Theoretical/Methodological/Review Article

ASSOCIATION FOR PSYCHOLOGICAL SCIENCE

Clinical Psychological Science
2023, Vol. 11(3) 552–573

DOI: 10.1177/21677026221131309
www.psychologicalscience.org/CPS
SAGE

A Meta-Analysis of the Relationship Between Worry and Rumination

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Abstract

Clinical scientists disagree about whether worry and rumination are distinct or represent a unitary construct. To inform this debate, we performed a series of meta-analyses evaluating the relationship between worry and different forms of rumination. A total of 719 effect sizes (N = 69,305) were analyzed. Worry showed a large association with global rumination and with the brooding and emotion-focused subtypes of rumination (rs = .51–.53). However, even when corrected for measurement error, the correlations did not approach unity (ρs = .57–.62). Worry showed a smaller, though still significant, association with the reflection subtype of rumination (r = .28, ρ = .34). Characteristics of the study, sample, and measures moderated the worry–rumination relationship. Worry and rumination, as indexed by current self-report measures, reflect closely related but nonredundant constructs. Given that these constructs have both common and distinct features, researchers should select between them carefully and, when possible, study them together.

Keywords

worry, rumination, perseverative thought, repetitive negative thinking, cognitive processes, meta-analysis

Received 11/29/21; Revision accepted 9/21/22

Worry and rumination are negatively valenced cognitive processes that are robustly associated with psychopathology. Worry, defined as negative, difficult-to-control thinking about future events whose outcomes are uncertain (Borkovec et al., 1983), is a core component of anxiety and the central feature of generalized anxiety disorder (GAD; American Psychiatric Association, 2013). Rumination, or repetitive negative thinking about one's feelings or past events, particularly past failures, has most often been studied in the context of depression (Nolen-Hoeksema et al., 2008).

Traditionally, worry and rumination have been viewed as distinct forms of negative thinking that are associated with different forms of psychopathology (Wells & Matthews, 1994, pp. 147–164). However, in the past two decades, theorists have begun to highlight conceptual overlap between these constructs (e.g., Fresco et al., 2002; Harvey et al., 2004; Segerstrom et al., 2000). Increasing recognition of overlapping features has spurred a proliferation of transdiagnostic measures assessing a unitary "perseverative" or "repetitive negative" thinking construct (e.g., Ehring et al., 2011; Magson et al., 2019; McEvoy et al., 2010; Miranda et al., 2017; Szkodny & Newman, 2019). In parallel, a growing number of studies have examined perseverative cognition, broadly defined, rather than studying worry or rumination individually (e.g., Everaert & Joormann, 2019; Ottaviani et al., 2015; Ruscio et al., 2011; Spinhoven et al., 2018; Stade et al., 2022). Despite these trends, researchers disagree about the merits of "lumping" in this domain, and most research continues to treat worry and rumination as separate constructs. Thus, a major unresolved question impeding progress in the field is whether to retain separate worry and rumination constructs or shift to a unitary transdiagnostic construct of perseverative thought.

The answer to this question has important implications for theory, measurement, research, and treatment. As prior examples of the jangle fallacy (Credé et al.,

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2017; Kelley, 1927) have shown, assigning different names to constructs that are nearly identical creates artificial divisions in the literature that slow the accumulation of knowledge. If worry and rumination are essentially redundant, combining them would permit more parsimonious theoretical models, a more efficient search for risk and maintaining factors, and a core set of intervention strategies that may be offered to a wider range of patients. By contrast, if worry and rumination are truly distinct, combining them may wipe out meaningful variation that is needed to account for differing clinical presentations, predict divergent outcomes, and develop more targeted and effective interventions.

Shared and Distinguishing Features of Worry and Rumination

Supporting the notion of a unitary construct, research has revealed many similarities between worry and rumination. Both processes involve repetitive, negative, self-focused thinking; are primarily verbal-linguistic and abstract; and are difficult to control (Ehring & Watkins, 2008; Harvey et al., 2004; Holmes & Mathews, 2010). By contrast, efforts to identify unique characteristics of worry and rumination have produced few robust effects. Studies that ask participants to rate a worried thought and a ruminative thought along various dimensions have revealed mostly shared and few distinguishing features (Papageorgiou & Wells, 1999; Watkins, 2004; Watkins et al., 2005). The only distinguishing feature that has been replicated across studies is temporal orientation, with worry oriented toward the future and rumination oriented toward the past (Papageorgiou & Wells, 1999; Watkins et al., 2005). This has led to the suggestion that the two processes may involve different unresolved concerns (e.g., future threat in worry, past loss in rumination) being worked through the same underlying mechanism (Harvey et al., 2004).

In vivo studies have yielded similar results. McLaughlin and colleagues (2007) induced worry and rumination in the laboratory, finding the former associated mostly with future thinking and the latter associated mostly with past thinking. Additionally, during rumination but not worry, participants shifted focus from the past to the present/future over time. Despite these differences, both processes showed a mix of temporal orientations, both were predominantly verbal rather than imagery based, and most participants reported content overlap between their worried and ruminative thoughts. Using experience-sampling methodology, Kircanski and colleagues (2015) studied features of rumination and worry in the daily lives of women with major depressive disorder (MDD) and GAD. Rumination and worry differed in temporal orientation and were further distinguished by self-focus (unique to rumination) and by situational uncertainty, verbal-linguistic focus, and concreteness (all unique to worry). However, the two processes also shared a number of characteristics, including unpleasantness, repetitiveness, and lack of situational control.

In summary, studies have revealed more similarities than differences between worry and rumination. Although temporal orientation has emerged as a consistent difference, this is perhaps unsurprising given that, when the constructs are defined for participants, the definitions typically reference the future (for worry) or past (for rumination). This raises the possibility that some differences reflect artifacts of how worry and rumination are operationalized rather than meaningful distinctions between the natures of these constructs. That said, a few differences await replication, and some theorized differences remain to be tested (see Nolen-Hoeksema et al., 2008), leaving open the possibility that robust distinctions may yet be found.

Worry and Rumination Factors

If worry and rumination are distinct constructs, they might be expected to form separate factors when analyzed together. Multiple-factor analyses have indeed found that worry and rumination load on separate factors (Fresco et al., 2002; Goring & Papageorgiou, 2008; Muris et al., 2004; Segerstrom et al., 2003). Other factor analyses, however, have found that worry and rumination load on a common factor. For example, Siegle and colleagues (2004) analyzed multiple measures of rumination and worry, showing that worry and negatively valenced trait rumination scales loaded on the same factor. Recognizing that measurement artifacts could inflate the number of factors, McEvoy and colleagues (2010) modified existing measures of worry, rumination, and postevent processing to standardize their response scale and language. Factor analysis revealed a two-factor solution, with most worry and rumination items loading on the first factor and only reverse-scored worry items loading on a second, method factor. More recent studies using bifactor modeling have revealed both a common factor and separate worry- and rumination-specific factors (e.g., Hur et al., 2017; Topper et al., 2014). These mixed results complicate the question of whether worry and rumination are best understood as distinct constructs or as a unitary process.

Correlates of Worry and Rumination

If worry and rumination represent distinct constructs, they would be expected to share differential associations with relevant outcomes. Contrary to early assumptions that worry relates specifically to anxiety whereas rumination relates specifically to depression, numerous studies have documented strong associations of worry and rumination with both outcomes (e.g., Muris et al., 2005; Nolen-Hoeksema, 2000; Olatunji et al., 2013). A few studies have directly compared the correlates of worry and rumination. In cross-sectional analyses, worry and rumination each correlated significantly with anxiety and depression symptoms, even when analyses controlled for the other type of perseverative thought (Hughes et al., 2008). Although the associations with anxiety were similar in magnitude, the association with depression was larger for rumination than worry, though this may be because the rumination measure was strongly saturated with depression content (Treynor et al., 2003). In longitudinal analyses predicting symptoms over 2 months, both worry and rumination predicted later anxiety, whereas neither worry nor rumination predicted later depression (Calmes & Roberts, 2007). When induced experimentally in the lab, worry and rumination both produced decreases in positive affect and increases in negative affect, anxiety, and depression (McLaughlin et al., 2007). However, when studied in patients' daily lives, rumination, but not worry, predicted subsequent decreases in positive affect and increases in negative affect (Kircanski et al., 2018).

In summary, worry and rumination exhibit similar associations with affective symptoms and experiences. There is mixed evidence for a more robust relationship of rumination with these outcomes, although measurement factors may account at least partly for this pattern. Although similar correlates do not substantiate a unitary worry–rumination construct, they do suggest that the two processes may be more alike than different.

Previous Meta-Analytic Research

Currently missing from the literature, yet a critical piece of the puzzle for informing this debate, is a comprehensive, unbiased estimate of the strength of association between worry and rumination. A meta-analysis is ideally suited to provide this information. To our knowledge, two previous studies reported meta-analytic estimates of the worry–rumination relationship. Olatunji and colleagues (2013) estimated this relationship as a precursor to calculating the partial correlation of rumination with depression and anxiety, controlling for worry. They reported a correlation between worry and rumination, corrected[1] for measurement error, of $\rho = .45$, based on 33 samples and 7,453 participants. Importantly, only studies that were eligible for the primary meta-analyses (i.e., studies reporting a rumination–depression or rumination–anxiety effect size in a sample with clinically diagnosed participants) were included in this secondary meta-analysis. This markedly restricted the number of samples and may have skewed the estimate toward clinical samples. Additionally, worry and rumination are commonly studied outside of the depression/anxiety literature, so the question remains whether including studies from other literatures would alter the findings.

