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Performance and belief-based emotion regulation capacity and tendency: Mapping links with
cognitive flexibility and perceived stress

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Abstract

Cognitive reappraisal is among the most effective and well-studied emotion regulation strategies humans have at their disposal. Here, in 250 healthy adults across two preregistered studies, we examined whether reappraisal capacity (the ability to reappraise) and tendency (the propensity to reappraise) differentially relate to perceived stress. We also investigated whether cognitive flexibility, a skill thought to support reappraisal, accounted for associations between reappraisal capacity and tendency, and perceived stress but found no evidence for this hypothesis. Both Studies 1 and 2 robustly showed that reappraisal tendency was associated with perceived stress, whereas a significant relationship between reappraisal capacity and perceived stress was only observed in Study 2. Further, Study 2 suggested that self-reported beliefs about one's emotion regulation capacity and tendency were predictive of wellbeing, whereas no such associations were observed with performance-based assessments of capacity and tendency. These data suggest that self-reported perceptions of reappraisal skills may be more predictive of wellbeing than actual reappraisal ability.

Keywords: Cognitive Reappraisal, Emotion, Emotion Regulation, Wellbeing, Open Science

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Cognitive reappraisal—reframing a stimulus so as to alter its emotional import—is one of the most widely studied emotion regulation strategies, in part because of its associations with numerous indicators of wellbeing and adjustment (Gross, 2015). Examples of such indicators include eating habits, decision making under uncertainty, mental health outcomes such as mood disorder symptomatology, and the measure tested in the present study—perceived stress (Denny & Ochsner, 2014; Giuliani & Pfeifer, 2015; Haines et al., 2016; Heilman, Crisan, Houser, Miclea, & Miu, 2010; Panno, Lauriola, & Figner, 2013; Zilverstand, Parvaz, & Goldstein, 2016). Though there are limitations to reappraisal (Aldao, Nolen-Hoeksema, & Schweitzer, 2010; Ford & Troy, 2019; Webb, Miles, & Sheeran, 2012), there is strong evidence to suggest it is a useful tool for maintaining both mental and physical health. And yet, the field currently lacks a mechanistic understanding of when and how reappraisal relates to wellbeing. Relatedly, inconsistent methodological choices in prior studies, along with unclear psychometric properties of commonly used measures, stand in the way of a mechanistic understanding of reappraisal's link to wellbeing and likely explain existing inconsistencies in the literature. The current report attempted to help resolve both issues of mechanism and psychometric understanding as they relate to cognitive reappraisal and wellbeing.

Towards a Mechanistic Understanding of Reappraisal and Wellbeing.

The present study sought to provide a better mechanistic understanding of how reappraisal begets wellbeing – specifically, how reappraisal relates to a common disruptor of wellbeing, perceived stress – by answering three specific questions across two studies: (i) Does the relationship between reappraisal and wellbeing depend on how often people reappraise (i.e.,

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their reappraisal “tendency”) or how effectively they reappraise (i.e., their reappraisal “capacity”)? (ii) Does it matter how people actually use reappraisal, or merely how they believe they use it? (iii) Does the link between reappraisal and wellbeing demand strong executive functioning? Here, we briefly introduce these open questions and subsequently review them in depth.

First, little is known about how individual differences in reappraisal capacity, one’s ability to reappraise, and reappraisal tendency, how frequently one chooses to reappraise, independently affect wellbeing (McRae, 2013; Silvers & Guassi Moreira, 2017). This is partially because only 4 studies to date have assessed both tendency and capacity in the same individuals and linked them to wellbeing (Ford, Karnilowicz, & Mauss, 2017; McRae, Jacobs, Ray, John, & Gross, 2012; Troy, Ford, McRae, Zarolina, & Mauss, 2017; Troy, Wilhelm, Shallcross, & Mauss, 2010). The results of these studies have yielded somewhat inconsistent findings (correlations ranging from .01 - .80, mean = 0.33, SD = .30), making it unclear precisely how capacity and tendency independently relate to adjustment.

Second, and relatedly, it is poorly understood how beliefs about one’s capacity and tendency to reappraise and one’s actual capacity and tendency differentially relate to wellbeing. In other words, we do not know whether it matters more that someone thinks they are good at reappraising or use reappraisal frequently, or if they actually are these things. This knowledge gap arises in part because different measurement modalities – performance (e.g., computerized tasks) and belief (e.g., global self-report inventories) measurements – are typically used to assess reappraisal capacity and tendency, respectively. Performance- and belief- based reappraisal capacity and tendency have rarely, if ever, been each measured in the same individuals, making

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it difficult to ascertain whether objective performance or one's self-reported beliefs about reappraisal capacity and tendency are more predictive of wellbeing.

Third, although a small number of studies have examined how different executive functions (e.g., cognitive flexibility) give rise to the ability to reappraise (McRae et al., 2012), none have assessed whether these executive functions account for reappraisal's associations with indicators of wellbeing like reduced perceived stress. Put differently, although reappraisal is thought to rely upon the engagement of executive functions—top down mental processes necessary for coordinating goal-directed behavior (Diamond, 2013) – it remains unknown whether these executive functions help explain ties between reappraisal and wellbeing. It is further unknown whether reappraisal's positive effects on wellbeing are driven by one executive function, or are due to interactive effects between different executive functions.

Addressing these three outstanding questions has important implications for basic and applied affective science. A better mechanistic understanding of reappraisal promises to both refine formal theories of emotion and inform interventions whose goal is to improve wellbeing by virtue of strengthening emotion regulation skills.

Distinguishing Reappraisal Capacity from Tendency in Predicting Wellbeing.

Some work relating reappraisal to wellbeing outcomes has focused on the *capacity* (sometimes referred to as 'reappraisal ability') to reappraise whereas other work has examined the *tendency* (sometimes referred to as 'habitual reappraisal use') to do so (Doré, Silvers, & Ochsner, 2016). These are, theoretically and empirically, distinct concepts (Doré et al., 2016; McRae, 2013; Troy et al., 2010; Silvers & Guassi Moreira, 2017): an individual may not

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reappraise effectively but still attempt to reappraise regularly, whereas another individual may be quite effective at reappraising yet rarely do so.

Given reappraisal's popularity as a research topic, surprisingly few studies have directly tested the extent to which reappraisal capacity and tendency are related to one another (Silvers & Guassi Moreira, 2017), let alone whether capacity and tendency are differentially associated with wellbeing. Among those that have examined links between reappraisal capacity and tendency, McRae and colleagues (2012) reported a modest, significant positive correlation ($r = 0.24$) whereas Troy et al (2010) found no significant relationships between capacity and tendency ($r_s = .12, .21$). Others (Ford et al., 2017; Troy et al., 2017) have found both significant and non-significant correlations between capacity and tendency ($r_s = .01 - .80$) that varied between different types of measurements (e.g., self-reports, daily diaries, laboratory tasks). To further complicate matters, much of the research in this area (e.g., Troy et al., 2010; McRae et al., 2012) has been conducted in samples characterized by narrow demographics (e.g., samples comprised of only one sex, poor representation of racial/ethnic minorities, etc.), making it difficult to ascertain how results generalize to broader populations.

Given the fact that little work has assessed the association between reappraisal capacity and tendency, it is perhaps unsurprising that even less work has tested whether capacity and tendency are differentially associated with wellbeing. Several studies have shown that reappraisal tendency is positively related to measures of wellbeing such as better life satisfaction, social functioning, mental health, and dampened physiological responses during anger (Gross & John, 2003; John & Gross, 2004; Mauss, Cook, Cheng, & Gross, 2007). Far less research has examined the relationship between reappraisal capacity and wellbeing, though one study did find that capacity is inversely associated with depressive symptoms (Troy et al., 2010). To our

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knowledge, four studies have examined links between both reappraisal capacity and tendency, and measures of wellbeing (e.g., McRae et al., 2012; Ford et al., 2017; Troy et al., 2010, 2017). These studies have yielded mixed results with regards to whether capacity and tendency both relate to wellbeing. One found that capacity and tendency share similar relationships with wellbeing (McRae et al., 2012), whereas another found that reappraisal tendency was robustly associated with wellbeing while capacity's association with wellbeing was inconsistent (Troy et al., 2017). A third study reported that capacity and tendency were uncorrelated, a non-significant main effect of capacity on wellbeing, a moderating role of capacity in predicting wellbeing, and did not report on the relationship between tendency and wellbeing (Troy et al., 2010). And yet, another study argued that there may be interactive effects between capacity and tendency (Ford et al., 2017). In sum, there is a possibility that the relationship between reappraisal and wellbeing outcomes differ for capacity and tendency but additional research is needed to test this.

Parsing Beliefs about Reappraisal Skills from Actual Performance.

The fact that research studies rarely examine reappraisal capacity and tendency in the same individuals is complicated by another methodological issue: researchers also seldom take into consideration whether they are collecting performance-based or belief-based measures of capacity and tendency. The former refers to one's objective ability to regulate emotions via reappraisal (capacity) or their objective reappraisal frequency (tendency); the latter refers to how one perceives their ability to use reappraisal or their tendency to do so. While the term 'belief-based' can refer to many aspects of one's self-referential thoughts—here, we use the term to refer to perceptions of one's own emotion regulation skills (Fugate, Gouzoules, & Feldman Barrett, 2009), as opposed to higher order, meta-beliefs about the properties of emotions (Ford & Gross, 2018). Typically, as it relates to reappraisal research, researchers use global self-report

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methodologies to index belief-based reappraisal skills (e.g., Likert-scale questionnaires) and momentary self-report techniques to tap performance-based reappraisal skills (computerized tasks that rely on participant responses). For convenience, we assume our global and momentary self-report measures amount to adequate belief- and performance-based measures. We outline the limitations of this assumption in the discussion section of the manuscript.

Assessments of capacity and tendency tend to vary according to methodology (McRae, 2013). Capacity is most commonly assessed through computerized tasks that involve measuring how well one reappraises by comparing ratings of affect taken during reappraisal to affect during a meaningful baseline. By contrast, tendency is most commonly assessed with self-report questionnaires that measure how frequently one believes they reappraise. Recent reports examining performance and belief-based estimates of self-regulation skills such as impulse inhibition and discounting tendencies suggest that different modes of assessment can yield significantly different results in terms of psychometric properties (e.g., test-retest reliability) and the ability to predict real-world adjustment outcomes (e.g., obesity, drug use, etc.) (Eisenberg et al., 2019; Enkavi et al., 2019). Even though belief-based measures of capacity (Troy et al., 2017, 2010) and performance-based measures of tendency (Hay, Sheppes, Gross, & Gruber, 2015; Scheibe, Sheppes, & Staudinger, 2015) exist, they are not as commonly used relatively to their traditional counterparts. Moreover, no study to our knowledge has directly compared associations between capacity, tendency, and stress using multiple measurement modalities.

Executive Functions, Reappraisal, and Wellbeing.

Dominant theoretical models posit that reappraisal relies on key executive functions that enable the formulation and deployment of a reappraisal (Buhle et al., 2014; Ochsner, Bunge, Gross, & Gabrieli, 2002; Ochsner, Silvers, & Buhle, 2012). Having received accumulating

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support over the years (Ochsner et al., 2012), these theories highlight that reappraisal is akin to a machine that operates on interactions between many moving cognitive parts (i.e., executive functions). What is currently missing from the literature is the knowledge of *which* executive functions (i.e., parts) are most crucial for reappraisal to function and support wellbeing. The current investigation sought to examine whether two executive functions, working memory and cognitive flexibility, may partially explain reappraisal's association with wellbeing. As described below, we further hypothesized that cognitive flexibility might be more privileged over working memory.

Working Memory. Working memory is defined as the ability to temporarily store, recall, and manipulate information in memory (Baddeley, 2012). Several theoretical accounts posit that working memory is a multi-component executive function that acts as a hub in a broader network with other executive functions (Baddeley, 2012; D'Esposito, 2007). Thus, we reasoned that working memory might help account for reappraisal's association with wellbeing in part because of its relationship with *other* executive functions. Working memory affects performance of other cognitive processes such as control of visual and spatial attention (Conway, Cowan, & Bunting, 2001; de Fockert, Rees, Frith, & Lavi, 2001), goal-directed planning (Kane et al., 2007), and inhibition (Kane & Engle, 2003). It follows that if working memory is compromised, then the ability to skillfully deploy other executive functions implicated in reappraisal might also be impacted. Consistent with this, prior work has suggested that individual differences in working memory track with cognitive reappraisal ability (Schmeichel, Volokhov, & Demaree, 2008; Scult, Knodt, Swartz, Brigidi, & Hariri, 2017).

Cognitive Flexibility. We define cognitive flexibility as the ability to dynamically update representations of a stimulus in accordance to external demands (Scott, 1962). Though fewer

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studies have examined links between reappraisal and cognitive flexibility than with working memory, there is some evidence that cognitive flexibility is also associated with reappraisal (McRae et al., 2012). We reason that cognitive flexibility is likely more crucial than working memory for supporting reappraisal and associated wellbeing for two main reasons. First, the very act of changing the way one thinks about a stimulus (i.e., reappraisal) is by definition an act of cognitive flexibility (Gross, 1998; Malooly, Genet, & Siemer, 2013). Studies suggest that because flexibility more closely mirrors the cognitive processes implicated with reappraisal, it may be more essential to cognitive reappraisal than working memory (Malooly et al., 2013; Siemer, Yoon, & Joormann, 2010). Second, though executive functions are broadly seen as being integral to ensuring wellbeing (e.g., Diamond, 2013; Insel, Morrow, Brewew, & Figueredo, 2006; Jacobson, Williford, & Pianta, 2011; Pennington & Ozonoff, 1996), cognitive flexibility appears to be more robustly related to markers of wellbeing than working memory is (Alexander, Hillier, Smith, Tivarus, & Beversdorf, 2007; Browning, Behrens, Jocham, O'Reilly, & Bishop, 2015).

