to process the event in between sessions) should also produce higher effect sizes. However, the data do not support these predictions; cognitive-processing instructions were not significantly related to higher effect sizes, and there was no difference between studies that arranged sessions on consecutive days and studies that arranged sessions spaced 1 week apart.

Self-regulation theory. As mentioned earlier, the self-regulation theory argues that disclosure allows people to observe themselves expressing and controlling their emotions, which, in turn, gives them a new or stronger sense of self-efficacy for emotional regulation. This self-efficacy causes them to feel that their challenges are more controllable, hence reducing negative affect (and leading to improvements in a number of areas). If self-regulation is indeed the mechanism by which experimental disclosure works, one might expect the following: Direct measures of self-regulation should improve after disclosure; mood-related problems, such as symptoms of depression, should lessen after disclosure; and, because the act of observing oneself express and control one's emotions does not need to be limited to negative emotion, writing about the positive should be just as good as writing about the negative.

In the present study, the self-regulation theory did receive some mixed support according to these predictions. Although the average effect for the category of self-regulation was not significant in either a random or a fixed effects model, the average effect for reducing symptoms of depression was significant, with a small and positive unweighted effect size ($r = .073$). In addition, there was no significant difference between studies that instructed participants to disclose negative events and those that instructed participants to disclose positive events. However, as noted earlier, only a very small number of studies examined the disclosure of positive events, so significant differences would be rather difficult to detect at this time.

Social integration theory. As previously described, the social integration theory argues that experimental disclosure affects the way people interact with their social world, which, in turn, improves their health and well-being. This theory, therefore, predicts that participants assigned to disclosure would be more likely to talk about the study, their disclosure topic, and their traumatic event more often and would be more likely to have improved social relationships in the days and weeks after the intervention. Upon examination of the results of the present study, mixed support is found for this theory regarding these two predictions. Participants in the treatment group were not significantly more likely to discuss the study or their event after the intervention, but they were significantly more likely to improve their social relationships (e.g., stop holding a grudge, attend a club or a meeting). However, it should be noted that because the effect of social relationships was rather small ($r = .060$) and was measured by a relatively small number of studies (16), the fail-safe N for this outcome is only 2 studies. Additional research examining social relationships should be conducted before definitive conclusions are drawn regarding this outcome (and this theory).

Exposure theory. As stated earlier, exposure theory argues that when a person repeatedly confronts, describes, and, in essence, relives the thoughts and feelings about his or her negative experience (as is accomplished by having participants disclose their event over and over throughout several days), this repetition and exposure should eventually lead to extinction of those thoughts and feelings. It has been argued elsewhere that if this theory is correct, disclosure should lead to reductions in posttraumatic stress symptoms and should also be especially helpful for participants who have a history of trauma (Sloan & Marx, 2004b). In addition, if repeated exposure is the mechanism by which disclosure is helpful, the dosage of the treatment—the number and length of sessions—should be an important predictor of effect size, in that stronger doses (more exposure) should lead to larger effect sizes.

Of all the theories described in this section, exposure theory receives the most support, with all four predictions being borne out by the data. Posttraumatic stress symptoms were marginally reduced by disclosure (at least in a fixed effects model), and studies

that used participants with a history of trauma had higher effect sizes (at least for subjective impact outcomes). Furthermore, studies with at least three sessions and studies with sessions of at least 15 min were more effective than studies with fewer than three sessions and those with sessions of less than 15 min.

Conclusions

In summary, experimental disclosure was found to be beneficial for one's psychological health, physical health, and overall functioning, with an average effect size of .075. Studies in this domain have varied widely on a number of characteristics, many of which are correlated with the size of the effect. The successful study tended to use participants with a health problem or participants with a history of trauma, to make sure participants were very comfortable during disclosure (e.g., by allowing them to disclose at home), to pay participants, to administer a large dose of disclosure (e.g., by requiring at least three disclosure sessions), to have participants disclose events that had yet to be fully processed (e.g., more recent events), to provide very detailed and specific disclosure instructions (e.g., directed questions), and to have relatively short follow-up periods (e.g., less than 1 month). Each of these potential moderators has received only limited attention, if any, in primary studies, and all may be good candidates for future research. Further examination of these moderators will add to our knowledge of the optimal conditions under which disclosure should take place, so that this accessible, inexpensive, simple, and helpful intervention can be administered to general populations in the most effective manner possible.

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