Naragon-Gainey and colleagues (2017) examined the relationship between worry and rumination as part of a series of meta-analyses investigating associations among common emotion-regulation strategies, with worry and rumination defined as two such strategies. The uncorrected correlation (r) of .49, and corrected correlation (ρ) of .58, were based on 46 samples and 11,562 participants. The correlation was moderated by sample type, appearing significantly smaller in clinical ($r = .42$) than nonclinical ($r = .50$) samples. Given that the primary focus of the study was emotion regulation, rumination was defined narrowly as repetitive, passive thinking about negative emotion; consequently, numerous measures that assess ruminative thoughts without reference to emotions were excluded. Furthermore, the meta-analysis was restricted to trait measures, excluding measures with shorter time frames and precluding a test of whether the strength of the worry–rumination relationship depends on the time frame assessed.

The Present Study

We sought to obtain a more definitive estimate of the relationship between worry and rumination by extending these previous meta-analyses in several ways. First, many researchers administer worry and rumination measures as part of larger studies but seldom publish the correlation between these measures; consequently, a large proportion of available effect sizes are unpublished and were not included in the earlier analyses. To provide maximum coverage of relevant effect sizes, we not only sent out blanket requests for unpublished data but also contacted authors of published articles in which worry and rumination were both assessed but no correlation was reported. This enabled us to collect and include hundreds of unpublished effect sizes in the present study.

Second, neither of the previous meta-analyses distinguished between different forms of rumination when estimating the relationship with worry. This is an important limitation given evidence that rumination, unlike worry, is a highly heterogeneous construct (Siegle et al., 2004). Studies have repeatedly revealed multiple types of rumination, most consistently a brooding type and a reflective type (McEvoy & Brans, 2013; Treynor et al., 2003). Given that some rumination subtypes may be unrelated to psychopathology (Takano & Tanno, 2009; Trapnell & Campbell, 1999), it is plausible that rumination's relationship to worry depends on the subtype assessed. We considered multiple types of rumination and tested whether they relate differently to worry.

Third, in addition to rumination subtype, we explored other potential moderators of the worry–rumination relationship. For example, Naragon-Gainey and colleagues (2017) found a weaker relationship in clinical compared with nonclinical samples, which may imply that worry and rumination are better differentiated among individuals with psychopathology. This finding, however, was based on only seven clinical samples. We attempted to replicate the moderating effect of sample type, not only in a larger data set, but also taking a more fine-grained approach by including mixed samples (those containing both a clinical group and a control group), which were excluded from the previous moderation analysis despite widespread use of case-control designs in this literature.

Fourth, neither of the previous meta-analyses employed a modeling approach that accommodated multiple effect sizes per study, instead averaging effect sizes within studies or randomly selecting a single effect size from each study. These approaches may have concealed important within-study differences between effect sizes, such as those based on different rumination measures that vary substantially in content (Siegle et al., 2004). We adopted a powerful statistical approach, three-level modeling, that allowed us to include all effect sizes in our analyses and to model within-study variability in effect sizes.

We expected to find a large correlation between worry and rumination, even before correcting for attenuation due to measurement error. However, given the mixed results reviewed above, it was unclear what the actual magnitude of this association would be. By quantifying the size of the relationship, we aimed not only to advance understanding of worry and rumination but also to contribute one useful piece of evidence toward the debate over whether they are best construed as unitary versus distinct processes. To our knowledge, there is no widely accepted and empirically based convention regarding a correlation size that justifies the combination of two overlapping constructs. To guide our interpretation of results, we looked to two constructs that have been the focus of a similar debate: anxiety and depression. Like worry and rumination, anxiety and depression are closely related: The correlation between nonspecific symptoms of anxiety and depression is .69 (Watson, 2009). Notably, GAD and MDD—which show particularly high comorbidity and are the disorders most strongly linked to worry and rumination, respectively—share a tetrachoric correlation of .64 (Watson, 2009). Whether GAD and MDD should be retained as separate diagnostic categories was debated extensively in the lead-up to creating the fifth edition of the *Diagnostic and Statistical Manual of Mental Disorders* (American Psychological Association, 2013; Goldberg et al., 2010), implying that a correlation as high as .64 was sufficient to raise questions about separability. This aligns with the conventional wisdom that a correlation around .7 may indicate essentially redundant constructs. Although we lacked a strong basis for setting an a priori threshold above which worry and rumination could defensibly be combined, we planned to use these correlations as a context when interpreting our meta-analysis results.

Method

Identification of studies

Eligibility criteria. Studies were eligible for the meta-analysis if they met the following criteria: (a) The study included a measure of worry; (b) the study included a measure of rumination; (c) both measures were administered concurrently and on the same time scale (i.e., both trait measures, both state measures), with the association reported as a zero-order correlation; and (d) the study was written in English. Though they were rare in our sample, we permitted studies that involved an experimental manipulation prior to assessment of worry and rumination as long as they met our aforementioned criteria.

Search strategies. We conducted a systematic search of three electronic databases—PsycINFO, PubMed, and Web of Science—covering all years up to the search date in March 2020. The search terms used were (worr*) and (ruminat* or brood*). The search yielded 1,765 results.

To address the file-drawer problem, we included unpublished doctoral dissertations in the literature search. Additionally, we contacted (twice, if necessary) 49 researchers who frequently publish research on worry or rumination, asking whether they had unpublished data relevant to the research question. These researchers identified seven additional researchers believed to have relevant data, whom we contacted with the same request. Nine researchers supplied new data for 10 studies identified in our electronic search, as well as 22 studies or unpublished data sets that were not identified in our electronic search but met eligibility criteria for the meta-analysis. Lastly, we included two unpublished data sets that the senior author (A. M. Ruscio) had on file.

Study selection. Figure 1 depicts the process by which studies were selected for the current analyses. Combining the 1,765 records identified via electronic database search and 24 records identified via direct correspondence with investigators yielded 1,789 total records. Duplicate records (n = 685) were removed, leaving 1,104 records. We

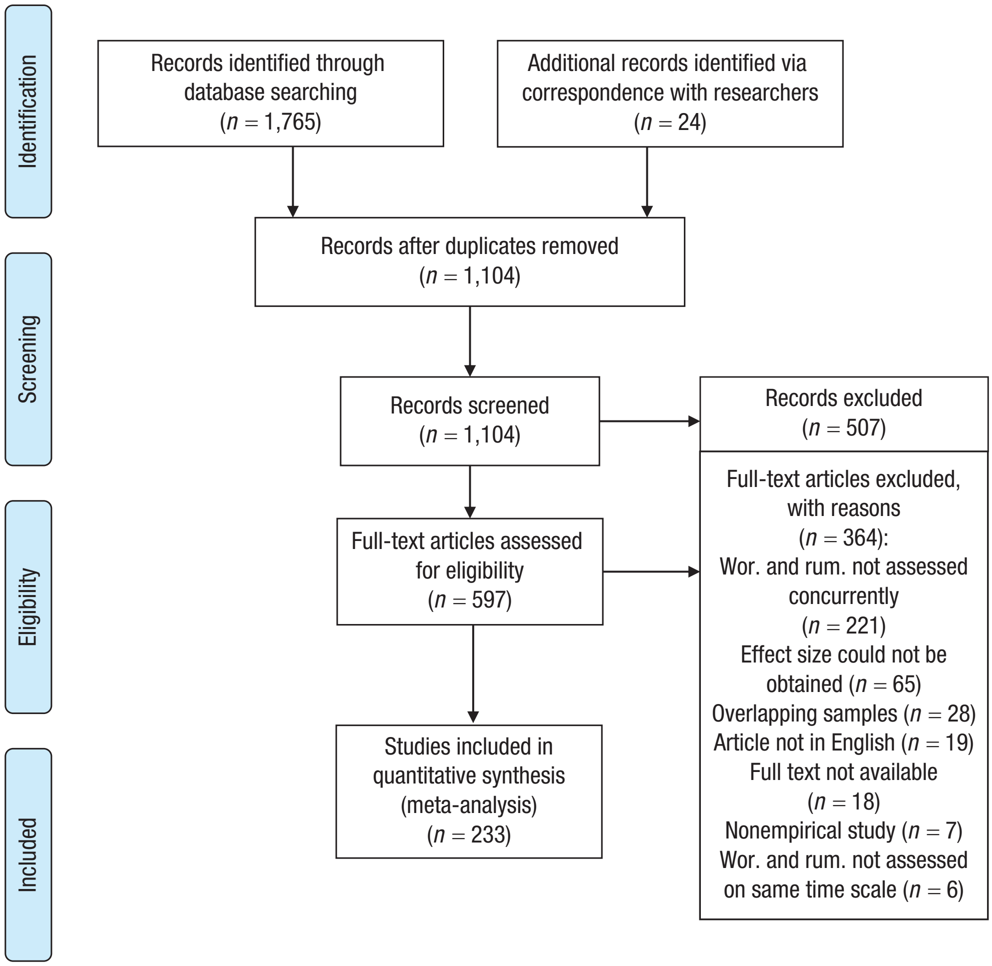

Fig. 1. Preferred reporting items for systematic reviews and meta-analyses (PRISMA) flow diagram depicting the process by which studies were selected for the current analyses. Rum. = rumination; Wor. = worry.

screened the title, keywords, and abstract of each study. If a study appeared to meet eligibility criteria, or more information was needed to determine eligibility, the full-text article was obtained. The full texts of 597 studies were assessed for eligibility. The main reasons for exclusion were (a) worry and rumination were not assessed in the same participants concurrently or (b) we were not able to extract an effect size. When the data required to extract an effect size were not reported in the article, we contacted the authors (twice, if necessary) and requested the necessary data. Through this process, 67 authors provided additional data for 85 studies, bringing the total number of studies in the meta-analyses to 233. In all, 719 effect sizes were included, of which 368 (51%) were obtained directly from researchers.