For these reasons listed above, a key focus of our study was determining whether cognitive flexibility accounted for the relationship with reappraisal and wellbeing, over and above working memory. We extended prior research by examining whether cognitive flexibility plays a privileged role in supporting associations between capacity and tendency, and wellbeing, over and above working memory. Though other studies have examined reappraisal, flexibility, and working memory in the same individual, our questions remain untested, given that prior work (i) has looked at reappraisal's relationships with working memory without controlling for cognitive flexibility, or vice versa and (ii) did not test whether either accounted for associations between reappraisal and wellbeing (McRae et al., 2012).

Present Work.

In Study 1 of the present manuscript, we compared reappraisal capacity and tendency using historically dominant assessments (i.e., performance-based and belief-based, respectively). We then expanded upon this work in Study 2 by measuring reappraisal capacity and tendency with both traditional and non-traditional methods (i.e., using both belief- and performance-based measures for tendency and capacity). Across both studies, we used a combination of multiple regression and bootstrapping approaches to estimate the independent contributions that capacity and tendency exert on an important measure of wellbeing – perceived stress.

Our decision to use perceived stress as our primary outcome measure was motivated by three factors. First, stress is ubiquitous, with some estimating that nearly 60% of adults suffer from elevated levels of subjective stress (Wiegner, Hange, Björkelund, & Ahlborg, 2015). Second, stress is strongly associated with other serious health and psychological problems (Din-Dzietham, Nembhard, Collins, & Davis, 2004; Nielsen, Kristensen, Schnohr, & Grønbæk, 2008; Rod, Grønbæk, Schnohr, Prescott, & Kristensen, 2009). Lastly, perceived stress has been previously used as a measure of wellbeing in the reappraisal literature and we were thus able to build upon prior findings by using it in the present sample (Denny & Ochsner, 2014; Troy et al., 2017).

Psychometric Considerations

Prior to presenting the methods and results of Studies 1 and 2, it is useful to briefly discuss the psychometric properties of the measures used in these studies. As described in subsequent sections, the results of Study 1 and (to a lesser extent) Study 2 left us with several important methodological questions about our measures. In planning these studies, we selected

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measures that have been used extensively in the prior literature, but were never psychometrically validated. To this end, we performed supplemental analyses to examine surface-level psychometric properties of our measures. These were not meant to be definitive analyses but were instead intended to detect any glaring deficiencies in psychometric properties. Although these analyses are useful and relevant, they are beyond the immediate scope of our primary goals for this report. Therefore, we report them in great detail in the supplement. The main point to take away from these analyses is that all but one (our computationally-based metric of cognitive flexibility being the only exception) of the measures range between fair (i.e., reasonable) to excellent in their psychometric properties.

Study 1: Testing Associations between Reappraisal Capacity, Reappraisal Tendency, Cognitive Flexibility, and Executive Function

To investigate the research topics described above, we constructed a set of regression models. These models investigated whether greater reappraisal capacity or tendency predicted less perceived stress, and then tested indirect effects to assess whether cognitive flexibility accounted for the association between reappraisal capacity or tendency and perceived stress. Notably, models involving indirect effects do not necessarily make assumptions of temporal causality between either facet of reappraisal, flexibility, and perceived stress. The aim is instead to understand whether the relationship between reappraisal and perceived stress is explained by flexibility (while controlling for working memory), agnostic to the direction of the causal arrows. We pre-registered all aspects of our study, including hypotheses, on the Open Science Framework (osf.io/f5aak). As noted below, our confirmatory hypotheses centered on cognitive flexibility while our exploratory analyses centered on contrasting reappraisal capacity and tendency.

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Our confirmatory hypotheses are as follows¹. Note that we controlled for working memory while testing these hypotheses, as indicated in our pre-registration:

H1. Cognitive flexibility will statistically account for the association between cognitive reappraisal capacity and perceived stress.

H2. Cognitive flexibility will statistically account for the association between cognitive reappraisal tendency and perceived stress.

Our post-hoc, exploratory analyses are as follows:

- (i) We first tested the effects of reappraisal capacity and tendency on perceived stress while controlling for one another.
- (ii) We evaluated additional models that reflect alternative theoretical relationships (e.g., moderating versus indirect relationships) between capacity, tendency, flexibility, and perceived stress.
- (iii) We used bootstrapping to compare model fits of associations between reappraisal capacity and perceived stress, as well as between reappraisal tendency and perceived stress.

Methods

Participants

We recruited participants from the undergraduate psychology subject pool at the University of California, Los Angeles. As part of the study's pre-registration, we set an *a priori* sample size of 125 subjects. This number was determined by considering the sample sizes of prior work and logistical constraints surrounding the current study. No formal power analysis

¹ The manuscript originally described cognitive flexibility as 'mediating' the association between capacity and tendency. Though our analyses remain the same, we have changed the language here to reflect the fact that we did not want to mislead readers into thinking we were making assumptions of temporal causality. Mediation models can still be informative in absence of these assumptions.

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was performed. It follows that our data collection stoppage rule was to terminate data collection upon running 125 subjects. Notably, our sample size is significantly larger than several prior studies with similar research questions (e.g., McRae et al., 2012), suggesting our predetermined sample size is sufficient. Our final sample of 125² undergraduates included 94 Females (75.2%) and had a mean age of 21.49 years ($SD = 2.5$; Range = 19-39). Racially, 36.8% of our participants were Asian, 32% were Caucasian, 5.6% were mixed race, 0.8% were African American, 0.8% were Native American, 14.4% identified as ‘Other’ and 9.6% declined to respond. Ethnically, 19.2% of our sample identified as Latinx. Though not explicit in our study protocol, English proficiency was a de facto requirement. Written consent was obtained for all participants in accordance with policies of the UCLA Institutional Review Board. Data, materials, and code are publicly available on the OSF (osf.io/97kqy).

Procedure

Participants responded to a study advertised on the subject pool website as investigating the link between emotions and decision making. Participants completed three computerized experimental measures (assessing reappraisal capacity, cognitive flexibility and working memory), a survey with self-report measures of interest (including reappraisal tendency and perceived stress), and other exploratory measures. Participants were greeted and consented upon arrival, completed the computerized measures, and then filled out the survey. All measures are detailed in our pre-registration document, hosted on the OSF. Computerized measures reported in this study were programmed in PsychoPy (version 1.82.01; Peirce, 2007). Another measure was collected to answer a set of orthogonal research questions to be addressed in a separate manuscript. The order of computerized measures was counterbalanced across subjects.

²Two participants failed to properly complete a measure of working memory (described later). Sample sizes for analyses that use this measure are thus 123.

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Participants were trained in accordance with well-validated procedures (McRae et al., 2012; Silvers et al., 2012) by an experimenter and were not allowed to proceed until demonstrating they properly understood the task. The experimenter remained present in the room during the experimental session in order to answer questions and unobtrusively monitor participants to ensure they remained focused. The computerized measures ranged between approximately 4-12 minutes in duration. Sessions typically took between 45-70 minutes to complete, including consenting participants, training them on how to complete the tasks, and completing questionnaire measures.

Measures

Reappraisal Capacity. Reappraisal (performance-based) capacity was assessed using a computerized paradigm adapted from prior reappraisal protocols (McRae et al., 2012; Silvers et al., 2012; Silvers, Weber, Wager, & Ochsner, 2015). During the task, participants were presented with a series of neutral or negatively valenced images drawn from the Open Affective Standardized Image Set (Kurdi, Lozano, & Banaji, 2017). Neutral images included depictions of inanimate objects, landscapes, and social interaction. Negative images included depictions of injuries, destruction, animal waste, and arguments. Each image was associated with a ‘Look’ or ‘Decrease’ cue. ‘Look’ cues instructed participants to passively view the stimuli. ‘Decrease’ cues were only paired with negative images, and meant participants were to reinterpret the stimulus in a way that helped decrease their emotional response to it. Each image was only shown once and paired with either the ‘Look’ or ‘Decrease’ cue, helping reduce the likelihood of habituation. ‘Decrease’ and ‘Look – Negative’ images were matched on valence.

After the stimulus was displayed, participants were prompted to rate their negative affect along a 5-point Likert scale (1 = “Neutral”, 5 = “Very Negative”). Participants were trained

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extensively to do this by thinking about how the possible antecedents, outcomes or reality of the event in the picture could actually be different than what was ostensibly depicted (Silvers et al., 2015). Individuals were guided through a series of PowerPoint slides giving them step-by-step instructions on what the task would look like and what they were to do at each part of a trial (e.g., cue, stim, rate). Afterwards, participants completed a brief practice session in which the experimenter walked them through two trials and they then completed four trials on their own. To avoid confounds due to potential demand characteristics, participants were made aware that it was possible to try to decrease negative emotion and still feel very negative.

Each trial could last a maximum of 12 seconds—cues were shown for 2 seconds, stimuli for 7 seconds, and the response scale was displayed for 3 seconds at most (terminated by user response; Mean response time = 888.96 ms, SD = 316.51 ms). There were 20 trials for each condition (Look Negative, Look Neutral, Decrease Negative), totaling 60 trials for the entire task. Reappraisal capacity, defined as the percent change in negative affect between the average affect ratings in the Look Negative and Decrease Negative conditions, was calculated as follows.

$$\text{Reappraisal Capacity} = ([\text{Look Negative} - \text{Decrease Negative}] / \text{Look Negative} \times 100)$$

A greater score indicates better reappraisal capacity. As noted above, performance-based capacity was used for this study so as to be consistent with historical norms. Study 2 employed multiple measures of capacity.

Reappraisal Tendency. Reappraisal tendency (belief-based) was assessed via self-report on the Emotion Regulation Questionnaire (ERQ; Gross & John, 2003). The ERQ taps two emotion regulation strategies, cognitive reappraisal and expressive suppression. We administered

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the entire measure to avoid impinging upon its validity but focused analyses on the 6-item reappraisal subscale of the ERQ, which measures habitual use of reappraisal for regulating emotions in everyday life. The reappraisal subscale of the ERQ is an appropriate measure of tendency for two major reasons. First, although some of the items indirectly allude to reappraisal success, it is arguably more geared towards assessing frequency of reappraisal use since the wording is more focused on what individuals do instead of how well they do it. Second, an overwhelming number of prior studies have used it to measure tendency (McRae et al., 2012; Troy et al., 2010). Respondents are asked to rate the extent to which they agree with a series of statements on a seven-point Likert scale (1 = “strongly disagree”, 7 = “strongly agree”). Sample items include “I control my emotions by changing the way I think about the situation I’m in” and “When I’m faced with a stressful situation, I make myself think about it in a way that helps me stay calm”. Items of the reappraisal subscale were averaged into a single mean score, with greater scores indicate greater reappraisal tendency. The measure displayed good reliability ($\alpha = .83$). As noted above, belief-based tendency was used for this study so as to be consistent with historical norms. Study 2 utilized multiple assessments of tendency.

Perceived Stress. Perceived stress was measured using the Perceived Stress Scale (S. Cohen, Kamarck, & Mermelstein, 1983). The scale measures the degree to which individuals appraise general life events as being subjectively stressful. Participants are presented with a series of 14 items related to thoughts and feelings people have when stressed. Participants are then asked to indicate, along a five-point Likert scale, how often they have experienced each of the items in the past month (1 = “never”, 3 = “sometimes”, 5 = “very often”). Sample items include “How often have you found that you could not cope with all the things you had to do?” and “How often have you been upset because of something that happened unexpectedly?”. Items

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were reverse coded when appropriate and averaged to yield a single score. Greater scores indicate greater perceived stress. As noted above, a self-reported questionnaire measure of perceived stress was used for this study while Study 2 utilized multiple measures of perceived stress. The measure was found to have good reliability in our sample ($\alpha = .85$).

Cognitive Flexibility. We measured cognitive flexibility by administering a computerized probabilistic reversal learning measure and then implementing a computational reinforcement learning model to obtain a parameter estimate that would serve as our metric of cognitive flexibility.

First, participants were asked to complete a canonical, two-choice probabilistic reversal learning task (e.g., den Ouden et al., 2013; Hanson et al., 2017). Participants were told they would see a blue and yellow slot machine on every trial and that the slot machines would differ in their likelihood of paying out. Participants were instructed to pick the machine that paid out the most often. Importantly, participants were notified that the likelihood of paying out for a given machine could change throughout the task so they had to keep track of the machine that was paying out the most often and select it. The payout likelihood of a given machine switched between .7 and .3 approximately every 35 trials (though participants were not made aware of this). Participants completed 138 trials in total. The ‘pay-outs’ from the slot machines in this task were hypothetical; participants did not receive actual money for completing the task.

In order to obtain a metric of cognitive flexibility from these data, we implemented a simple computational reinforcement learning model. Specifically, a Rescorla-Wagner model (Rescorla & Wagner, 1972) was used to model how participants update the value of stimuli. The critical parameter of this model is typically referred to as the learning rate (α) and is traditionally seen as a constant value that weighs the extent to which a prediction error is incorporated when

updating the subjective value of a given stimulus. Here we interpret alpha as a measure of cognitive flexibility, in line with past research (Hauser, Iannaccone, Walitza, Brandeis, & Brem, 2015). In order for one to learn during the task, a participant must be aware that value contingencies are changing and revise their beliefs accordingly – similarly to how one must revise their interpretation of an affective stimulus during reappraisal. Using a probabilistic reversal learning task in conjunction with this computational model has several advantages. First, other approaches, like a global/local task (Hedden & Gabrieli, 2010), tend to collapse performance over the course an entire task or questionnaire, essentially ignoring valuable heterogeneity across task performance. Other measures of flexibility rely on subjective scoring of participant responses (e.g., unusual use tasks). By contrast, our approach is data -driven, readily incorporates within-task heterogeneity, and results in an index that is theoretically more sensitive to individual differences than conventional measures (Hauser et al., 2015). Lastly, and most crucially, using the RW equation allowed us to characterize a psychological construct using a computational approach, reducing ambiguity in the form of a mathematical definition, providing a clearer operationalization of what we intend to study, and being more generative and specific with respect to underlying mechanisms.

Working Memory. Because cognitive reappraisal is thought to be comprised of several different executive functions (Ochsner et al., 2012), it was necessary to control for working memory to test whether cognitive flexibility uniquely accounted for the association between reappraisal and perceived stress. To this end, we administered a standard 2-back working memory task. Participants were shown a series of letters, each one at a time, and were asked to indicate whether the current letter is the same or different as the one presented two trials before it. Each letter was presented for 2000 ms, separated by a 750 ms fixation cross. There were 100

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trials total, 30 of which were target trials (i.e., “same”) and 70 were non-target trials (i.e., “different”). To create a summary index from the data, we calculated d' scores based on normalized rates of hits (correctly remembering a letter matched the one shown two trials earlier) and false alarms (incorrectly indicating a letter matched one shown two trials earlier; Stanislaw & Todorov, 1999).

Analysis Plan.

We tested our hypotheses by running two sets of pre-registered analyses—each set included a different facet of cognitive reappraisal as a predictor (i.e., capacity or tendency). First, we conducted two simple linear regressions in which perceived stress was the dependent variable regressed on capacity or tendency. Second, we ran two bias-corrected bootstrapped path analyses³, as specified by Preacher and Hayes (2008), to determine whether cognitive flexibility (indexed by α from the RW model) accounted for covariance between capacity and perceived stress, and tendency and perceived stress (model number 4 in the SPSS PROCESS macro). We specifically chose to use bias-corrected bootstrapped analyses because of their distinct power advantages—they are equipped with better power to detect a range of effect sizes than other alternatives and thus require only relatively modest sample sizes (with respect to other methods) (Fritz & Mackinnon, 2007). Third, we re-ran the path analyses, but this time residualized our cognitive flexibility variable on d' scores of working memory. In addition to reporting traditional p-values, we also report confidence intervals for all results. All analyses were conducted on a Windows 10 Machine with IBM SPSS Statistics (version 24).

We note here that the path analyses we employ are frequently referred to as mediation. We have intentionally refrained from using that particular term in order to avoid confusion about

³ We ran the same analyses for working memory, our control executive function, at the suggestion of a previous reviewer. Said analyses yielded null results.

temporal assumptions of causality. Our aim is instead to test whether cognitive flexibility statistically accounts for the covariance in a set of given association *without* making temporally causal assumptions (Thoemmes, 2015).

Results

Overview

The results are organized as follows. We first summarize results of diagnostic analyses (fully described in the supplement) testing the basic Gauss-Markov assumptions of ordinary least squares regression and then describe basic descriptive statistics of the sample. We then report results of confirmatory (i.e., pre-registered) analyses and conclude with a description of exploratory (i.e., post-hoc, follow-up) results.

Diagnostics and Basic Descriptive Statistics.

Regression Diagnostics. We ran a series of diagnostics to verify whether our data violated the Gauss-Markov assumptions for linear regression. Procedures and results are detailed in depth in the supplementary material. Briefly, we generally found no major violations of the Gauss-Markov assumptions. Although there were slight violations of homoscedasticity, we note that linear regressions are generally robust to such violations (Cohen, Cohen, West, & Aiken, 2002). In consideration of the slight biases observed, we report 99%, bootstrapped confidence intervals (10,000 iterations).

Bivariate Correlations and Descriptive Statistics. Descriptive statistics and bivariate correlations between all study variables are displayed in Table 1. Notably, reappraisal capacity and reappraisal tendency were not correlated ($r(123) = .01$, 99% CI $[-.22, .23]$, $p > .250$). Tendency and perceived stress evinced a medium-sized, significant correlation ($r(123) = -.30$,

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99% CI [-.49, -.06], $p = .001$). Cognitive flexibility exhibited a modest, albeit non-significant, correlation with reappraisal tendency ($r(123) = -.16$, 99% CI [-.38, .06], $p = .070$) and displayed a similar relationship with reappraisal capacity ($r(123) = .13$, 99% CI [-.10, .35], $p = .159$).

Working memory was significantly correlated with capacity ($r(121) = .20$, 99% CI [-.03, .41], $p = .029$) but not tendency ($r(121) = .08$, 99% CI [-.15, .30]). Lastly, we ran a one-sample t-test on scores of capacity to check if participants were engaging in reappraisal as directed. The mean capacity value of 29.11 was highly significant relative to a null value of zero ($t(124) = 20.41$, $p < .001$), providing evidence that our task tapped cognitive reappraisal capacity in the current sample. Mean scores of tendency and perceived stress were 4.86 (SD = 1.10) and 3.05 (SD = .56), respectively (Table 1).

Confirmatory Results

Relationship Between Reappraisal Capacity, Cognitive Flexibility & Perceived Stress.

First, we conducted a simple linear regression in which perceived stress was regressed on scores of reappraisal capacity. The results of this analysis failed to reject the null hypothesis ($B = .002$, $SE = .003$, 99% CI = [-.005, .009], $p > .250$, $R^2 = .002$). Next, we ran path analyses in which capacity was entered as the predictor, perceived stress as the outcome, and cognitive flexibility as accounting for the relationship between the two. The point estimate of the indirect effect of X on Y (i.e., $c - c'$) was found to be -.0005 (bootstrapped SE = .0006, 99% CI [-.0035, .0006], 10,000 iterations). Because the confidence interval included 0, we cannot conclude a statistically significant indirect effect. Lastly, we re-ran the analysis with cognitive flexibility scores residualized on working memory, allowing us to control for the latter. The point estimate of the indirect effect was still not significant (Indirect Effect = -.0003, bootstrapped SE = .0005, 99% CI = [-.0029, .0006]).

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Relationship Between Reappraisal Tendency, Cognitive Flexibility and Perceived Stress.

Similar to the analyses described above, we began by regressing perceived stress on scores of reappraisal tendency. Consistent with our hypothesis, greater reappraisal tendency was related to lower self-reported perceived stress ($B = -.151$, $SE = .043$, 99% CI = $[-.261, -.035]$, $p < .001$, $R^2 = .089$). Subsequent path analyses revealed that cognitive flexibility did not statistically account for the association between tendency and perceived stress (Indirect Effect = .0113, bootstrapped SE = .0107, 99% CI = $[-.0058, .0581]$, 10,000 iterations), even when controlling for working memory (Indirect Effect = .0117, bootstrapped SE = .0114, 99% CI = $[-.0075, .0610]$).

Exploratory Results

After pre-registration, data collection, initial stages of data analysis, and correspondence with colleagues, we were motivated to conduct a set of post-hoc, exploratory analyses.

Does Tendency Predict Perceived Stress Above and Beyond Capacity? First, even though tendency and capacity were not significantly correlated, we still thought it necessary to test whether tendency was still associated with perceived stress, over and above the effect of capacity. A multiple regression analysis confirmed that this was the case (capacity: $B = .002$, $SE = .003$, 99% CI = $[-.005, .009]$, $p > .250$; tendency: $B = -.151$, $SE = .043$, 99% CI = $[-.262, -.036]$, $p < .001$, adjusted $R^2 = .077$).

Considering Alternative Models. Next, we considered that we may have specified a theoretically incorrect model—perhaps cognitive flexibility *moderates* the relationship between reappraisal and perceived stress. We ran two follow-up moderation analyses to determine whether cognitive flexibility—residualized on working memory—moderated links between reappraisal tendency and capacity. Both analyses yielded null results (Model 1, Tendency x Flexibility interaction term $B = -.051$, $SE = .047$, $p > .250$; Model 2, Capacity x Flexibility

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interaction term $B = .034$, $SE = .052$, $p > .250$). Based on prior literature (Ford et al., 2017), we also tested whether capacity and tendency have interactive effects on perceived stress with a moderation analysis in which perceived stress was predicted from capacity, tendency, and a capacity x tendency interaction term. The interaction term was not significant ($B = -.037$, $SE = .050$, $p > .250$).

Comparing Model Fits between Capacity and Tendency. The final follow-up analysis we conducted was to determine whether the model predicting perceived stress from tendency was significantly better fitting than the one predicting perceived stress from capacity. Descriptively, the former has a higher R^2 value than the latter (.089 vs .002). However, it was unclear whether this value is statistically significant. Since the models are not nested, we could not conduct a formal model comparison by traditional means. In light of this, we opted to use a bootstrapping approach, implemented in R (version 3.3.2). We sampled from our own data 50,000 times, with replacement, fitting a perceived stress-tendency model and a perceived stress-capacity model each iteration and computing the difference in their R^2 values. We used the 50,000 scores of differences in R^2 values as an approximation of the sampling distribution. Constructing a 95% confidence around the distribution revealed that it did not include zero (95% CI = [.005, .158]), indicating that the perceived stress-tendency model is significantly better fitting than the perceived stress-capacity model.

Interim Discussion

The first key finding from Study 1 indicates that the tendency to engage in reappraisal more frequently is associated with significantly lower perceived stress, while there was no such evidence for a relationship between cognitive reappraisal capacity and perceived stress. Events can often be perceived as stressful when individuals feel they lack control over them (Averill,

1973; Thoits, 2010; Thompson, 1981). For this reason, greater reappraisal tendency may be related to less perceived stress because it imbues individuals with a sense of control over their circumstances. We also found that cognitive flexibility does not account for the association between either facet of reappraisal (capacity or tendency) and perceived stress. This null finding could mean that cognitive flexibility is simply not important for reappraisal, or that it does not have implications for wellbeing. However, given that our study was the first reappraisal study to use reinforcement learning models to derive an index of cognitive flexibility; it is also plausible that this measure of cognitive flexibility fails to appropriately assess cognitive flexibility as it is used in the context of cognitive reappraisal. Another possibility is that reappraisal is an emergent system—a system that is greater than the sum of its parts and no one single mechanistic feature is any more privileged than the others. If such is the case, future work would benefit from examining cognitive flexibility in tandem with other executive skills. One final explanation for Study 1's findings is that we tested the association between beliefs about reappraisal tendency and beliefs about perceived stress, compared to performance related to reappraisal capacity and beliefs about perceived stress. This implies the possibility that the constructs studied here only show relations within belief- or performance-based categories for substantive or methodological (e.g., method variance) reasons. For this reason, Study 2 sought to replicate and extend the findings of Study 1 using multiple measures of reappraisal capacity and tendency as well as perceived stress.

Study 2: Replication and Understanding Differences between Beliefs and Performance

We aimed to accomplish two goals in Study 2. First, we set out to replicate Study 1 results (confirmatory and exploratory) to bolster confidence in the robustness of our initial findings. To recapitulate, we broadly found that tendency was both a significant and better

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predictor of perceived stress than capacity while also showing that cognitive flexibility evinced an indirect effect on either relationship. Second, we sought to address Study 1's aforementioned limitations by (i) assessing performance- and belief-based metrics of capacity, tendency, and perceived stress while (ii) collecting an additional, more traditional measure of cognitive flexibility. By doing so, we hoped to clarify whether reappraisal tendency is truly a better predictor of perceived stress than reappraisal capacity, or whether self-report measures about reappraisal, regardless of whether its related to tendency or capacity, are more predictive of perceived stress than performance-based measures. Additionally, we sought to test whether the null (or negligibly small) indirect effect of cognitive flexibility on perceived stress observed in Study 1 was idiosyncratic to the specific measure of cognitive flexibility used. Our hypotheses for Study 2 were the same as Study 1, tested under novel conditions (e.g., with additional index of cognitive flexibility, etc.), and all hypotheses were again pre-registered (osf.io/4rm6e). Exploratory analyses similar to Study 1 were also included.

Methods

Participants

We recruited participants from the at-large community at the University of California, Los Angeles by posting flyers, tapping mailing lists, and encouraging word of mouth referrals. As part of Study 2's pre-registration, we set an *a priori* sample size of 125⁴ subjects to be consistent with Study 1. Our final sample included 88 females (70.4%) and had a mean age of 21.17 years ($SD = 3.1$, Range = 18-37). In terms of race, 36.89% identified as Asian, 34.43% identified as Caucasian, 7.4% identified as African American, 0.8% identified as American

⁴ 3 participants dropped out of the study following the daily survey period, and a handful of participants were excluded from analyses for non-compliance. Sample sizes thus varied between 119 and 125.

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Indian/Alaskan Native, 0% identified as Native Hawaiian or Other Pacific Islander, 8.2% were mixed race, 5.7% identified as ‘Other’, and 6.5% declined to respond. In terms of ethnicity, 23.8% of the sample identified as Latinx. As in Study 1, English proficiency was a de facto requirement as all study communication was performed in English. Written consent was obtained for all participants in accordance with the policies of the UCLA Institutional Review Board. Data, materials, and code for Study 2 are publicly available on the OSF (osf.io/nmyp4).