Data extraction

Effect-size coding. All studies used the Pearson's r correlation coefficient to represent the association between worry and rumination. Along with r, we coded the reliability of the measures (Cronbach's α) to correct for attenuation due to measurement error. If a study did not report Cronbach's α, the mean α from all studies in the meta-analysis that reported αs for that measure was used.

More than one relevant effect size was reported for 33% of the studies in the global rumination meta-analysis and for 25%, 31%, and 30% of the studies in the brooding, reflection, and emotion-focused rumination meta-analyses, respectively. The following reasons existed for multiple effect sizes per study: (a) Multiple measures of worry or rumination were administered in a single study, (b) correlations between worry and rumination were reported at multiple time points, (c) different worry or rumination subtypes were assessed within a single study, and (d) worry and rumination were measured in different groups of participants (i.e., separate effect sizes were reported for male and female participants, clinical and nonclinical samples, or two or more separate samples). When multiple effect sizes from overlapping measures in the same study were eligible for the same meta-analysis (e.g., both a total scale score of global rumination and a subscale consisting of a subset of rumination items from the same scale), we retained only the effect size derived from the more complete scale. For multiple effect sizes from the same study that did not represent overlapping measures, we handled these dependencies in the data using three-level meta-analysis (discussed below).

Several rumination subtypes have been identified in the literature. Olatunji et al. (2013) examined three subtypes: brooding, reflection, and "emotion-driven" rumination. On the basis of an examination of the measures in our data set (see Table 1 and the Supplemental Material available online, where we present references for all measures), we derived the same three subtypes. Emotion-focused rumination included measures of rumination in response to sadness (Conway et al., 2000), anger (Sukhodolsky et al., 2001), and anxiety/worry (Starr & Davila, 2012). Following Olatunji et al. (2013), we classified a rumination scale as assessing "global rumination" if a subtype of rumination was not apparent or if the scale comprised two or more rumination subtypes. We excluded rumination measure subscales that assessed a construct theoretically distinct from rumination, such as problem solving (e.g., Stress-Reactive Rumination Scale [SRRS] Active Problem-Solving subscale) or distraction (e.g., Ruminative Responses Scale [RRS] Children–Distraction subscale).

Methodological quality assessment. Study quality was rated using the Quality Assessment Tool for Observational Cohort and Cross-Sectional Studies (National Institutes of Health, 2014). This instrument contains 14 criteria, each of which assesses a different set of potential threats to the internal validity of a study. We retained six criteria that were relevant to assessing the quality of the cross-sectional data included in our analyses: "Was the research question or objective in this paper clearly stated?" (Criterion 1), "Was the study population clearly specified and defined?" (Criterion 2), "Were all the subjects selected or recruited from the same or similar populations (including the same time period?)" (Criterion 4), "Was a sample size justification, power description, or variance and effect estimates provided?" (Criterion 5), "Were the exposure measures (independent variables) [operationalized as the worry measure] clearly defined, valid, reliable, and implemented consistently across all study participants?" (Criterion 9), and "Were the outcome measures (dependent variables) [operationalized as the rumination measure] clearly defined, valid, reliable, and implemented consistently across all study participants?" (Criterion 11). Because quality ratings could differ across effect sizes within a study, we assigned each effect size a score from 0 to 6, with lower scores indicating poorer study quality.

Moderator coding. We coded moderators that might explain variability in effect sizes between and within studies. Our moderators reflected (a) study characteristics, (b) sample characteristics, and (c) measure characteristics. Following Fu et al. (2011), we set $k = 6$ as the minimum number of studies required to test a continuous moderator, and $k = 4$ as the minimum number of studies per category to test a categorical moderator. For significant categorical moderators with more than two categories, we performed post hoc pairwise comparisons using Tukey's honestly significant difference test.

Study characteristics. We coded whether the study was published or unpublished, year of publication, and geographic location where the study was conducted. For each effect size, we recorded whether the correlation coefficient itself was available in the report obtained via our literature search or was obtained via direct communication with the author.

Sample characteristics. For each effect size, we coded mean age, percentage of female participants, and percentage of White participants. We also coded sample composition: whether the sample was selected for the presence or elevated levels of psychopathology or was unselected/selected for the absence of psychopathology. Within selected samples, we classified the sample as either (a) clinical, if participants were diagnosed with a mental disorder via clinical interview or were referred from a mental health clinic, or (b) analogue, if participants self-reported symptoms of psychopathology or were diagnosed with a mental disorder on the basis of a questionnaire. For clinical and analogue samples, we also coded whether the sample included a control group without psychopathology.

Measure characteristics. For each effect size, we coded the particular worry and rumination scale/subscale employed, whether the measure indexed thinking about a single topic or content area (e.g., worry about sleep, rumination about pain) or assessed worry or rumination more generally, and the measure time frame (trait or state). Following Clancy et al. (2020), we coded measures as trait when their instructions referenced the habitual tendency to engage in worry or rumination without a specified time frame, and we coded the remaining measures according to the time frame that was referenced (i.e., past week or weeks, past day, or current moment).

Interrater reliability. Interrater reliability for data extraction, performed on a randomly selected 20% of studies

Table 1. Examples of Rumination Measures Included in the Meta-Analyses, by Rumination Type and Subtype

Rumination type and scale	Reference	Items (n)	Sample item
Global rumination			
Ruminative Responses Scale	Nolen-Hoeksema & Morrow, 1991; Treynor et al., 2003	22	Think about all your shortcomings, failings, faults, mistakes; Think about how alone you feel
Rumination-Reflection Questionnaire, Rumination subscale	Trapnell & Campbell, 1999	12	I often find myself reevaluating something I've done; I tend to "ruminate" or dwell over things that happen to me for a really long time afterward.
Ruminative Thought Style Questionnaire	Brinker & Dozois, 2009	20	I find that my mind often goes over things again and again; When I have a problem, it will gnaw on my mind for a long time
Scott McIntosh Rumination Inventory	Scott & McIntosh, 1999	9	I rarely get upset at myself when I am having problems reaching important goals; I often get distracted from what I'm doing by thoughts about something else
Rumination Scale	McIntosh et al., 1995; McIntosh & Martin, 1992	10	If I don't want to think about something, I'm able to just stop thinking about it; When I have a problem, I tend to think about it a lot of the time
Brooding			
Ruminative Responses Scale, Brooding subscale	Treynor et al., 2003	5	Think "Why do I always react this way?"; Think "Why do I have problems other people don't have?"
Multidimensional Rumination in Illness Scale, Brooding subscale	Soo et al., 2014	9	I think about the things my illness might stop me doing; I think about how little I can do to improve my situation
Reflection			
Ruminative Responses Scale, Reflection subscale	Treynor et al., 2003	5	Go away by yourself and think about why you feel this way; Analyze recent events to try to understand why you are depressed
Rumination-Reflection Questionnaire, Reflection subscale	Trapnell & Campbell, 1999	12	I'm very self-inquisitive by nature; People often say I'm a "deep," introspective type of person.
Emotion-focused			
Rumination on Sadness Scale	Conway et al., 2000	13	I repeatedly think about what sadness really is by concentrating on my feelings and trying to understand them; I have difficulty getting myself to stop thinking about how sad I am.
Angry Rumination Scale	Sukhodolsky et al., 2001	19	I analyze events that make me angry; Whenever I experience anger, I keep thinking about it for a while
Response to Anxiety Questionnaire	Starr & Davila, 2012	32	Think your anxiety will stop you from enjoying life; Go someplace alone to think about your anxiety/worries

by E. C. Stade and a trained research assistant, was perfect for effect sizes (intraclass correlation coefficient [ICC] = 1.00) and excellent for sample size and for Cronbach's αs for worry and rumination measures (ICCs = .95–1.00). For moderators, the modal ICC value was .92, with all but two values (for sampling structure and worry measure focus) greater than .80. For the latter two moderators, the raters discussed each disagreement and arrived at a consensus rating that was used in subsequent analyses, then revisited the remaining ratings to ensure they were in line with the consensus.

Statistical analysis

The correlation between worry and rumination was corrected for unreliability using Cronbach's α. The formula for correlation is given as

$$r_{\text{corrected}} = \frac{r_{\text{observed}}}{\sqrt{\alpha_{\text{rumination measure}} \times \alpha_{\text{worry measure}}}}$$

(Lipsey & Wilson, 2001). We employed a random-effects model, which allows for the possibility that the true effect size varies between studies (Borenstein et al., 2009). The effect sizes included in the analysis are assumed to represent a random sample of all possible effect sizes. Because we were interested in examining the relationship between worry and rumination at varying levels of psychopathology, a random-effects model is more appropriate than the more traditional fixed-effects model, which assumes that the true effect size is the same in all studies (Borenstein et al., 2009).