Procedure

Study 2 occurred in three phases. First, participants scheduled an initial laboratory visit to consent to participate in the study and receive instruction on how to complete ‘daily surveys’ that were used as a performance-based measure of perceived stress (see subsequent text). Second, participants completed a daily survey period wherein they were emailed surveys in the evening for seven consecutive days. Third, 1-3 days following completion of the survey period, participants returned to the lab to complete the rest of the study measures. This final lab session was similar to that in Study 1. Participants completed computerized measures (assessing performance-based reappraisal capacity, performance-based reappraisal tendency, two separate measures of cognitive flexibility, and working memory) and self-report measures (self-reported belief-based reappraisal capacity, self-reported belief-based reappraisal tendency, self-reported belief-based perceived stress, and other exploratory measures). A subset of the exploratory self-report measures administered in Study 1 were also collected in Study 2 (as before, the pre-registration discloses all measures that were collected). The order of computerized measures was counterbalanced across subjects. Training procedures, non-intrusive monitoring, and task/session durations for this final lab session were similar to Study 1.

Measures

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Performance-Based Reappraisal Capacity. Performance-based reappraisal capacity was assessed using the same instructions, paradigm, and estimation procedure as described in Study 1.

Belief-Based Reappraisal Capacity. Belief-based reappraisal capacity was operationalized as scores from a modified version of the ERQ (sometimes referred to as a ‘Reappraisal Capability’ or ‘Cognitive Reappraisal Ability’ self-report measure; we refer to it as the ‘ERQ Capacity’ measure) (Troy et al., 2017). The eight item measure was adapted from the original ERQ. Specifically, the original ERQ items, which ask about reappraisal frequency, were adapted to ask one’s *ability* to reappraise (sample items: “When I really want to, I am very capable of changing the way I am thinking about a situation that is likely to make me feel strong emotions” and “When I really want to, I am very capable of controlling my emotions by changing the way I think about the situation I’m in”). As with the original ERQ, all responses were made a seven-point scale (1 = “strongly disagree”, 7 = “strongly agree”), and items were averaged together to yield a single value of belief-based capacity ($\alpha = .90$).

Performance-Based Reappraisal Tendency. We used a computerized Emotion Regulation Choice task (ERC; Sheppes, Scheibe, Suri, & Gross, 2011) to derive a performance-based metric of reappraisal tendency. Each trial on the task is comprised of four phases: stimulus preview, decision, decision follow-through, and self-reported affect rating. Participants are first presented with a brief (750ms) preview of the stimulus without any accompanying instruction. Afterwards, the image disappeared and they were shown a decision screen with a centered fixation cross and ‘CHOOSE’ shown at the top of the screen. Participants had 15,000ms to decide, via button press, whether to reappraise via reinterpretation (‘decrease’; e.g., imagining the antecedents of the image were misleading as depicted in the image) or passively observe the image (‘look’). If no

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choice was made, ‘look’ was chosen by default. Next, the image was re-displayed for 5,000ms, accompanied by text (‘LOOK’ or ‘DECREASE’) at the top of screen to ensure participants were reminded of their choice. Finally, participants rated their affect on a 5-point Likert scale identical to the one from the task assessing performance-based capacity (3000ms). A 1500ms ITI (with fixation cross) was sandwiched between trials. Design features of this task (e.g., timing) are broadly consistent with prior work (Hay et al., 2015; Martins, Sheppes, Gross, & Mather, 2018; Scheibe et al., 2015).

All stimuli for this task were negatively valenced and drawn from both OASIS and the International Affective Picture System (Lang, Bradley, & Cuthbert, 2008) stimulus set; stimuli are available on the OSF. The task consisted of 40 trials. One participant declined to complete the ERC task, three participants dropped out after the daily survey period, and two did not properly comply with instructions, meaning the final sample size for ERC task data was $N = 119$. As a manipulation check, we used random coefficient regression (which accounts for repeated measures nested within individuals) to determine whether choices to reappraise were associated with decrease negative affect ($Affect_{it} = \gamma_{00} + \gamma_{10}(Decision_{it}) + u_{0j} + u_{1j} + e_{ij}$). Indeed, the fixed effect of choosing to reappraise indicated a significant, expected decrease in negative affect ($\gamma_{10} = -0.721$, $SE = .099$, $p < .001$). We obtained subject-specific estimates of performance-based tendency by using empirical Bayes estimates from a random coefficient regression model. Larger values indicated a greater propensity to reappraise. Our pre-registered use of this technique theoretically allows for better estimation of performance-based tendency as it not only uses information from each individual, but also incorporates information about the rest of the sample into the estimate. Specific details on this estimation procedure can be accessed in the supplement.

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Belief-Based Reappraisal Tendency. Belief-based reappraisal tendency was measured the same way as it was in Study 1, via the original ERQ ($\alpha = .84$).

Performance-Based Perceived Stress. A metric of performance-based stress was computed using the aforementioned daily surveys. During the daily survey period, participants were required to answer a 1-item surveys tapping their perceived stress during the day. The survey required participants to use a 5-point Likert scale (1 = “Not at all stressed”, 2 = “Slightly stressed”, 3 = “Moderately stressed”, 4 = “Very stressed”, 5 = “Extremely stressed”) to answer a single question about that day’s subjective stress (“Overall, how stressed do you feel today?”) The daily survey period lasted for seven consecutive evenings. Participants were emailed their nightly survey at 7PM (sent via Qualtrics), received a text message reminder at 8PM (sent via ohdontforget.com), and were instructed to complete the item anytime between 7PM and when they went to bed. Every participant except one completed at least 5 nightly surveys (only four surveys were recorded for one participant due to a technical error).

We obtained subject-specific estimates of performance-based perceived stress by using empirical Bayes estimates from a random coefficient regression model. Larger values indicated greater performance-based perceived stress. As noted in the preceding section, this technique theoretically allows for better estimation of performance-based tendency as it not only uses information from each individual, but also incorporates information about the rest of the sample into the estimate. Specific details on this estimation procedure can be accessed in the supplement. We note that this composite of perceived stress is not necessarily ‘performance-based’ in the same way that the performance-based reappraisal measures are, but it is nevertheless more similar to a performance-based assessment insofar that it is not as susceptible

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to response or memory biases in the same way that completing a one-shot, summary questionnaire like the PSS is.

Belief-Based Perceived Stress. Belief-based perceived stress was assessed in the same manner as in Study 1 (using the PSS). The only notable difference is that individuals completed two versions of the PSS: the original, and a version that asked about stressful events in the past week only. This was to ensure the timespan covered by our performance- and belief-based measures of stress were approximately the same. Replication analyses for this study (i.e., Study 2 analyses aiming to replicate Study 1 results) use the original PSS, and confirmatory analyses for this study use the modified measure (modified (week) $\alpha = .85$, original (month) $\alpha = .88$).

Cognitive Flexibility. We collected two measures of cognitive flexibility in this study. The first measure was identical to Study 1 (i.e., cognitive flexibility estimated using a standard Rescorla-Wagner model). The second measure was a canonical global/local task, constituting a more traditional measure of cognitive flexibility, similar to what has been previously collected in prior studies on cognitive control and reappraisal (McRae et al., 2012). During the task (Hedden & Gabrieli, 2010), participants were shown a series of large letters (S or H) comprised of smaller letters (S or H) and were asked to attend to a particular level (global or local) and identify the letter via button press. The color of the letters was used to indicate which level to focus on (purple for local, white for global). Participants completed the task in blocks. Possible block types were limited to congruent (i.e., global and local matched) non-shifting (i.e., the same level was indicated for every trial) global-focused, congruent non-shifting local-focused, congruent shifting, incongruent non-shifting global-focused, incongruent non-shifting local-focused, and incongruent shifting. Each block consisted of 12 trials and was presented twice in random order (144 total trials). Reaction times were averaged for all shift vs non-shift trials. A switch cost

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metric was computed by subtracting average non-shift RTs from average shift RTs. This latter, traditional metric of cognitive flexibility was taken to further scrutinize whether the null results with cognitive flexibility in Study 1 were due to our novel operationalization of the construct.

Working Memory. Working memory was assessed using the same procedures as in Study 1.

Analysis Plan.

The same general analysis plan used in Study 1 was employed here. First, we began by replicating all our results from Study 1. This involved quantifying the direct effects of capacity and tendency on perceived stress, determining whether they interacted, verifying if tendency alone was a better model for perceived stress than capacity alone, and testing for indirect effects of cognitive flexibility. Next, we examined how associations between capacity, tendency, and wellbeing changed when varying the assessment modality (incorporating belief- and performance-based metrics, respectively). We then searched for indirect effects (due to the large number of possible models, we only ran mediation/indirect effect analyses if we found a significant direct effect between reappraisal and perceived stress). These analyses were pre-registered on the OSF (osf.io/sk3y2). Post-hoc, exploratory analyses focusing on unpacking differential associations between capacity and tendency on wellbeing are described at the end.

Results

Overview. Study 2 results are structured in the following way. First, we replicate findings from Study 1. This involves replicating all of Study 1's confirmatory (quantifying effects of capacity and tendency on perceived stress and testing for indirect effects of cognitive flexibility) and exploratory analyses (effect of tendency over and above capacity, better fitting

tendency model over capacity model, interactive effects of capacity and tendency on stress, and interactive effects of cognitive flexibility and capacity/tendency on stress). Afterwards, we conducted confirmatory analyses to test our Study 2 hypotheses, which involved examining how associations between capacity, tendency, and wellbeing changed when varying the assessment modality (incorporating belief- and performance-based metrics, respectively) and searching for indirect effects⁵. These analyses were pre-registered on the OSF (osf.io/sk3y2). Post-hoc, exploratory analyses focusing on unpacking differential associations between capacity and tendency on wellbeing are described at the end.

Diagnostics and Basic Descriptive Statistics.

Regression Diagnostics. We ran the same set of regression diagnostics here as we did in Study 1 (described in the Supplement). To be consistent with Study 1, we report 99% bootstrapped confidence intervals (10,000 iterations).

Bivariate Correlations and Descriptive Statistics. Descriptive statistics and bivariate correlations between all study variables are displayed in Table 2.

In Study 2, reappraisal capacity and tendency were indeed correlated, but only within measurement modality. In other words, performance-based capacity and tendency were positively correlated ($r = .20, p = .033, 99\% \text{ CI } [-.01, .41]$), belief-based capacity and tendency were positively correlated ($r = 0.67, p < .001, 99\% \text{ CI } [.45, .82]$), but belief-based capacity and performance-based tendency ($r = -.14, p = .133, 99\% \text{ CI } [-.37, .11]$), as well as performance-based capacity and belief-based tendency ($r = .06, p > .250, 99\% \text{ CI } [-.19, .28]$), were uncorrelated. Belief-based and performance-based capacity, in addition to belief-based and

⁵ due to the large number of possible models, we only ran mediation/indirect effect analyses if we found a significant direct effect between reappraisal and perceived stress

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performance-based tendency were respectively uncorrelated with one another (capacity: $r = .08$, $p > .250$, 99% CI [-.18, .33]; tendency: $r = -.11$, $p > .250$, 99% CI [-.35, .16]).

The computationally derived index of cognitive flexibility (α) was inversely correlated with working memory ($r = -.24$, $p = .010$, 99% CI [-.46, .01]) and was unassociated with all other measures. The traditional, set-shifting index of cognitive flexibility was positively correlated with performance-based capacity ($r = .20$, $p = .031$, 99% CI [-.05, .42]) and negatively correlated with belief-based tendency ($r = -.20$, $p = .034$, 99% CI [-.43, .03]).

Performance-based perceived stress was inversely correlated with belief-based capacity ($r = -.19$, $p = .045$, 99% CI [-.44, .08]) and positively correlated with belief-based perceived stress ($r = .41$, $p < .001$, 99% CI [.20, .61]). Belief-based perceived stress was negatively correlated with belief-based capacity ($r = -.49$, $p < .001$, 99% CI [-.67, -.27]) and tendency ($r = -.48$, $p < .001$, 99% CI [-.70, -.20]).

Replicating Study 1 Analyses

Replicating Study 1's Pre-registered analyses

Measuring the Effects of Capacity and Tendency on Stress. First, we conducted a simple linear regression wherein we regressed belief-based perceived stress scores on performance-based reappraisal capacity and replicated the null results obtained in Study 1 ($B = .002$, $SE = .003$, 99% CI [-.006, .010], $p > .250$, $R^2 = .002$). These results did not depend on whether belief-based perceived stress was reported from the prior week or prior month. We conducted another simple linear regression by regressing belief-based perceived stress on belief-based tendency and replicated the significant effects observed in Study 1 ($B = -.262$, $SE = .051$, 99% CI [-.379, -

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.118], $p < .001$, $R^2 = .233$). These results did not depend on whether belief-based perceived stress was reported from the prior week or prior month.

Estimating Indirect Effects of Cognitive Flexibility. We replicated our null results from Study 1, finding that there was no significant indirect effect of cognitive flexibility on performance-based capacity and belief-based stress (Indirect effect = .0000, bootstrapped SE = .0003, 99% CI [-.0007, .0007]), even after accounting for working memory (Indirect effect = .000, bootstrapped SE = .0003, 99% CI [-.0007, .0006]). The same held true for the indirect effect of cognitive flexibility on belief-based tendency and belief-based stress (Indirect effect = .0086, bootstrapped SE = .0092, 99% CI [-.0041, .0315]), even when accounting for working memory (Indirect effect = .0092, bootstrapped SE = .0107, 99% CI [-.0048, .0364]).

Replicating Study 1's Exploratory Analyses

Do Reappraisal Capacity and Tendency Predict Stress Over and Above Each Other? We attempted to determine whether the effect of belief-based reappraisal tendency on belief-based perceived stress from the prior week remained significant after adjusting for performance-based capacity. Multiple regression analysis confirmed this was indeed the case (performance-based capacity: $B = .000$, SE = .003, 99% CI [-.007, .009], $p > .250$; belief-based tendency: $B = -.262$, SE = .051, 99% CI [-.380, -.115], $p < .001$, adjusted $R^2 = .220$). The association between belief-based tendency dropped out of significance if using perceived stress scores assessed over the prior month, as opposed to the prior week ($B = -.072$, SE = .041, 99% CI [-.179, .043], $p = .082$, adjusted $R^2 = .008$).

Do Capacity and Tendency Interact to Predict Wellbeing? In Study 1 we ran an exploratory analysis to test whether, as has been suggested in the literature (Ford et al., 2017),

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capacity and tendency may interact to predict wellbeing but did not find evidence to support this hypothesis. In Study 2, we sought to confirm this result and ran another moderation analysis in which performance-based capacity, belief-based tendency (both standardized), and their interaction were entered as predictors of stress. We observed a conditional effect of performance-based capacity, ($B = .030$, $SE = .057$, 99% CI $[-.108, .185]$, $p > .592$; conditional effect of belief-based tendency: $B = -.299$, $SE = .054$, 99% CI $[-.425, -.142]$, $p < .001$; interaction term: $B = -.133$, $SE = .061$, 99% CI $[-.297, .031]$, $p = .025$, adjusted $R^2 = .252$) such that a one unit increase in capacity, while holding tendency constant, is expected to change the association between tendency and perceived stress by $-.133$ (or vice versa). In other words, the effect of reappraisal capacity on perceived stress becomes stronger given greater levels of tendency and the effect of reappraisal tendency on perceived stress becomes stronger given greater levels of capacity.

Comparing Model Fits for Reappraisal Capacity and Tendency Predicting Stress. We next sought to replicate Study 1's exploratory findings that tested whether the model predicting belief-based stress from belief-based tendency evinced a better fit than the model predicting belief-based stress from performance-based capacity. We used the same bootstrapping approach as described in the Study 1 results. The difference in R^2 values was $.233$ (belief-based stress ~ belief-based tendency - belief-based stress ~ performance-based capacity) with a 95% CI of $[.066, .415]$. Given the confidence interval around our bootstrapped sampling distribution did not include the null value of zero, we can conclude that the model with tendency as a predictor significantly fits better compared to capacity. This result held when examining perceived stress scores measuring stress from the prior month (difference in $R^2 = .222$; 95% $[.057, .404]$).

Testing Whether Cognitive Flexibility Moderates the Association between Reappraisal Skills and Stress. In Study 1, we considered that we may have theoretically mis-specified our

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model and reasoned that cognitive flexibility may moderate the association between facets of reappraisal and stress. Here we attempt to replicate the null results of that analysis. As before, we ran two moderation models, one in which flexibility moderated the link between capacity and stress, and another in which flexibility moderated the link between tendency and stress. In both models, we failed to reject the null hypothesis in these analyses, as we did in Study 1, concluding again that cognitive flexibility does not act as a moderator (first model, capacity X flexibility: $B = .007$, $SE = .061$, 99% CI $[-.167, .155]$, $p > .250$, adjusted $R^2 = -.026$; second model, tendency X flexibility: $B = -.023$, $SE = .056$, 99% CI $[-.139, .151]$, $p > .250$, adjusted $R^2 = .203$).

Study 2 Analyses: Incorporating Performance- and Belief-Based Measurement Modalities

Confirmatory Results.

Probing Relationships between Stress and Capacity Across Different Assessment Modalities. We sought to determine whether the associations observed above would hold across different types of assessments (i.e., performance v tendency). First, we tested whether performance-based stress was predicted by performance-based capacity; it was not (Table 3, row (i)). By contrast, we found that belief-based capacity was inversely related to belief-based stress (Table 3, row (ii)). Performance-based tendency was not related to performance-based stress (Table 3, row (iii)), whereas belief-based tendency was indeed inversely related to belief-based stress (as reported above).

Testing Indirect Effects of Cognitive Flexibility on Performance- and Belief-Based Associations. Analyses of indirect effects showed that neither index of cognitive flexibility accounted for the significant links reappraisal and perceived stress described above (even when controlling for working memory).

Exploratory Results.

Probing Cross-Modal Relationships between Stress and Tendency. As reported above, we found a null association between belief-based perceived stress and performance-based reappraisal capacity. To probe whether this null association was due to these measures being obtained through different modalities (self-reported beliefs versus performance), we next tested whether performance-based *tendency* also failed to predict belief-based perceived stress. This was indeed the case – performance-based tendency was not predictive of belief-based stress (Table 3, row (v)).

Do Capacity and Tendency Differentially Predict Perceived Stress? We next tested whether the independent effects of belief-based capacity and tendency on belief-based stress held when controlling for one another. We first addressed this above as a replication of a Study 1 analysis, and here we expand to incorporate different measurements of capacity and tendency (e.g., performance, belief-based). Multiple regression results showed that capacity still evinced significant, inverse associations with belief-based stress, over and above tendency, whereas the effect of tendency, over and above capacity, was no longer significant (Table 3, row (vi)). For thoroughness, we performed the same analysis with performance-based measures of stress, capacity, and tendency. Results from this analysis remained null (Table 3, row (vii)).

Teasing Capacity and Tendency Apart from Performance and Beliefs. Our final set of exploratory analyses attempted to tease apart associations between stress and capacity/tendency (regardless of measurement modality), and stress and measurement modality (regardless of whether measuring capacity or tendency). For purposes of clarity and brevity, we have included the detailed methods and results of these analyses in the supplement. The main point to note here

is that we found weak but consistent evidence that belief-based composites of reappraisal skills were better predictors of wellbeing than performance-based ones.

General Discussion

Cognitive reappraisal is a putatively adaptive form of emotion regulation that has been linked to several wellbeing outcomes. The present study sought to test whether reappraisal capacity and tendency are differentially associated with wellbeing, whether cognitive flexibility accounts for links between reappraisal and wellbeing, and whether associations between reappraisal and perceived stress generalize across belief- and performance-based measures of reappraisal capacity and tendency. Study 1 is one of few to test both reappraisal tendency and capacity independently in the same sample and Study 2 is the first—to our knowledge—to analyze both belief- and performance-based scores of reappraisal to attempt to identify the ‘critical component’ of reappraisal that accounts for its efficacy in improving wellbeing outcomes. Across both studies, we found no evidence that cognitive flexibility accounts for the association between either facet of reappraisal (capacity or tendency) and perceived stress, regardless of measurement modality. Our results generally indicated that individuals who endorsed greater belief-based scores of reappraisal capacity and tendency tended to report significantly lower perceived stress, while we found no evidence for a similar relationship between perceived stress and performance-based measures of reappraisal. These findings have several theoretical and practical implications.

Theoretical Implications. Our first key finding was that we did not find evidence that cognitive flexibility mediates or moderates the association between either facet of reappraisal (capacity or tendency) and perceived stress. These null findings could be interpreted multiple ways. First, it could indicate that other untested executive functions (e.g., self-monitoring) are

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indeed important for linking reappraisal to psychological adjustment while cognitive flexibility is not. However, another possibility is that reappraisal is an emergent system—a system that is greater than the sum of its parts and no single mechanistic feature is more privileged than others. Although we found an association between working memory and reappraisal capacity, it is still possible that individual executive functions may not be lynchpins for reappraisal's success in engendering adjustment outcomes given that working memory did not account for the association between reappraisal and perceived stress.

Although prior work has linked executive functions such as cognitive flexibility (McRae et al., 2012) and working memory (Scully et al., 2017) to both reappraisal capacity and tendency, we found that cognitive flexibility was not significantly related with either facet of reappraisal, while working memory was related with capacity. It is plausible that cognitive reappraisal is indeed linked to cognitive flexibility, but only as it is assessed by certain paradigms. For instance, one prior study that found a link between cognitive flexibility and reappraisal capacity used a set-shifting task to measure cognitive flexibility (McRae et al., 2012), whereas we initially used a probabilistic reversal learning task. If the relationship between reappraisal and cognitive flexibility varies as a function of the context in which flexibility is measured (e.g., perceptual, value-based, etc.), then it would suggest that different types of cognitive flexibility have different associations with reappraisal (Malooly et al., 2013). Consistent with this possibility is our finding that performance-based reappraisal capacity was positively associated with a traditional measure of cognitive flexibility but not the measure assessed in the context of reversal learning. Another possible methodological reason for differences across studies, however, is that significance may be driven by variability in sample size and composition—we sampled over one hundred individuals from an ethnically diverse population of college undergraduates, whereas other

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studies report smaller, community samples (e.g., McRae et al., 2012) or larger, less ethnically diverse samples (e.g., Scult et al., 2017) with different age and sex compositions.

Our second key finding was that we observed that individuals who reported possessing strong reappraisal skills (i.e., higher capacity, greater tendency) also reported relatively low levels of perceived stress. Intriguingly, when examined simultaneously, belief-based capacity was significantly predictive of perceived stress while belief-based tendency was weakly related to perceived stress. One interpretation of these results suggests that simply perceiving oneself to have better reappraisal skills may engender reduced perceived stress. One reason for this may lie in perceptions of control (see Study 1 Interim Discussion). Prior work suggests that individuals who believe that emotions are controllable utilize emotion regulation strategies more effectively (Ford & Gross, 2018). This is particularly relevant in the context of stress, given that beliefs about stressor controllability predict the effectiveness of other forms of emotion regulation such as fear extinction (Hartley, Gorun, Reddan, Ramirez, & Phelps, 2014). Though we did not explicitly assess participants' beliefs about the nature of emotional processes, it is plausible that individuals who perceive better control of their emotions likely are more likely to believe that emotions are inherently controllable. Of course, it could alternatively be that individuals who are less stress-prone believe that they are better at regulating their emotions. That performance-based measures largely turned up non-significant associations with stress is also notable, because it suggests a disconnect between individuals' meta-perceptions of affective experience and their objective reappraisal performance. Alternatively, it is possible that the manner in which computerized measures assess reappraisal is simply not related to how reappraisal is implemented outside the laboratory. Future studies should work to unpack this effect in addition to searching for potential 'hidden moderators.'

Practical Considerations. The findings reported here could have practical implications for programs aimed at improving wellbeing, such as psychotherapy approaches. For instance, cognitive-behavioral therapy (CBT), one of the most widely employed and successful forms of psychotherapy (Covin, Ouimet, Seeds, & Dozois, 2008; Durlak, Fuhrman, & Lampman, 1991; Stewart & Chambless, 2009), is predicated upon helping clients learn to reappraise emotionally distressing events and situations. While more work is needed to unpack this effect, therapies like CBT could benefit from the knowledge that individuals who possess greater self-efficacy in reappraisal skills tend to show better adjustment (at least in terms of perceived stress).

Moreover, our findings make an important contribution to the recent discussion about how bolstering executive function may enhance emotion regulation (Cohen & Mor, 2017; Scult et al., 2017). Generally, because executive function may have a causal role in emotion regulation success (Cohen & Mor, 2017), there has been optimism that training it may help bolster emotion regulation and, consequently, wellbeing outcomes. However, our results highlight need for caution in thinking about this issue moving forward, posing important caveats. Executive function is a multi-faceted concept. As we have noted here, emotion regulation strategies are supported by *several* executive function processes. Because a large number of executive processes were not exhaustively assessed in the current study, we cannot conclusively say whether links between reappraisal and wellbeing are supported by one (or a few) privileged executive functions. That said, we *can* conclude there is no evidence to suggest neither cognitive flexibility nor working memory ability fully explains the relationship between reappraisal and perceived stress. This is relevant for the matter of selecting an appropriate facet of executive function to train in order to improve emotion regulation. In addition, since beliefs about reappraisal was related to adjustment in our sample, it may be inefficient to focus on methods

that help improve one's objective performance on reappraisal skills and instead focus on ways to bolster self-efficacy in extant reappraisal skills.

Another practical consideration is whether to continue to use laboratory reappraisal tasks if they continue to fail to show associations with meaningful markers of wellbeing. It is likely that these laboratory tasks do measure reappraisal—they have face validity—but perhaps in a different way than how reappraisal is executed in more naturalistic settings. These tasks typically prompt participants with a cue, require they reappraise the meaning of a static emotional image, and then immediately rate their affect. By contrast, in everyday life individuals must respond to dynamic situations that they have differing degrees of agency over, regulate emotions via their own internal imperatives, and may not always have the opportunity to pause to meta-analyze their own beliefs. Creating paradigms that are low-dimensional distillations of their real-world targets is tractable for controlled laboratory research, but often requires sacrificing elements of ecological validity (Jolly & Chang, 2017). Future work ought to continue to rigorously weigh these competing methodological considerations when assessing the role of emotion regulation in wellbeing. This might include designing assessment techniques that make use of naturalistic stimuli or ecological measurements that capture reappraisal as it unfolds (e.g., analyzing a participant's recollection of a stressful memory, asking a participant to recall their thought process shortly after being present with an emotionally evocative stimulus, etc.).