Conventional meta-analysis employs a two-level approach, modeling two sources of variability: sampling error of individual studies (ϵ_k) and between-study heterogeneity (ζ_k; Harrer et al., 2021). This approach assumes that effect sizes are independent. Dependency of effect sizes is typically handled by eliminating effect sizes or by averaging the dependent effect sizes within a study. These strategies sacrifice statistical power and gloss over what may be meaningful differences between effect sizes in the same study (Assink & Wibbelink, 2016).

Three-level meta-analysis is designed to handle dependency of effect sizes. In these random-effects models, ϵ_k is modeled at Level 1, variation in effect sizes due to within-study differences is modeled at Level 2, and variation in effect sizes due to between-study differences is modeled at Level 3. Moderator analyses can be used to test variables that explain between- or within-study heterogeneity (Assink & Wibbelink, 2016). Three-level meta-analysis requires investigators to estimate the correlations among effect sizes within studies. As we did not have access to this information for most studies, we imputed the sampling covariances within studies on the basis of the variance of the effect size estimates. We assumed a correlation of .5 between effect sizes estimates to reflect the typically strong relationship between these types of effects, and we conducted follow-up sensitivity analyses at varying correlation sizes to test the assumption. Last, we estimated cluster-robust standard errors for our meta-analytic models using a sandwich-type estimator, which allows for valid inferences of average effect sizes despite missing covariance estimates.

We performed analyses in the R programming environment (Version 3.6.1; R Core Team, 2019). We built models with the rma.mv function and estimated robust standard errors with the robust.rma.mv function from the *metafor* package (Version 3.0.2; Viechtbauer, 2010). Restricted maximum likelihood estimates were calculated as the τ^2 estimator. We imputed covariance matrices using the impute_covariance_matrix function of the *clubSandwich* package (Version 0.4.1; Pustejovsky, 2020). We performed post hoc analyses using the glht function from the *multcomp* package (Version 1.4.16; Hothorn et al., 2008).

Outliers. We calculated Cook's distance to determine whether any individual studies had a disproportionate effect on the overall correlation. Cook's distance (D) is calculated as follows:

$$D_i = \frac{\sum_{j=1}^{n} (\hat{Y}_j - \hat{Y}_{j(i)})^2}{pMSE},$$

where $\hat{Y}_j$ is the fitted value for the jth observation, $\hat{Y}_{j(i)}$ is the fitted value for the jth observation with the ith observation removed during model generation, p is the number of parameters in the model, and MSE is the mean square error of the model. A large Cook's distance indicates that the data point in question has a significant influence on the model.

Publication bias. We undertook multiple strategies to assess publication bias. First, we examined funnel plots, which plot the standard error on the y-axis by the effect size (r) on the x-axis, centered around the mean effect size from the meta-analysis. Larger studies, which estimate the population mean more precisely, should be clustered near the center of the plot, whereas smaller studies, which estimate the population mean less precisely, should show more scatter. If publication bias is present, funnel plots tend to appear asymmetrical given the absence of smaller studies reporting negative or near-zero results.

Next, we used the Egger regression method (Egger et al., 1997) to quantify the amount of asymmetry in the funnel plots. This is accomplished by testing the inverse of the sample size as a moderator in each meta-analysis. If publication bias is present, this term will significantly moderate the observed effect size.

Last, we tested two types of publication status as moderators: (a) study publication status, comparing effect sizes for published reports with effect sizes for unpublished dissertations and data sets, and (b) effect size publication status, comparing effect sizes that were included in the studies we obtained via the literature search with effect sizes obtained via direct communication with authors.

Heterogeneity. Rather than examining variance of all effect sizes in the data set (as is calculated by the Q statistic), it is more appropriate in three-level meta-analysis to examine variance of effect sizes within studies (at Level 2) and between studies (at Level 3).

Significance of within- and between-study variance. To determine the value added by modeling within-study variance and between-study variance, we built two additional models in which the within-study variance (Level 2) and between-study variance (Level 3) were fixed to

Table 2. Effect Sizes for the Association Between Each Rumination Type and Worry

Rumination type	k	ES	N	r (SE)	95% CI	t	ρ (SE)	I^2
Global	180	386	55,599	.52 (.01)	[.50, .54]	t(179) = 48.96	.58 (.01)	81.27%
Brooding	103	157	28,749	.53 (.01)	[.51, .55]	t(102) = 51.72	.62 (.01)	55.20%
Reflection	84	145	22,146	.28 (.02)	[.24, .31]	t(83) = 14.96	.34 (.02)	82.08%
Emotion-focused	23	31	6,002	.51 (.02)	[.46, .55]	t(22) = 22.40	.57 (.02)	57.97%

Note: All effect sizes are significant at $p < .001$. k = number of studies; ES = number of effect sizes; N = number of unique participants; CI = confidence interval (for Pearson's r coefficient); t = t value of Pearson's r coefficient; ρ = corrected correlation coefficient; I^2 = variance not due to sampling error.

zero, respectively, whereas the other variance component was freely estimated. We then performed a series of tests comparing each of these reduced models with our original model, in which both variance components were freely estimated. As recommended by Assink and Wibbelink (2016), we used a log-likelihood-ratio test, testing the null hypothesis that the variance component in question equals zero (the alternative hypothesis is that the variance component is greater than zero). We used a one-sided test because variance components cannot be negative.

Distribution of variance across levels. We examined I^2_{total}, which represents the sum of the within-study heterogeneity and the between-study heterogeneity, or the total amount of heterogeneity not due to sampling error (Harrer et al., 2021). We applied Hunter and Schmidt's (1990) 75% rule, which states that if less than 75% of the total variance is attributed to sampling variance (Level 1), the heterogeneity can be considered substantial.

Results

Preliminary analyses

We present individual effect sizes along with study and sample characteristics in Table S1 in the Supplemental Material, where we also present references for all studies included in the meta-analyses. Participants in our samples ranged in age from 10.80 to 76.00 years old (M = 28.25, SD = 11.28). Across our samples, 63% of participants were female (SD = 24%, range = 0%–100%) and 73% were White (SD = 23%, range = 0%–100%). Of the effect sizes included in these meta-analyses, 69% represented unselected samples, 23% represented clinical samples, and 8% represented analogue samples. Overall, quality ratings were high, with an average value of 4.77 (SD = 0.70), and ranged from 2 to 6. All studies were relatively recent, with publication years ranging from 2000 and 2020 and a median year of 2016. In terms of geographic location, approximately 47% of the studies were conducted in North America, 34% were conducted in Europe, 10% were conducted in Asia, 7% were conducted in Oceana, and 1% were conducted in South America.

Primary analyses

Table 2 shows the meta-analytic correlations of worry with each type of rumination, both uncorrected and corrected for measure unreliability.

Global rumination and worry. The meta-analysis for the relationship between global rumination and worry was based on 386 effect sizes from 55,599 unique participants nested within 180 studies. Individual effect sizes varied widely in magnitude, ranging from a small negative correlation, r = −.13 (Devynck et al., 2017), to a near-perfect positive correlation, r = .97 (Radstaak et al., 2014). In the total data set, the association of global rumination with worry was large, positive, and significant, r = .52, 95% confidence interval (CI) = [.50, .54]. After correcting for measure unreliability, we found that this association rose to ρ = .58, 95% CI = [.55, .60]. We performed sensitivity analyses for the uncorrected meta-analysis, varying the within-study effect size correlation that was used to impute sampling covariances (rs = .1–.9 in intervals of .1). These models produced nearly identical results, rs = .51–.52, so we performed all subsequent analyses using the original within-study effect size correlation of .5.

Brooding and worry. The meta-analysis for the association between brooding and worry included 157 effect sizes from 28,749 unique participants nested within 103 studies. Effect size rs ranged from −.05 to .85, with the minimum and maximum estimates coming from a single longitudinal study in which fewer than 10 participants provided data at each time point in question (Thorslund et al., 2020). Overall, brooding showed a significant, large positive association with worry, r = .53, 95% CI = [.51, .55]. After correcting for unreliability, we found that this association rose to ρ = .62, 95% CI = [.60, .65].

Reflection and worry. The meta-analysis for the association between reflection and worry was based on 145 effect sizes from 22,146 unique participants nested within

84 studies. Effects ranged from a moderate negative correlation, $r = -.26$ (Ruiz et al., 2016), to a very large positive correlation, $r = .76$ (Hjemdal et al., 2019). Across all studies, reflection showed a significant, moderate positive association with worry, $r = .28$, 95% CI = [.24, .31]. After correcting for unreliability, we found that the association rose to $\rho = .34$, 95% CI = [.29, .38].

Emotion-focused rumination and worry. The meta-analysis for the relationship between emotion-focused rumination and worry was based on a smaller pool of 31 effect sizes from 6,002 unique participants nested within 23 studies. All effect size *r*s were positive, ranging from .19 (Segerstrom et al., 2000) to .69 (Brown et al., 2020). Overall, emotion-focused rumination showed a significant, large positive association with worry, $r = .51$, 95% CI = [.46, .55], which rose to $\rho = .57$, 95% CI = [.52, .62], after we corrected for unreliability.

Outliers

We examined Cook's distance for each meta-analysis to identify studies that disproportionately influenced the results. We identified 13 and 10 outliers for the uncorrected and corrected global rumination meta-analyses, respectively; six and five outliers for the uncorrected and corrected brooding meta-analyses, respectively; and two outliers for each of the four reflection and emotion-focused rumination meta-analyses.

Reanalyzing the data without these outliers produced nearly the same results. The corrected brooding estimate rose by .01 ($\rho = .63$, 95% CI = [.61, .65]), $t(97) = 63.74$, $p < .001$, $k = 98$, number of effect sizes = 140, as did the corrected reflection estimate ($\rho = .35$, 95% CI = [.30, .39]), $t(81) = 15.88$, $p < .001$, $k = 82$, number of effect sizes = 122. All other estimates were unchanged. Because the results with and without outliers were nearly identical, we retained the outliers in subsequent analyses.

Publication bias

Neither the analysis for global rumination nor the analysis for any rumination subtype showed evidence of publication bias. None of the funnel plots appeared asymmetrical (see Fig. 2). Furthermore, Egger's regression test found no evidence of asymmetry in the funnel plots: The slope of the inverse sample size was nonsignificant in the models for global rumination, $\beta = 0.37$, $F(1, 178) = 0.09$, $p = .769$; brooding, $\beta = 0.25$, $F(1, 101) = 0.05$, $p = .828$; reflection, $\beta = 2.03$, $F(1, 82) = 1.18$, $p = .281$; and emotion-focused rumination, $\beta = -4.73$, $F(1, 21) = 1.99$, $p = .174$.

Next, we examined whether the relationship between worry and rumination depended on the publication status of the study or effect size. Study publication status did not moderate the association of worry with global rumination, $F(1, 178) = 0.49$, $p = .485$; brooding, $F(1, 101) = 2.59$, $p = .111$; reflection, $F(1, 82) = 0.21$, $p = .647$; or emotion-focused rumination, $F(1, 21) = 0.17$, $p = .689$. Similarly, effect size publication status did not moderate the relationship of worry with global rumination, $F(1, 178) = 1.90$, $p = .170$; brooding, $F(1, 101) = 1.35$, $p = .248$; reflection, $F(1, 82) = 0.01$, $p = .924$; or emotion-focused rumination, $F(1, 21) = 0.24$, $p = .628$. These results support the aggregation of published and unpublished data for effect size estimation.

Heterogeneity

We evaluated heterogeneity within and between studies by testing whether a three-level model (which modeled variance both within and between studies) fitted the data better than a two-level model (which freely estimated only one of these variance components). We performed these tests on the uncorrected correlations. For the correlation between global rumination and worry, freely estimating the between-study variance significantly improved model fit, $\hat{\sigma}^2 = .01$, $\chi^2(1) = 15.69$, $p = .001$, as did freely estimating the within-study variance, $\hat{\sigma}^2 = .01$, $\chi^2(1) = 628.07$, $p < .001$, indicating significant heterogeneity at each level. Of the total variance in the global rumination–worry correlation, approximately 26% was explained by variation in effect sizes between studies, 56% by variation within studies, and 19% by random sampling variance. Parallel heterogeneity analyses for the rumination subtype correlations are presented in the Supplemental Material.

I^2_{total}, the total amount of heterogeneity not due to sampling error, was 81% for the global rumination meta-analysis. I^2_{total} was 55%, 82%, and 58%, respectively, for the brooding, reflection, and emotion-focused rumination meta-analyses. Thus, for all four meta-analyses, the sampling variance at Level 1 (the inverse of I^2_{total}) was less than 75%, indicating sufficient heterogeneity within and between studies to justify moderator analyses (see Hunter & Schmidt, 1990). To hold down the family-wise error rate, we tested only moderators of the relationship between global rumination and worry. The sole exceptions to this were gender, which is the moderator that has received the most attention in connection with the rumination subtypes, and measure, because measures of rumination subtypes were available only in the subtype meta-analyses.

Moderation analyses

Study characteristics. Results of moderation analyses appear in Table 3. We began by examining whether the magnitude of the worry–rumination association was moderated by study quality, which we viewed as an important test of the robustness of our findings. The

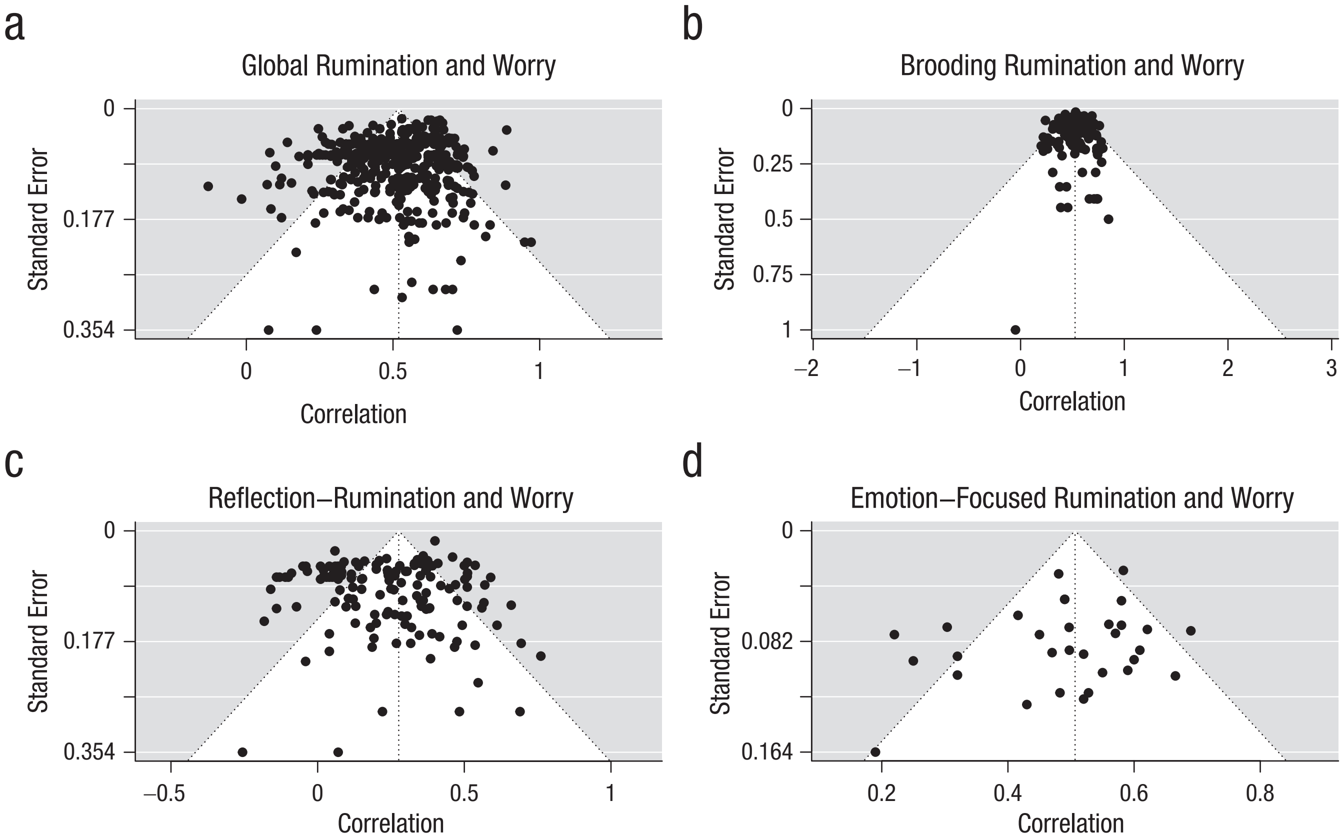

Fig. 2. Funnel plots showing the individual effect size for each study (x-axis) as a function of its standard error (y-axis), separately for the association between worry and (a) global rumination, (b) brooding, (c) reflection, and (d) emotion-focused rumination. Dashed vertical line is the meta-analytically derived effect size. Outer dashed lines indicate a pseudo 95% confidence interval region (triangular white region), within which 95% of studies are expected to lie in the absence of publication bias. This region is drawn around the meta-analytically derived effect size, with bounds equal to ±1.96 standard error depicted on the y-axis.

correlation between worry and rumination did not depend on study quality, $F(1, 162) = 0.73$, $p = .394$. By contrast, the correlation did vary by year of publication, $F(1, 168) = 17.71$, $p < .001$, with studies published more recently finding a stronger association between worry and rumination, $\beta = 0.01$, 95% CI = [0.01, 0.01]. Finally, we tested whether the correlation differed by geographic location, excluding South America, for which only one study with two effect sizes was available. Location was a significant moderator of the worry–rumination relationship, $F(3, 175) = 5.37$, $p = .002$, with a larger correlation reported in studies from North America ($r = .54$) compared with Asia ($r = .46$; $z = -2.60$, $p = .028$) and Oceana ($r = .42$; $z = -2.76$, $p = .028$).

Sample characteristics. We examined moderation by characteristics of the sample, beginning with the demographic characteristics of age, gender (percentage female), and race (percentage White). The correlation between worry and rumination did not differ significantly as a function of these characteristics, all Fs < 3.52, all ps > .062. We followed up our gender analysis by testing whether this characteristic moderated the relationship between worry and brooding, reflection, and emotion-focused rumination, respectively. Gender was not a significant moderator of worry's relationship with any rumination subtype, all Fs < 3.75, all ps > .067.