Limitations. There are several limitations to the current study that merit consideration for future work. First, one salient limitation is in the generalizability of the results. While our sample was large ($N \approx 250$) and racially and ethnically diverse, our results nevertheless come from a sample of college undergraduates and may not generalize to other populations. Similarly, we focused exclusively on perceived stress as an outcome, yet it is entirely possible that the

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associations described in this report differ across different types of wellbeing (Ford et al., 2017). Perceived stress may have different relationships with performance-based markers of capacity and tendency than measures of mood, anxiety, or social function. We also assumed that our global and momentary self-report assessments served as adequate markers of belief- and performance-based constructs. While this practice is consistent with the general literature and helps simplify aspects of our experimental design, we do note that these techniques may not be perfect belief- and performance-based assessments. This is of particular concern as it regards performance-based assessments since such a term implies objectivity when it may not necessarily be warranted. With that mind, we note that we simply have no way of further verifying this notion, so we discuss it here so that readers are aware of this limitation. One final consideration rests on recent reports questioning the reliability of computerized, task-based measures of self-regulation (Enkavi et al., 2019). We found that several measures had adequate, but not stellar, reliability (e.g., performance-based tendency) and one (computationally-derived cognitive flexibility) had quite poor reliability. Relatedly, the fact that cross-method assessments of the same construct did not correlate may be indicative of acceptable, but less-than-optimal psychometric properties (though the psychometrics of our measures were not bad, overall). Another interpretation of divergence in cross-method assessments of the same construct is that they signal an interesting substantive result—that one’s beliefs about their reappraisal abilities and use are not good predictors of their actual abilities. Future methodological work should be conducted to even better flesh out the psychometric validity of the measures used here to help settle this question. This could involve revising existing computerized paradigms to be compatible with sophisticated psychometric evaluation tools such as item response theory, developing additional computerized tasks and determining whether they all map on to a latent

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common factor, and analyzing specific stimuli in standardized stimulus sets (e.g., IAPS, OASIS) to determine which are better suited for computerized tasks and which are not.

Conclusion. In conclusion, our study represents a first attempt to assess associations between reappraisal capacity, tendency and a measure of wellbeing (perceived stress), while simultaneously investigating whether cognitive flexibility helps support such associations. Our results highlight the importance of measuring both reappraisal capacity and tendency, and the importance of the manner in which capacity and tendency are measured, as they may have different relationships with wellbeing. These findings given credence to the possibility that the complex and dynamic cognitive processes that support cognitive reappraisal are emergent, such that one single component of reappraisal is unlikely to solely account for its utility.

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CAPACITY, TENDENCY, FLEXIBILITY & STRESS

Table 1. Descriptive Statistics and Bootstrapped Bivariate Correlations between All Study Variables.

Variable	M (SD)	1	2	3	4	5
1. Capacity	29.11 (15.95)	1				
2. Tendency	4.86 (1.10)	.01	1			
3. Perceived Stress	3.05 (.56)	.05	-.30*	1		
4. Cognitive Flexibility	.57 (.32)	.13	-.16	-.09	1	
5. Working Memory	3.42 (1.32)	.20*	.08	-.10	.13	1

Note. * refers to $p < .05$. Capacity refers to reappraisal capacity and represents percent change in negative affect when reappraising and passively viewing negative stimuli; Tendency refers to mean scores from the reappraisal subscale of the ERQ; Cognitive flexibility refers to the computationally estimated alpha parameter; Working memory refers to d' scores calculated from our n -back task (2-back design). Assessment of capacity was performance-based; assessments of tendency and perceived stress were belief-based.

CAPACITY, TENDENCY, FLEXIBILITY & STRESS

Table 2. Bivariate correlations between study 2 variables.

Variable	M (SD)	1	2	3	4	5	6	7	8	9
1. Capacity - Performance	29.89 (18.32)	1								
2. Capacity - Belief	4.82 (1.21)	.08	1							
3. Tendency - Performance	-.2365 (.60)	.20*	-.14	1						
4. Tendency - Belief	4.59 (1.10)	.06	.67***	-.11	1					
5. Perceived Stress - Performance	2.61 (.50)	.02	-.19*	.06	-.18	1				
6. Perceived Stress - Belief	2.87 (.59)	.01	-.49***	.18	-.48***	.41***	1			
7. Cognitive Flexibility - Alpha	.57 (.31)	.01	-.10	-.04	-.15	.13	.07	1		
8. Cognitive Flexibility - Set Shift Cost	.28 (.11)	.20*	-.16	.06	-.20*	.06	.02	.14	1	
9. Working Memory	3.64 (1.22)	.12	-.17	-.01	-.06	-.11	-.00	-.24*	-.11	1

Note. * refers to $p < .05$, ** refers to $p < .01$, *** refers to $p < .001$. Capacity refers to reappraisal capacity; Tendency refers to reappraisal tendency; Performance based refers to estimates taken from a laboratory task (reappraisal) or daily survey (perceived stress). Cognitive flexibility - alpha to the computationally estimated alpha parameter; Cognitive flexibility set shift cost refers to the difference in shift minus non-shift mean reaction times on the global local task; Working memory refers to d' scores calculated from our n -back task (2-back design).

CAPACITY, TENDENCY, FLEXIBILITY & STRESS

Table 3. Regression results for select Study 2 models

Analysis	Slopes	SE	99% CI	Fit (R^2)
(i) PB Stress ~ PB Capacity	$B = 0.001$	.002	[-.004, .005]	.001
(ii) BB Stress ~ BB Capacity	$B = -0.246$	.042	[-.330, -.163]	.248
(iii) PB Stress ~ PB Tendency	$B = 0.024$	.089	[-.149, .206]	.001
(iv) BB Stress ~ BB Tendency	$B = -0.262$	.051	[-.379, -.118]	.233
(v) BB Stress ~ PB Tendency	$B = 0.091$	.112	[-.124, .311]	.010
(vi) BB Stress ~ BB Capacity + BB Tendency	$B_{cap} = -0.156$ $B_{ten} = -0.147$	.066 .079	[-.284, -.023] [-.303, .013]	.276
(vii) PB Stress ~ PB Capacity + PB Tendency	$B_{cap} = 0.001$ $B_{ten} = 0.020$	.002 .092	[-.006, .007] [-.213, .273]	-.016

Note. B refers to unstandardized coefficients; Fit column reflects adjusted R^2 values when multiple predictors are present in the model; BB refers to belief-based, PB refers to performance-based, Capacity/cap refers to cognitive reappraisal capacity, Tendency/ten refers to cognitive reappraisal tendency, Stress refers to perceived stress, ~ used as 'predicted by' operator.

Supplementary Materials

This supplement contains additional information on our psychometric evaluation of study measures, methodologies for estimating performance-based metrics of perceived stress and reappraisal tendency in Study 2, our process of inspecting regression diagnostics (Studies 1 & 2), and results from an exploratory analysis in Study 2.

Psychometric Evaluation of Select Study Measures

Rationale & Overview.

In conducting Studies 1 and 2, it became apparent to us that our pattern of observed results may be due to (i) interesting effects in some underlying data generation mechanism(s) (i.e., ground truth, or some approximation thereof) *or* (ii) methodological artifacts⁶. In order to help minimize concerns that our results were influenced by the latter, we decided to evaluate some of the psychometric properties of select study measures. Several study measures used herein have never undergone rigorous psychometric evaluation and validation. In fact, many psychological instruments, especially computerized tasks, never undergo psychometric evaluation (Eisenberg et al., 2019). Our subsequent goal was not to exhaustively validate each previously un-validated measure used across our two studies⁷—entire reports could be devoted to this topic (Eisenberg et al., 2019; Enkavi et al., 2019), and some of the measures, namely the computer tasks, are difficult to validate with existing statistical frameworks. Therefore, here we set out to conduct basic, preliminary psychometric evaluation to help rule out the possibility that results were driven by significant deficiencies in reliability.

This aim entailed evaluating the following measures: The emotion regulation choice task used to measure performance-based reappraisal tendency in Study 2; the computerized reappraisal task used to assess performance-based reappraisal capacity in Studies 1 and 2; the traditional cognitive flexibility index (set-shift cost in reaction time) in Study 2; our computationally derived cognitive flexibility index (α) in Studies 1 and 2; the capacity version of the ERQ (ERQ Capacity, which had never been previously psychometrically evaluated) which was used to assess belief-based reappraisal capacity in Study 2. The other measures in our study were already well-validated (e.g., the traditional ERQ), or were not amenable to validation given the scope of the current investigation and available data (e.g., performance-based perceived stress from experience sampled data, working memory (indexed via d')).

We again acknowledge there are several ways to psychometrically evaluate measures, but reiterate our goal was to evaluate any glaring issues having to do with assessment. Thus, we simply tested reliability (with the exception of the ERQ Capacity measure, which we were able to more thoroughly validate). Generally, since more sophisticated tools are available for self-report measures, we used model-based reliability and validity assessments for the ERQ Capacity measure (described below) while incorporating a variant of split-half reliability for the computerized tasks. Split-half reliability was selected for computerized measures because it is easily implemented and makes fewer assumptions. The details and results for each task are described below (roughly in order of increasing complexity).

Results

⁶ Of course, our results could also reflect random chance/noise or some other systematic yet uninteresting confound.

⁷ Some measures have already been thoroughly validated, such as the traditional ERQ and PSS.

Performance-Based Reappraisal Tendency (Study 2). To validate our performance-based metric of reappraisal capacity, we calculated split-half reliability based on even and odd trials. This involved splitting each subject's trial-by-trial data into two halves, one based on even trials and another based on odd trials. We then ran aggregated subjects' data from these two halves to form an even trial dataset and an odd trial dataset and computed our metrics of tendency for each person in each dataset (see details on the computation of this metric in the 'Additional Methodological Details – Study 2' section of this document). That our stimuli were presented randomly across trials to each subject (different random order per subject) means we can be confident there are no aggregate confounds due to even-odd trial distinctions. To be completely thorough, we also ran the same manipulation check described in the main text on the even and odd halves to make sure that reappraisal was associated with reduced negative affect on both even and odd trials. Indeed, reappraisal was associated with reduced negative affect in both the even and odd splits (even: $\gamma_{10} = -0.733$, $SE = .103$, $p < .001$; odd: $\gamma_{10} = -0.730$, $SE = .105$, $p < .001$). Afterwards, we simply took the Pearson correlation between the metrics from the two-halves as the split-half reliability. The Pearson correlation between the metrics in the even and odd datasets proved to be relatively high, suggesting this measure has reasonable split-half reliability ($r = 0.627$).

Performance-Based Reappraisal Capacity (Studies 1 and 2). To validate our performance-based metric of reappraisal capacity, we calculated split-half reliability based on even and odd trials. After splitting data into two halves, we computed our metric of capacity (a percent change score) for each half. We then took the Pearson correlation between the metrics in both halves, constituting split-half reliability. The Pearson correlation between the metrics in the even and odd datasets proved to be fair, not particularly good but not particularly bad ($r = 0.461$).

Traditional Cognitive Flexibility (Set Shift Cost; Study 2). To validate our traditional metric of cognitive flexibility, we again calculated split-half reliability based on even and odd trials. As before, we split each subject's trial-by-trial data into two halves, one based on even trials and the other based on odd trials. We then computed our set shift cost reaction time metric (specified in the main text) on each subject's even- and odd-trial datasets. Afterwards, we took the Pearson correlation between the metrics from the two-halves as split-half reliability. The Pearson correlation between the metrics in the even and odd datasets proved to be relatively high, suggesting this measure has reasonable split-half reliability ($r = 0.599$).

Computationally Derived Cognitive Flexibility (α ; Studies 1 and 2). To validate our computational metric of cognitive flexibility, we employed a similar strategy to the split-half reliability approach. In this case, we split the probabilistic reversal learning task into four blocks, based on the task parameters (alternating blocks where the two machines 'took turns' being the high payout machine). This approach was adopted because the Rescorla-Wagner model used to derive the metric of cognitive flexibility is temporally sensitive—the value of a stimulus on the i th trial depends in part on the value at the prior (trial $i-1$) trial. Because of this, splitting the data into even and odd halves would render the model nonsensical. Instead, we opted to split the data based on blocks, model each block separately, derive α from each one, and then correlate (Pearson) the α 's to get an approximate sense of reliability. We first did this assuming that initial anticipated reward values (necessary for the reinforcement learning model) for each slot machine were equal (as we do for analyses reported in the main document). The reliability values among the blocks were relatively low ($r_{1,2} = 0.383$, $r_{1,3} = 0.337$, $r_{1,4} = 0.257$, $r_{2,3} = 0.422$, $r_{2,4} = 0.312$, $r_{3,4} = 0.345$, $r_{average} = 0.343$).

We re-ran the analysis using different initial anticipated reward values for each slot machine such that the high payout-likelihood machine from the prior block had a value of 1 and the other machine (low payout-likelihood) had a value of zero (the first block remained with equal initial reward values). This particular scheme was chosen to compromise between the fact that having equal anticipated reward values at the start of each block is unrealistic but the fact that using the anticipated reward values from the other block might bias the computation and artificially inflate reliability estimates. In practice, doing so appeared to have a negligible impact on the reliability values, which remained relatively low ($r_{1,2} = 0.337$, $r_{1,3} = 0.324$, $r_{1,4} = 0.273$, $r_{2,3} = 0.404$, $r_{2,4} = 0.283$, $r_{3,4} = 0.348$, $r_{average} = 0.328$).