Next, we considered the composition of the sample. In an initial analysis, we tested whether the correlation between worry and rumination depended on sample type: nonclinical, analogue, or clinical. Sample type was not a significant moderator of the worry–rumination relationship, $F(2, 177) = 2.73$, $p = .068$. In a second planned analysis, we tested whether the proportion of symptomatic participants in the sample moderated the worry-rumination relationship. For this analysis, we examined moderation by sample structure: all nonclinical (sample was unselected or selected for absence of psychopathology), mixed (sample included clinical/analogue participants as well as nonclinical controls), and all symptomatic (sample consisted exclusively of clinical/analogue participants). Sample structure moderated the worry–rumination relationship, $F(2, 177) = 13.81$, $p < .001$, and a significantly stronger association was observed in mixed samples ($r = .63$) than in "pure" nonclinical ($r = .50$; $z = 4.10$, $p < .001$) or symptomatic ($r = .49$; $z = -3.45$, $p = .001$) samples.

Table 3. Results of Analyses Investigating Moderators of the Worry–Rumination Association

Moderator	*k*	ES	*r*/β (*SE*)	95% CI	*t*	*F*[a]	*Q*	*p*
Study quality	164	362	−.01 (.01)	[−.04, .02]	—	*F*(1, 162) = 0.73	*Q*(360) = 3,965.57	.394
Publication year	170	373	.01 (.00)	[.01, .01]	—	*F*(1, 168) = 17.71	*Q*(371) = 1,884.75	< .001
Location						*F*(3, 175) = 5.37	*Q*(380) = 3,756.88	.002
North America	87	236	.54 (.02)	[.51, .57]	35.81			
Asia	17	21	.46 (.03)	[.40, .53]	13.98			
Europe	61	106	.53 (.02)	[.48, .57]	24.20			
Oceana	14	21	.42 (.03)	[.35, .48]	12.83			
Age	174	375	< .01 (.00)	[−.00, .00]	—	*F*(1, 172) = 3.51	*Q*(373) = 1,896.11	.063
Gender	176	381	< .01 (.00)	[−.00, .00]	—	*F*(1, 174) = 0.01	*Q*(379) = 2,042.53	.941
Race	92	250	> −.01 (.00)	[−.00, .00]	—	*F*(1, 90) = 2.26	*Q*(248) = 1,322.15	.137
Sample type						*F*(2, 177) = 2.73	*Q*(383) = 2,033.89	.068
Nonclinical	123	284	.51 (.01)	[.48, .53]	43.71			
Analogue	15	23	.54 (.03)	[.48, .60]	16.60			
Clinical	53	79	.55 (.02)	[.51, .59]	27.52			
Proportion symptomatic						*F*(2, 177) = 13.81	*Q*(383) = 1,951.16	< .001
Nonclinical	123	284	.50 (.01)	[.48, .53]	41.79			
Mixed	30	43	.63 (.02)	[.58, .67]	29.95			
Symptomatic	38	59	.49 (.02)	[.45, .53]	24.50			
Time frame						*F*(1, 178) = 4.42	*Q*(384) = 2,041.86	.037
Trait	175	359	.52 (.01)	[.49, .54]	47.85			
State	11	27	.61 (.04)	[.52, .69]	14.31			
Rumination focus						*F*(1, 178) < 0.01	*Q*(384) = 2,045.41	.998
General	174	370	.52 (.01)	[.50, .54]	51.20			
Specific	9	16	.52 (.06)	[.40, .64]	8.56			
Worry focus						*F*(1, 178) = 5.50	*Q*(384) = 1,982.41	.020
General	173	336	.52 (.01)	[.50, .55]	48.74			
Specific	9	50	.42 (.05)	[.33, .51]	9.24			

Note: Degrees of freedom for the *t* tests were 175 for location, 177 for sample type, and 178 for all other moderators. All *t* tests and tests for residual heterogeneity are significant at $p < .001$. *k* = number of studies, ES = number of effect sizes; CI = confidence interval; *Q* = test for residual heterogeneity.

[a]Omnibus test of all regression coefficients in the model.

Measure characteristics. We tested whether the ways in which worry and rumination were measured moderated the strength of their relationship. We began by examining the time frame of the measures. Because none of the measures in the global rumination–worry meta-analysis used an intermediate time frame of "past week or weeks," we combined "past day" and momentary measures into a single (state) category that we compared with the remaining (trait) category. Time frame was a significant moderator, $F(1, 178) = 4.42$, $p < .037$, with state measures of worry and rumination yielding a stronger association ($r = .61$) than trait measures ($r = .52$).

Next, we considered the focus of the measure. Measures of rumination focused on a specific domain (e.g., sleep, pain) showed the same relationship with worry as general measures of rumination, $F(1, 178) < 0.01$, $p = .998$. In contrast, content-specific measures of worry showed a significantly weaker relationship with rumination ($r = .42$) than general measures of worry ($r = .52$), $F(1, 178) = 5.50$, $p = .020$.

We performed a series of analyses testing whether the association between worry and rumination depended on the scale that was used to measure each construct (see Table 4). We ran these moderation analyses first in the global rumination meta-analysis, which included the largest number of effects and the greatest variety of rumination and worry scales. The magnitude of the global rumination–worry relationship depended on the rumination scale that was used, $F(5, 174) = 5.52$, $p < .001$. Specifically, the Rumination-Reflection Questionnaire (RRQ) Rumination subscale demonstrated a stronger association with worry ($r = .60$) than did the RRS ($r = .51$; $z = 4.45$, $p < .001$), Ruminative Thought Style Questionnaire (RTSQ; r = .49; $z = −2.69$, $p = .043$), or SRRS ($r = .39$; $z = 2.63$, $p = .043$). By contrast, the worry scale that was used did not moderate

Table 4. Moderation of the Worry–Rumination Association by Measure Used

Measure	*k*	ES	*r* (*SE*)	95% CI	*t*	*F*[a]	*Q*	*p*
Global rumination								
Rumination scale						$F(5, 174) = 5.52$	$Q(380) = 3{,}786.38$	< .001
Ruminative Responses Scale	140	239	.51 (.01)	[.49, .53]	45.85			
Rumination-Reflection Questionnaire, Rumination subscale	28	63	.60 (.02)	[.56, .65]	28.55			
Rumination Scale	6	10	.53 (.04)	[.45, .61]	13.48			
Ruminative Thought Style Questionnaire	5	6	.49 (.04)	[.41, .56]	12.58			
Stress-Reactive Rumination Scale	5	12	.39 (.08)	[.24, .54]	5.03			
Other	23	56	.54 (.03)	[.47, .60]	15.64			
Worry scale						$F(2, 177) = 0.38$	$Q(383) = 4{,}152.91$	.688
Penn State Worry Questionnaire	166	276	.52 (.01)	[.49, .54]	42.45			
Worry Domains Questionnaire	5	9	.55 (.04)	[.48, .62]	14.83			
Other	21	101	.54 (.04)	[.46, .62]	12.96			
Reflection rumination								
Rumination scale						$F(1, 82) = 125.43$	$Q(143) = 398.01$	< .001
Ruminative Responses Scale, Reflection subscale	73	104	.33 (.02)	[.30, .36]	21.13			
Rumination-Reflection Questionnaire, Reflection subscale	19	41	.06 (.02)	[.02, .09]	2.96			

Note: Degrees of freedom for the *t* tests were 177 for global worry scale, 82 for reflection rumination scale, and 174 for all other moderators. All *t* tests and tests for residual heterogeneity are significant at $p < .001$. *k* = number of studies, ES = number of effect sizes; CI = confidence interval; *Q* = test for residual heterogeneity.
[a]Omnibus test of all regression coefficients in the model.

the relationship with rumination, $F(2, 177) = 0.38$, $p = .688$.

We also planned to conduct tests of moderation by measure in our rumination subtype meta-analyses. Because neither the brooding nor emotion-focused rumination models demonstrated significant between-study variability, and no studies included multiple brooding or emotion-focused rumination measures, we were unable to evaluate moderation by measure for these rumination subtypes. We were, however, able to evaluate moderation by measure for reflection. We found that the size of the reflection–worry relationship did vary by rumination measure, $F(1, 82) = 125.43$, $p < .001$, with the RRS Reflection subscale demonstrating a stronger correlation with worry ($r = .33$) than the RRQ Reflection subscale ($r = .06$).

Discussion

The present study used meta-analysis to quantify the relationship between worry and rumination. In a data set aggregating 719 effect sizes within 233 studies for 69,305 unique participants, we examined effect sizes for global rumination and for three rumination subtypes. Worry showed a large relationship with global rumination as well as with the brooding and emotion-focused rumination subtypes, producing correlations in the range of $r = .51$ to $.53$, which rose to $\rho = .57$ to $.62$ after we corrected for measurement error. The relationship was still significant, but more moderate, for the reflection subtype of rumination ($r = .28$, $\rho = .34$). The relationship was larger for more recent studies, for studies conducted in North America, for samples containing a mix of nonclinical and symptomatic participants, for state rather than trait measures, for general rather than domain-specific worry measures, and for correlations based on the RRQ Rumination subscale rather than other rumination measures. These findings shed new light on the separability of worry and rumination while revealing conditions that influence their relationship, raising several implications for theory, measurement, and research.

Should worry and rumination be combined?

We set out to inform the debate over whether worry and rumination are best understood as a single overarching construct. Of course, a meta-analytically derived correlation cannot alone determine whether these constructs should be kept separate or combined. A high correlation, though consistent with a single-construct explanation, could instead be indicative of two distinct constructs that strongly influence each other or share a common cause. A correlation also is only one piece of evidence that should be considered alongside evidence provided by other, complementary methods. However, given the inconsistent results yielded by prior descriptive, factor analytic, correlational, and experimental studies, we sought to inform the debate by quantifying the cross-sectional association between self-report measures of worry and rumination.