Belief-Based Reappraisal Capacity (Study 2). Validating the belief-based metric of reappraisal capacity was more involved than any of the other measures. This measure is a self-report instrument, which meant that we had a bevy of sophisticated statistical tools at our disposal for the purposes of validation. Validating this measure was comprised of four broad steps: (i) acquiring additional data, (ii) factor analyzing (exploratory and confirmatory) the ERQ capacity measure, (iii) analyzing item-level data using item response theory (IRT: both exploratory and confirmatory), (iv) combining data with traditional ERQ reappraisal items to ensure belief-based capacity indeed reflected a factor that was distinct from the factor reflected by the traditional ERQ reappraisal items (which we argue index reappraisal tendency). Each step is described in detail below.

(i) Additional Data Acquisition. When validating a self-report measuring using factor analysis, it is customary to split one's sample and use one half for exploratory purposes (e.g., identify prospective factor structures to generate hypotheses) and the other half for confirmatory procedures (attempt to confirm hypotheses). In either case, sample sizes required for each analysis can vary widely, ranging between $n = 150$ -500 under reasonably favorable conditions (Floyd & Widaman, 1995; Morizot, Ainsworth, & Reise, 2007). We were not able to meet this requirement given that our aggregate sample size across the two studies here was 250. To that end, we acquired additional ERQ Capacity⁸ data from parallel studies that our group was running. In these studies, the measure was either added for exploratory aims, or to address research questions orthogonal to the ones in the current paper. In all, we aggregated data from two additional projects: one on social decision-making (Guassi Moreira, Tashjian, Galván, & Silvers, 2019; osf.io/u3tr9) and another on social support and reappraisal (Sahi, Ninova, & Silvers, in prep). Both studies were recruited from the sample population as the current report (with the exception that the latter study we pooled from recruited only females). This left us with a novel, vastly increased sample size of $N = 995$ (79.40% female) for this analysis (including the original 250 participants from the current report).

(ii) Factor Analysis. With additional data in hand, we proceeded to conduct an exploratory and then confirmatory factor analysis after randomly splitting the data into exploratory ($N = 497$) and confirmatory subsets ($N = 498$).

We began by running a series of five exploratory factor analysis models. The first model was a 1 factor model, the next two were correlated factors models with 2 and 3 respective factors, and the final two models were bifactor models with 2 and 3 respective group factors (in addition to the general factor). In the single factor model, loadings ranged from $\lambda = 0.66 - 0.87$

⁸As well as additional traditional ERQ data, which be used later on

and explained 59% of the items' variance ($\chi^2(20) = 61.60$; TLI = 0.891; RMSEA = 0.143, 90% CI [0.126 - 0.160]). In the two correlated factors model, items 5-8 loaded most strongly on the first factor ($\lambda = 0.69 - 0.96$) and items 1-4 loaded most strongly on the second factor ($\lambda = 0.49 - 0.97$). Together, the two factors explained 64% of the items' variance ($p_1 = 34\%$, $p_2 = 30\%$,) and were highly correlated ($\Phi = 0.83$) ($\chi^2(13) = 13.19$; TLI = 0.965; RMSEA = 0.080, 90% CI [0.059 - 0.103]). In the three correlated factors model, items 1-4 loaded most strongly on the first factor ($\lambda = 0.55 - 0.96$), items 5, 6, and 8 loaded most strongly on the second factor ($\lambda = 0.37 - 0.95$), and item 7 loaded most strongly on the third factor ($\lambda = 0.81$). Together, the three factors explained 69% of the items' variance ($p_1 = 34\%$, $p_2 = 20\%$, $p_3 = 15\%$) and were highly intercorrelated ($\Phi_{1,2} = 0.65$, $\Phi_{1,3} = 0.77$, $\Phi_{2,3} = 0.63$) ($\chi^2(7) = 3.34$; TLI = 0.984; RMSEA = 0.055, 90% CI [0.022 - 0.088]). In the bifactor model, loadings on the general factor ranged from $\lambda = 0.62 - 0.83$, items 5-8 loaded most strongly on the first group factor ($\lambda = 0.29 - 0.40$), and items 1-4 loaded most strongly on the second group factor ($\lambda = 0.20 - 0.41$). For this model, $\omega = 0.93$ and ω -hierarchical = 0.84, respectively indicating that 93% of the items' variance is explained by the general and group factors and 84% of the items' variance is explained by the general factor ($\chi^2(13) = 54.25$; RMSEA = 0.080, 90% CI [0.059 - 0.103]). In the 3 group factors bifactor model, loadings on the general factor ranged from $\lambda = 0.63 - 0.82$, items 1-4 loaded most strongly on the first group factor ($\lambda = 0.25 - 0.43$), items 5 and 8 loaded most strongly on the second group factor ($\lambda = 0.48$ and 0.24), and items 7 and 8 loaded most strongly on the third group factor ($\lambda = 0.55$ and 0.24). For this model, $\omega = 0.94$ and ω -hierarchical = 0.83, respectively indicating that 94% of the items' variance is explained by the general and group factors and 83% of the items' variance is explained by the general factor ($\chi^2(7) = 17.37$; RMSEA = 0.055, 90% CI [0.022 - 0.088]). Exploratory factor analyses were implemented using the `fa()` and `omega()` functions in R's 'psych' package. Solutions were obtained by minimizing the residual matrix and using an oblique rotation (oblimin). In the bifactor case, a Schmid-Leiman transformation was applied after rotation to ensure the bifactor form wherein each item loads on the general factor and (ideally) on only one group factor (Mansolf & Reise, 2016).

Above we began with five candidate models hypothesizing the factor structure of the ERQ capacity. We winnowed down the list of prospective models by discarding the 3 correlated factors and 3 group factor bifactor solutions due to their obvious overfactoring and negligible improvements in model fit. We tested the three remaining models using confirmatory factor analysis, implemented in R's 'lavaan' package using the `cfa()` function. Maximum likelihood estimation was used and all variables (latent and observed) were standardized. Full statistics for these models are reported in Supplementary Table 3. Despite the ERQ Capacity measure being seemingly unidimensional in concept, the one factor model failed to provide convincing model fit on the basis of CFI, TLI, and RMSEA. The 2 correlated factors model performed better, but does not make theoretical sense given the pattern of loadings (i.e., there is no reason to theoretically assume why the first four items of the scale would be explained by one factor and the latter four items be explained by a second factor). The 2 group factors bifactor model evinced excellent fit and the pattern of loadings are more consistent with theory: The general factor dominates the common variance (evinced by high item factor loadings and by a high ω -hierarchical) and the group factor loadings are generally relatively low. Given that ω -hierarchical is quite high, we can think of this model as being essentially unidimensional (Toland, Sulis, Giambona, Porcu, & Campbell, 2017). Given the lack of apparent theoretical motivation for the two group factors, it is likely these group factors are weak artifacts (possibly introduced by survey format, item ordering, item syntax/grammar, etc.) that contribute some systematic

variance across items but do not indicate theoretically or substantively meaningful latent constructs.

(iii) *Item Response Theory*. Following identification of the scale's factor structure, we conducted IRT analyses on item level data to assess the validity of each item. IRT is an item-level analysis whose fundamental goal is to estimate individuals' position on a latent trait (Θ) and relate latent trait scores to the likelihood of endorsing an item using exponential binomial and multinomial models (Morizot et al., 2007; Reise & Henson, 2003; Toland et al., 2017). IRT analyses yield *location* (or *difficulty*) (denoted with b)—the latent trait level at which the probability of endorsing an item is .50—and *discrimination* (denoted with a) parameters—the strength of association between Θ and responses on a given item. Because the ERQ capacity measure is a polytomous 7-point scale, we have six location parameters (b_{1-6}), each indicating the position on Θ corresponding to a .5 chance of rating an item at a particular scale value or something higher. From these parameters, response (association between Θ and an item), information (an item's ability to differentiate individuals at different trait levels conditional on Θ), and standard error of measurement (measurement error conditional on Θ) curves can be constructed for each item and aggregated into scale-level totals.

IRT analyses were conducted using the `mirt()` function from R's 'mirt' package. Exploratory and confirmatory graded response models were fit on the respective halves of our data, using maximum likelihood via an expectation-maximization algorithm (BFGS optimizer). Importantly, latent trait values were assumed to follow a normal distribution ($\Theta \sim N(0, 1)$). From Supplementary Table 5, we can see that b values ranged from ~ -2.5 to ~ 1.6 across items, indicating that ERQ Capacity items covered most of the range typically observed on latent variables (-2.5 to 2.5). a parameters ranged between 1.7 and 3.9, indicating very high discrimination relative to other similar measures in this area of research (Morizot et al., 2007). Results were comparable between exploratory and confirmatory sets. Supplementary Figures 1 and 2 show the response and information curves at the item level (top row), visually indicating that most items are discriminating and provide much information across a range of theta. Because the curves are additive, scale level aggregates were computed and plotted (bottom row). These show that, at the level of the entire scale, the measure is reasonably discriminating and provides information across a wide range Θ (between approximately -3 and 2). By contrast, these plots also show that the scale appears to be ill-suited for measuring individuals who score exceptionally high on the latent trait (more than 2 standard deviations above).

(iv) *Differentiating Between Tendency*. Thus far, we have demonstrated evidence to suggest that the ERQ Capacity is a psychometrically valid and reliable instrument. Although it presumably measures capacity, one major doubt left is whether this construct is distinct from tendency. That is, we only tested factor models with ERQ Capacity items; it was still possible that those items were not measuring anything different from the traditional ERQ reappraisal items, which we argue measures tendency. To evaluate this possibility, we conducted two additional confirmatory factor analyses. The first analysis evaluating 2 correlated factors, with each factor uniquely being indicated by one set of items (capacity, tendency). The second analysis was a bifactor model with two group factors, with a general factor being indicated by all items and group factors corresponding to the respective capacity and tendency items. The 2 correlated factors model evinced comparably worse fit, but was still informative insofar that the pattern of item factor loadings indicated items loaded highly on their respective factor, providing evidence the two items sets are distinct in their latent variables. The bifactor model, however,

indicated superior fit. In the general, the items loaded highly on group factors and relatively lower on the common factor, suggesting the influence of the group factors were relatively strong. Moreover, the group factors were only correlated at .34, further suggesting that the capacity and tendency items do indeed measure distinct constructs.

Additional Methodological Details – Study 2

Empirical Bayes Estimation of Performance-Based Reappraisal Tendency, Study 2.

To obtain subject-specific estimates of performance-based tendency, we first fit a random coefficient logistic regression model of the following form.

$$\text{logit}(\text{decision}_{it}) = \gamma_{00} + u_{0j} + e_{ij}$$

The log odds of the i -th participant's decision to reappraise (reappraise = 1, look = 0) on the t -th trial are modeled as a function of an overall propensity (likelihood) to choose reappraisal (γ_{00}), a subject-specific deviation from said overall propensity to choose reappraisal (u_{0j}), and an error (e_{ij}) in predicting the i -th participant's log-odds of reappraising on the t -th trial from the two aforementioned terms. Each subject-specific deviation was be summed with the overall, fixed effect of reappraisal choice propensity to yield our performance-based metric of cognitive reappraisal tendency (in logits). Formally, these estimates are known as Empirical Bayes Estimates (EBE), and do not represent arithmetic averages of participant-level reappraisal propensity, but instead weighted averages that combine information from overall (γ_{00}) and participant-specific (u_{0j}) estimates of reappraisal choice propensity (Candel, 2004). Random coefficient regression and subsequent EBEs were calculated using the Hierarchical Linear Modeling Software (Raudenbush & Byrk, 2002).

Empirical Bayes Estimation of Performance-Based Perceived Stress, Study 2.

To derive a single metric of performance-based stress, we used a random coefficient regression model of the following form.

$$\text{stress}_{it} = \gamma_{00} + u_{0j} + e_{ij}$$

The i -th participant's rating of stress on the t -th day was modeled as a function of an overall average fixed effect stress rating (γ_{00}), a subject-specific deviation (u_{0j}), and an error (e_{ij}). Each subject-specific deviation was be summed with the overall, fixed effect of reappraisal choice propensity to yield our performance-based metric of perceived stress. Just as we did when calculating performance-based reappraisal tendency (see above), here we again used EBEs in order to obtain a performance-based metric of stress. Random coefficient regression and subsequent EBEs were calculated using the Hierarchical Linear Modeling Software (Raudenbush & Byrk, 2002).

Regression Diagnostics – Studies 1 & 2

In order to be as rigorous as possible, we opted to run several diagnostics to ensure our data did not violate the Gauss-Markov assumptions of ordinary least squares regression and potentially bias our regression coefficients and standard errors. Further, we ran additional diagnostics to guarantee our results weren't driven by problematic or influential cases. All values necessary for the following diagnostics were obtained via SPSS (version 24) while all figures were generated with R (version 3.3.2).

Homoscedasticity. First, we checked for violations of homoscedasticity by rendering residual scatter plots and histograms of residuals. Residuals for the regressions of perceived stress on reappraisal capacity and tendency both yielded slightly skewed residuals. Across both studies, the variance of the residuals for pre-registered analyses roughly appeared to increase directly with the predictor variables in both regressions, indicating that our standard errors may be downwardly biased. Although this violation seemed modest, and linear regressions are generally robust to these violations (Cohen, Cohen, Aiken, & West, 2002), we nevertheless opted to widen our confidence intervals to 99% in order to be more conservative.

Normality of errors. Second, we examined whether our residuals were normally distributed by generating Q-Q plots. Such plots contrast observed residuals against what would be expected under a normal distribution. Q-Q plots for pre-registered analyses across both Study 1 and Study 2 are bowed upwards revealing that our residuals are positively skewed. However, t-tests are usually robust to a minor violation such as ours, and the confidence intervals we reported provide converging evidence.