Our results indicate that although worry and rumination are strongly related, their relationship is no stronger than those previously reported for anxiety and depression in general, nor for GAD and MDD in particular. Indeed, none of our effect sizes were as high as the tetrachoric correlation of .64 previously reported between GAD and MDD (Watson, 2009), although our corrected correlation between worry and brooding came close (ρ = .62, reflecting 38% shared variance). Although there are reasonable clinical and empirical arguments to the contrary, GAD and MDD have been retained as separate syndromes. If we apply the same standard to our meta-analysis results, the associations do not support abandoning the concepts of worry and rumination in favor of a unitary perseverative-thinking dimension.

However, our results need to be considered within the context of a significant limitation: The effect sizes in these meta-analyses are based on existing measures of worry and rumination, and differences in the response scales, wording, and instructions of these measures doubtlessly reduced the correlations between them. For example, the RRS, the most popular measure of rumination, instructs respondents to rate their engagement in each thought/behavior when "feel[ing] down, sad, or depressed." By contrast, the Penn State Worry Questionnaire (PSWQ; Meyer et al., 1990), the most popular measure of worry, instructs respondents to rate "how typical or characteristic the item is of you." Although we corrected effect sizes for attenuation due to measurement error, it was not possible to correct for other features of the scales that were not theoretically relevant to the construct. Had we been able to account for method variance, we likely would have obtained larger correlations.

Measurement artifacts such as these are a perennial problem for efforts to estimate construct-level relationships (Le et al., 2009). As noted earlier, researchers have attempted to address this problem in a few studies by creating standardized measures that preserve the core distinguishing features of worry and rumination while discarding ostensibly insignificant features (Ehring et al., 2011; McEvoy et al., 2010). A drawback of this approach is that it requires researchers to make assumptions about which features are core features and which are trivial. For example, should uncontrollability be retained as a core feature of worry, or should references to uncontrollability be stripped from worry items out of concern that such references add construct-unrelated method variance between worry and rumination items? A serious challenge faced by researchers in the quest to understand worry and rumination is how to define the constructs with enough precision to validly assess their characteristics while avoiding reifying their putative characteristics in the definition process (see Hallion et al., 2022). By taking a meta-analytic approach, we preserved the theoretical distinctions between worry and rumination that are captured by existing measures. These distinctions emerged from clinical accounts of the phenomena and reflect definitions of worry and rumination that are accepted in the field, bolstering the practical applications of our findings. Nevertheless, until better measures become available, the effect sizes yielded by our meta-analyses may be best understood as lower-bound estimates of the relationship between worry and rumination.

Even as lower-bound estimates, our results prompt careful reflection on recent efforts to develop theories, studies, and measures around a unified perseverative-thought construct. The discovery of correlations that fall well below unity suggests that any comprehensive theory will need to specify multifinal, as well as equifinal, pathways that can account for the diverse forms that perseverative thought may take (see Cicchetti & Rogosch, 1996). Relatedly, studies are needed to explain why some individuals worry whereas others ruminate, as well as why individuals worry on some occasions but ruminate on others. At the same time, the large correlations we observed are consistent with the notion of a core process that is shared by worry and rumination (Ehring & Watkins, 2008). Our results are in line with recent bifactor models suggesting that worry and rumination are best represented by a common factor of repetitive negative thinking along with separate worry- and rumination-specific factors (Hur et al., 2017; Topper et al., 2014). Critically, there is evidence that the common factor accounts for most of the variance in psychopathological outcomes (Taylor & Snyder, 2021; Topper et al., 2014), underscoring that the existence of specific factors need not imply that they are clinically significant nor equal in importance to the common factor. Further work is needed to understand the nature of the specific factors and to clarify what value they add, over and above the common factor, for prediction and treatment.

In the meantime, given the sizable correlation between worry and rumination, we encourage researchers to be intentional when deciding which construct to measure. The decision to measure rumination but not worry, for example, should be based on a theoretical rationale for studying rumination specifically rather than repetitive negative thinking more broadly. In most cases, it would be ideal to administer measures of *both* worry and rumination, permitting parallel analyses that move the field toward a better understanding of shared and unique characteristics of these constructs. Alternatively, for researchers who are primarily interested in the common factor of perseverative thinking, it may be most defensible to administer a transdiagnostic perseverative-thinking measure. Continuing to measure only worry or rumination, without a clear reason to hypothesize specificity vis-à-vis the other construct, risks reifying distinctions that artificially divide the literature.

The heterogeneity of rumination

Our findings add to a growing literature documenting the heterogeneity of the rumination construct. In particular, reflection stood out for having a markedly smaller association with worry compared with global rumination or the other rumination subtypes. Despite this, reflection still showed a moderate, positive relationship with worry. Ours is not the first study to associate reflection with maladaptive constructs. For example, Olatunji and colleagues (2013) found moderate, positive relationships between reflection and symptoms of anxiety and depression. Nevertheless, our findings run counter to assertions by some theorists that reflection is a benign or even constructive approach to problems (Nolen-Hoeksema et al., 2008; Trapnell & Campbell, 1999). What drives the association of reflection with worry? Both constructs are cognitive processes that share a focus on the self and are abstract rather than concrete in nature. The constructiveness of reflection was previously far from resolved; a seminal review of different types of repetitive thought listed the consequences of reflection as unknown (Watkins, 2008). Our finding that reflection is associated with worry adds to this conversation and casts further doubt on the notion of reflection as a constructive form of repetitive thought. More research is needed to clarify the relationships among all types of repetitive thought in order to better understand their associations with adaptive and maladaptive consequences.

Notably, the relationship between reflection and worry depended on which rumination measure was used. Worry showed a moderate positive correlation with reflection as measured by the RRS but was uncorrelated with reflection as measured by the RRQ. Closer inspection of the two measures offers clues into these differences. Treynor and colleagues (2003) defined the RRS Reflection subscale as "neutrally valenced" and suggested that it reflects "cognitive problem solving" in response to depressive symptoms (p. 251, p. 256). However, it may be more precise to describe the subscale's valence as undefined, as the items reference the acts of thinking or analyzing but do not describe the resulting thoughts themselves. Importantly, the RRS instructs respondents to indicate what they do when feeling down, sad, or depressed. In the context of depression, it is unlikely that the content of these reflective thoughts is truly neutral. In fact, three reflection items reference searching for "why" one is depressed, which is analogous to the abstract-evaluative form of rumination defined by Watkins and Moulds (2005) in that it is focused on the causes and meanings of symptoms. Abstract-evaluative thinking is known to be associated with negative cognitive and affective outcomes (Watkins, 2008).

In contrast, the RRQ Reflection subscale was designed to measure intellectual self-attentiveness (Trapnell & Campbell, 1999). This form of reflection extends beyond self-focused thinking into deep thinking on other topics (e.g., "I love to meditate on the nature and meaning of things," "Philosophical or abstract thinking doesn't appeal to me that much" [reverse scored]). The items seem to reference, beyond a tendency toward introspection, taking pleasure in the process (e.g., "I love analyzing why I do things," "My attitudes and feelings about things fascinate me"). Nevertheless, we included this subscale in our analyses for the following reasons: (a) The authors combined rumination and reflection subscales in the same measure, indicating that they viewed these constructs as related; (b) reflection is significantly associated with both rumination and neuroticism, suggesting shared variance among these constructs (Trapnell & Campbell, 1999); (c) nominally, the RRQ Reflection subscale purportedly indexes the same construct or subconstruct as the RRS Reflection subscale; and (d) reflection is conceptualized as at least partly adaptive by the authors of both measures (Nolen-Hoeksema et al., 2008; Trapnell & Campbell, 1999). Our results suggest that introspection experienced as pleasant may not be unconstructive, although we lack evidence that this form of thinking is adaptive per se (as a negative association with worry might indicate). By contrast, abstract thinking about negative mood, even if the thoughts themselves are neutral, appears to be detrimental.

We found evidence for heterogeneity not only in reflection but also in rumination more broadly. Correlations with worry ranged from moderate to large across five measures of global rumination, underscoring that rumination measures are not interchangeable and that different measures reflect different conceptualizations of the construct. For example, the RRS, which originated from the depression literature, assesses thinking in the context of depressed mood, whereas the RRQ Rumination subscale, which stems from the personality literature, assesses thinking more generally. The latter measure correlated more strongly with worry, perhaps because it is less contaminated by depressive affect. In fact, all significant post hoc comparisons involved the RRQ Rumination subscale, hinting that this measure was driving the omnibus moderation finding. At the other extreme, the SRRS had the smallest correlation with worry. The SRRS emphasizes thought content following stressful events (e.g., "Think about how the stressful event is all your fault"), whereas the RRQ Rumination subscale emphasizes process characteristics such as uncontrollability and repetitiveness (e.g., "Sometimes it is hard for me to shut off thoughts about myself"). Larger effects for the RRQ Rumination subscale could result from process (rather than content) similarities between worry and rumination or could reflect greater concordance of worry with rumination that is framed as a habitual, stable quality of the individual rather than as a reaction to discrete, stressful events. These findings stress the importance of careful measure selection that considers the features of rumination being assessed while highlighting the need for greater consensus on which features of rumination are most important to assess.

Other moderators of the worry–rumination relationship

Our analyses revealed several other moderators of the relationship between worry and global rumination. The relationship was stronger in studies that were published more recently, despite the fact that all studies were conducted over a period of only about two decades. Although worry and rumination have been studied individually for many decades, studies that have measured both constructs and investigated their relationship are quite recent. The advent of such studies coincides roughly with when theorists began to highlight the conceptual overlap between these constructs. Our finding of moderation by publication year may reflect methodological changes in studies over time, particularly the increased use of state (daily or momentary) measures, which were associated with higher effect sizes in our analysis. It is also possible that declining stigma and increasing willingness to report psychopathology, coupled with rising levels of anxiety and depression (Compton et al., 2006; Twenge et al., 2010), led to a widening range of scores on worry and rumination measures, thus reducing range restriction and translating into larger effect sizes. The relationship was also stronger in studies conducted in North America compared with Asia and Oceana. This is in line with meta-analytic findings of larger effect sizes for psychological and behavioral (though not biological) measures in the United States relative to other countries (Fanelli & Ioannidis, 2013), which also could have reduced range restriction, thus resulting in a larger correlation. Further research is needed to determine whether these patterns represent substantive changes in the relationship between worry and rumination over time and across cultures or whether they result from systematic differences in how worry and rumination have been measured and studied in these contexts.

The worry–rumination relationship was not significantly moderated by the age, gender, or racial composition of the sample. Given a well-established literature on gender differences in repetitive negative thinking, especially in the brooding form of rumination (Nolen-Hoeksema, 1987), we conducted follow-up analyses by subtype and found no evidence that gender moderated the relationship of worry with any form of rumination, increasing confidence in the robustness of this result. Moreover, contrary to results reported by Naragon-Gainey and colleagues (2017) using a much smaller data set, we found no evidence that worry and rumination were more strongly correlated in nonclinical than clinical samples. Instead, the relationship was stronger in mixed samples (those combining nonclinical with clinical participants) than in "pure" nonclinical or clinical samples. Given that mixed samples are likely to include the widest range of worry and rumination scores, they would be expected, for strictly statistical reasons, to yield higher correlations than samples in which the range is restricted. Our results encourage the use of mixed samples in future studies to maximize statistical power, capitalizing on the broad representation of mild to severe levels of worry and rumination to probe their relationships with each other and with external constructs.

The worry–rumination relationship was larger when using state measures rather than trait measures. This may reflect the influence of a strong, shared situational context on both cognitive processes. Researchers are more likely to administer state measures after an experimental manipulation (e.g., affect induction) that may heighten negative thinking in general and produce a less differentiated response compared with less acute situations. Another possibility is that state measures, which often assess worry and rumination using the same response scale and instructions, may minimize scale-specific variance relative to trait measures. Given that trait measures are more susceptible to metacognitive appraisals and retrospective recall biases that are known to bias reporting (e.g., Mathersul & Ruscio, 2020; Wells, 2013), we speculate that state measures may offer a purer assessment of thoughts, and a more accurate estimate of the worry–rumination relationship, than trait measures.

Finally, the relationship between worry and rumination depended on how worry was defined. Worry about a specific topic (e.g., parenting, illness) had a significantly weaker association with rumination than did general worry. It is possible that these domain-specific measures index a type of worry reflecting a proportionate, situationally bound, transient response to a defined stressor (e.g., parenting concerns, medical diagnosis) that is more normative than persistent worry about a wide range of topics. A different possibility is that measures of general worry—in particular, the widely used PSWQ—emphasize process features that overlap more strongly with rumination than the content features emphasized by domain-specific measures. Interestingly, the same pattern did not arise for domain-specific versus general measures of rumination. This may suggest that rumination, even when narrowly focused, is less normative or more pathological than worry.

Limitations

Several caveats must be considered when interpreting the present results. We included a wide array of worry and rumination measures, which increased confidence that our estimates capture meaningful properties of worry and rumination rather than idiosyncratic properties of one or two measures. However, all of the measures were self-report questionnaires; as such, all were subject to limitations of this measurement approach, and their shared method variance may have inflated estimates of the worry–rumination relationship. Furthermore, worry was measured more uniformly than rumination, influencing our tests for moderation by measure. Specifically, whereas five rumination measures contributed enough effect sizes to be examined separately in the moderation analyses, only two worry measures—the PSWQ and Worry Domains Questionnaire—met our threshold for inclusion as separate categories, requiring us to combine all other worry measures into an "other" category. The heterogeneity of the "other" category, which contained 101 effect sizes, may have obscured differences in the worry–rumination relationship as a function of worry measure.

Although our conclusions were informed by analyses evaluating moderation by identity variables (age, gender, race), our analyses provided a fairly crude test of these identity factors, as we had access only to sample-level—rather than participant-level—demographic characteristics (mean age, percentage female, percentage White). We could not test for moderation by finer-grained racial-ethnic categories or by other variables of interest (e.g., socioeconomic status, gender identity), as they were not commonly reported in this literature. Notably, we were unable to distinguish between sex and gender (Hyde et al., 2019), as very few studies made this distinction; consequently, our "gender" analyses likely tested some ambiguous mixture of gender and sex. More research is needed to examine the worry–rumination relationship in well-characterized, sociodemographically diverse samples. This is especially important in light of work that has identified rumination as a node in the nomological network of minority stress. For example, sexual minority individuals are exposed to higher rates of stress and experience higher rates of emotional disorders compared with other people in their community (Cochran et al., 2003; Hatzenbuehler, 2009; Herek & Garnets, 2007); rumination mediates the association between minority stress and psychological distress and, accordingly, is likely to be heightened among sexual minority individuals (Liao et al., 2015; Szymanski et al., 2014). Higher rates of rumination in members of minority groups could lead to a wider range of scores and, in turn, a stronger relationship between worry and rumination in more representative samples.

We were able to perform well-powered tests of the moderating effect of gender, given that studies generally included relatively equal representations of male and female participants. In contrast, our data were not ideally suited to exploring the moderating effect of age, as relatively few studies measured both worry and rumination in adolescents, and very few studies measured these constructs in younger children. For example, in the meta-analysis for global rumination and worry, only 7% of the studies involved adolescents, and no studies reported a mean age below 10 years. Even fewer studies measured pubertal indicators, so we were unable to test for moderation by development. More research is needed on the worry–rumination relationship in youth, ideally assessing pubertal stage as well as chronological age given the important role of pubertal timing in the development of psychopathology (Graber, 2013; Negriff & Susman, 2011). Evidence that rumination mediates the association between early pubertal timing and depression specifically for girls (Alloy et al., 2016; Hamilton et al., 2015) suggests that future research syntheses should explore more complex interactions among gender and developmental variables in connection with the worry–rumination relationship, perhaps using techniques such as mega-analysis that can accommodate participant-level data (Curran & Hussong, 2009).

Finally, given that few studies have reported longitudinal associations between worry and rumination, we focused on cross-sectional associations in the current article. These results cannot shed light on the temporal relationships between the constructs. Longitudinal research is needed to help explain the large correlations observed here, including the possibility that worry and rumination serve as risk factors for one another. Given that rates of depression increase dramatically in adolescence, especially among girls (Salk et al., 2017), and that worry and rumination predict depression onset during this period (Young & Dietrich, 2015), prospective studies extending into adolescence may be especially valuable for revealing developmental trajectories (e.g., does early-life worry serve as a risk factor for later rumination?) and their mapping to disorder onsets.

Overall, the current study reveals a strong relationship between worry and rumination, but not one so strong as to support the argument that these constructs are redundant. Our findings introduce caution into emerging theories and measures of perseverative thought, suggesting that proponents of these efforts may need to consider not only the shared features of worry and rumination but also the features that distinguish them. Our findings also highlight the heterogeneity of the rumination construct, underscoring that efforts to understand rumination's relationship to worry must take into account both the type of rumination and the measure used to operationalize it. Rather than perpetuating divisions in the literature by studying one construct without the other, we encourage researchers to study both constructs together so that their common and distinct features—and the relative importance of those features—can be elucidated.

Transparency

Action Editor: Steve S. Lee
Editor: Jennifer L. Tackett
Author Contributions

Elizabeth C. Stade: Conceptualization; Data curation; Formal analysis; Funding acquisition; Investigation; Methodology; Project administration; Resources; Software; Validation; Visualization; Writing – original draft; Writing – review & editing.

Ayelet Meron Ruscio: Conceptualization; Methodology; Resources; Supervision; Writing – review & editing.

Declaration of Conflicting Interests

The author(s) declared that there were no conflicts of interest with respect to the authorship or the publication of this article.

Funding

This work was supported by the National Science Foundation Graduate Research Fellowship Program under Grant No. DGE-1845298 (to E. C. Stade). Opinions, findings, conclusions, and recommendations expressed in this article are those of the authors and do not necessarily reflect the views of the National Science Foundation.

Open Practices

R code has been made publicly available via OSF and can be accessed at https://osf.io/jhm2n/.

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Acknowledgments

We gratefully acknowledge the numerous researchers who contributed unpublished data to this project.

Supplemental Material

Additional supporting information can be found at http://journals.sagepub.com/doi/suppl/10.1177/21677026221131309

Note

1. Corrected correlation coefficients are adjusted for measurement unreliability and follow the same interpretation guidelines as standard correlation coefficients.

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