Influential Cases on Estimates of Coefficients and Precision. Third, we investigated whether there were any cases may have unduly influenced estimation of regression coefficients and standard errors. To this end, we calculated values for each case on the following metrics: Cook's Distance, DFBetas, DFFITS, Studentized Deleted Residuals (SDR), and COVRATIO. Cohen and colleagues (2002) previously specified a set of guidelines for each of the above metrics that help determine when a case may be problematic. The formula for each guideline (where applicable) along with examples calculations (those from Study 1) are listed in Supplementary Table 1.

Study 1. A number of cases for both regressions (e.g., Perceived Stress ~ Capacity; Perceived Stress ~ Tendency) were identified to be problematic in terms of Cook's Distance. However, since Cook's Distance measures the influence each case has on shaping *all* regression parameters simultaneously, including the intercept, it was possible that these cases were only influencing the intercept of both analyses. To verify this, we checked our DFBeta values for the regression coefficient from each analysis that related perceived stress to a facet of reappraisal (i.e., capacity or tendency). Sets of DFBeta values are generated for each parameter estimate in a regression model and describe the extent to which a single case influences estimation of a particular parameter in isolation from other parameters. According to the DFBeta guideline listed below, no cases influenced estimations of parameters that related a facet of reappraisal to perceived stress (i.e., capacity & tendency). No cases were found to be problematic for the DFFITS and SDR metrics.

Lastly, various cases for regression analyses exceeded the COVRATIO guidelines (13 for PSS ~ Capacity; 10 for PSS ~ Tendency). COVRATIO measures the extent to which a single case influences estimation of the standard error, either by contributing towards a loss of precision or leading to an unwarranted enhancement of precision. To determine whether these cases had a significant effect across both sets of analyses, we excluded them and re-ran our analyses. There were no appreciable differences between our original analyses and those excluding problematic cases. We chose to report analyses with the full sample in the main text.

Study 2. In Study 2, a much larger number of influential cases across pre-registered regression models were identified. We dealt with this by excluding any case from all models as long as it was an influential case in at least one of the models. This led us to re-run our pre-registered regression models with sample sizes ranging from 90-91. Excluding cases did not meaningfully affect the results. We chose to report analyses with the full sample in the main text.

Additional Exploratory Analyses – Study 2

Exploratory Analysis from Study 2: Teasing Capacity and Tendency Apart from Performance and Beliefs

We conducted another set of exploratory analyses attempted to tease apart associations between stress and capacity/tendency (regardless of measurement modality), and stress and measurement modality (regardless of whether measuring capacity or tendency). To do so, we created a composite score of capacity and tendency by Z-scoring their respective performance- and belief-based measures and taking the average of the two values, yielding a composite measure of capacity and tendency, respectively. The same procedure was repeated to create composites of performance-based aspects of reappraisal and belief-based aspects of reappraisal (collapsed across tendency and capacity).

We ran two models with capacity and tendency composites predicting self-reported, belief-based perceived stress (capacity: $B = -.213$, $SE = .075$, $p = .005$; tendency: $B = -.148$, $SE = .080$, $p = .066$, adjusted $R^2 = .127$), and then performance-based perceived stress (capacity: $B = -.056$, $SE = .069$, $p > .250$; tendency: $B = -.042$, $SE = .074$, $p > .250$, adjusted $R^2 = -.003$). We then did the same with performance- and belief-based composites, predicting belief-based perceived stress (performance-based: $B = .041$, $SE = .058$, $p > .250$; belief-based: $B = -.342$, $SE = .050$, $p < .001$, adjusted $R^2 = .281$) and performance-based stress (performance-based: $B = .023$, $SE = .059$, $p > .250$; belief-based: $B = -.101$, $SE = .050$, $p = .046$, adjusted $R^2 = .019$).

Next, we ran a Principle Components Analysis on our performance- and belief-based metrics of reappraisal capacity and tendency (varimax rotation with Kaiser normalization, average absolute residual correlation = .128). The rationale for this was to obtain composites in a data driven fashion by allowing for clustering of the reappraisal skill four variables (belief- and performance-based tendency and capacity). Two components emerged such that belief-based measures of reappraisal loaded strongly on Component 1 (capacity = .913, tendency = .906) and performance-based measures of reappraisal loaded strongly on Component 2 (capacity = .779, tendency = .773). That the dimensionality of these data was reduced along lines of measurement modality, as opposed to facet of reappraisal, is telling. We then extracted principle component scores for each participant and used them predict performance-based (Component 1: $B = -.091$, $SE = .046$, $p = .050$; Component 2: $B = .019$, $SE = .046$, $p > .250$, adjusted $R^2 = .017$) and belief-based (Component 1: $B = -.313$, $SE = .046$, $p < .001$; Component 2: $B = .033$, $SE = .046$, $p > .250$, adjusted $R^2 = .280$) perceived stress. The convergence of these PCA results with those using our other composite scores of performance- and belief-based facets of reappraisal is notable given that component scores from PCA are not mathematically equivalent to our composites based on arithmetic means of standardized variables.

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Supplementary Table 1. Guidelines for determining influential values along several diagnostic metrics (Cohen et al., 2002).

<i>Metric</i>	<i>Guideline</i>	<i>Example Calculation</i>
Cook's D	Value $> 4/(n-k-1)$	$4/(125-1-1) = .033$
DFBeta	Abs value $> 2/\sqrt{n}$	$2/\sqrt{125} = .179$
DFFITS	Abs value $> 2*\sqrt{(k+1)/(n-k-1)}$	$2 * \sqrt{(1+1)/(125-1-1)} = .255$
SDR	Abs values > 3	—
COVRATIO	$\text{Abs}(\text{COVRATIO} - 1) > 3(k+1)/n$	$3(1+1)/125 = .048$

Note. SDR refers to studentized deleted residual. k refers to the number of non-intercept regression parameters; n refers to sample size.

Supplementary Table 2. The ERQ Capacity measure (courtesy of Drs. Allison Troy & Iris Mauss)

ERQ Capacity Item Label	ERQ Capacity Item Text
ERQ Capacity 1	When I really want to, I am very capable of controlling my emotions by changing the way I think about the situation I'm in.
ERQ Capacity 2	When I really want to, I am very capable of changing what I'm think about when I want to feel less negative emotion (such as sadness or anger).
ERQ Capacity 3	When I really want to, I am very capable of making myself think about a stressful situation in a way that helps me stay calm.
ERQ Capacity 4	When I really want to, I am very capable of changing the way I'm thinking about a situation when I want to feel less negative emotion.
ERQ Capacity 5	When I really want to, I am very capable of changing what I'm thinking about when I want to feel more positive emotion (such as joy or amusement).
ERQ Capacity 6	When I really want to, I am very capable of changing the way I am thinking about a situation that is likely to make me feel strong emotions.
ERQ Capacity 7	When I really want to, I am very capable of reconsidering what relevance the situation really has for me if a situation is likely to make me upset.
ERQ Capacity 8	When I really want to, I am very capable of changing the way I'm thinking about a situation when I want to feel more positive emotion.
Traditional ERQ Item Label	Traditional ERQ Item Text
ERQ 1 (Reapp)	When I want to feel a more positive emotion (such as joy), I change what I'm thinking about.

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1 Factor, Unidimensional	2 Correlated Factors	Bifactor, 2 Group Factors

ERQ 3 (Reapp)

When I want to feel less negative emotion (such as sadness or anger), I change what I'm thinking about.

ERQ 5 (Reapp)

When I'm faced with a stressful situation, I make myself think about it in a way that helps me stay calm.

ERQ 7 (Reapp)

When I want to feel more positive emotion, I change the way I'm thinking about the situation.

ERQ 8 (Reapp)

I control my emotions by changing the way I think about the situation I'm in.

ERQ 10 (Reapp)

When I want to feel less negative emotion, I change the way I'm thinking about the situation.

Note. The reappraisal items of the traditional ERQ are listed below the ERQ Capacity items. The ERQ Capacity items all refer to reappraisal. The traditional ERQ contains items about reappraisal and suppression tendency. Here we only used the traditional ERQ reappraisal items about *reappraisal* tendency.

Supplementary Table 3. Confirmatory Factor Analyses for the reappraisal capacity measure

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Item	Factor1	Factor 1	Factor2	GFactor	Factor1	Factor2
ERQ Capacity 1	0.82 (.037) [.74 - .89]	0.82 (.038) [.75 - .90]	-	0.75 (.043) [.67 - .83]	0.32 (.053) [.22 - .43]	-
ERQ Capacity 2	0.82 (.037) [.74 - .89]	0.85 (.037) [.78 - .92]	-	0.68 (.051) [.58 - .78]	0.60 (.057) [.49 - .71]	-
ERQ Capacity 3	0.70 (.040) [.62 - .78]	0.70 (.040) [.62 - .78]	-	0.69 (.043) [.61 - .77]	0.18 (.055) [.07 - .29]	-
ERQ Capacity 4	0.84 (.037) [.77 - .91]	0.88 (.036) [.81 - .95]	-	0.76 (.045) [.67 - .85]	0.43 (.054) [.32 - .53]	-
ERQ Capacity 5	0.77 (.039) [.69 - .84]	-	0.83 (.038) [.75 - .90]	0.62 (.062) [.50 - .74]	-	1.08 (.405) [.29 - 1.88]
ERQ Capacity 6	0.71 (.040) [.63 - .79]	-	0.72 (.040) [.65 - .80]	0.74 (.043) [.65 - .82]	-	0.12 (.074) [-.02 - .27]
ERQ Capacity 7	0.61 (.042) [.53 - .70]	-	0.62 (.042) [.53 - .70]	0.68 (.044) [.60 - .77]	-	0.00 (.035) [-.07 - .07]
ERQ Capacity 8	0.82 (.038) [.74 - .89]	-	0.89 (.036) [.81 - .96]	0.80 (.046) [.71 - .89]	-	0.25 (.126) [.00 - .50]
Fit Statistic						
χ^2 (df)	238.272 (20)	120.239 (19)			22.660 (11)	
CFI	0.914	0.960			0.995	
TLI	0.880	0.941			0.988	
RMSEA [95% CI]	0.148 [0.132 – 0.165]	0.104 [0.086 - 0.122]			0.046 [0.018 – 0.073]	
ω , ω_H	NA	NA			0.934, 0.822	

Note. $\Phi_{1,2} = 0.86$ in the 2 Correlated Factors model; $\Phi_{1,2} = 0.29$ in the Bifactor model

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2 Factors, Correlated			Bifactor, 2 Group Factors		
Item	Capacity (Reappraisal)	Tendency (Reappraisal)	GFactor	Factor1	Factor2
ERQ Capacity 1	0.81 (.038) [.74 - .89]	-	0.59 (.137) [.32 - .85]	0.57 (.139) [.29 - .84]	-
ERQ Capacity 2	0.82 (.038) [.74 - .89]	-	0.49 (.169) [.16 - .82]	0.71 (.119) [.48 - .94]	-
ERQ Capacity 3	0.70 (.040) [.62 - .78]	-	0.50 (.120) [.26 - .74]	0.49 (.122) [.25 - .73]	-
ERQ Capacity 4	0.84 (.037) [.77 - .91]	-	0.50 (.176) [.16 - .85]	0.74 (.122) [.50 - .98]	-
ERQ Capacity 5	0.77 (.039) [.69 - .84]	-	0.78 (.075) [.63 - .93]	0.27 (.182) [-.08 - .63]	-

Supplementary Table 4. Confirmatory Factor Analyses for the reappraisal capacity measure and the traditional reappraisal tendency measure

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ERQ Capacity 6	0.71 (.040) [.63 - .79]	-	0.61 (.093) [.43 - .79]	0.36 (.146) [.07 - .64]	-
ERQ Capacity 7	0.62 (.042) [.54 - .70]	-	0.51 (.089) [.34 - .69]	0.33 (.126) [.09 - .58]	-
ERQ Capacity 8	0.82 (.037) [.74 - .89]	-	0.89 (.071) [.75 - 1.03]	0.24 (.208) [-.17 - .65]	-
ERQ 1 (Reapp)	-	0.69 (.041) [.61 - .77]	0.42 (.060) [.30 - .54]	-	0.55 (.051) [.45 - .65]
ERQ 3 (Reapp)	-	0.71 (.040) [.63 - .79]	0.28 (.071) [.14 - .42]	-	0.66 (.046) [.57 - .75]
ERQ 5 (Reapp)	-	0.56 (.043) [.48 - .64]	0.31 (.059) [.19 - .42]	-	0.47 (.049) [.37 - .56]
ERQ 7 (Reapp)	-	0.86 (.037) [.79 - .94]	0.40 (.076) [.25 - .55]	-	0.77 (.048) [.67 - .86]
ERQ 8 (Reapp)	-	0.85 (.037) [.78 - .92]	0.38 (.076) [.24 - .53]	-	0.76 (.047) [.67 - .85]
ERQ 10 (Reapp)	-	0.84 (.037) [.77 - .91]	0.34 (.078) [.19 - .50]	-	0.78 (.046) [.69 - .86]
Fit Statistic					
χ^2 (df)	481.714 (29)		299.994 (62)		
CFI	0.909		0.947		
TLI	0.891		0.922		
RMSEA [95% CI]	0.104 [0.095 - 0.113]		0.088 [0.078 - 0.098]		
ω , ω_H	NA		0.918, 0.572		

Note. $\Phi_{1,2} = 0.55$ in the 2 Correlated Factors model; $\Phi_{1,2} = 0.34$ in the Bifactor model

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Supplementary Table 5. Parameter estimates of exploratory and confirmatory IRT analysis

Item	<i>a</i>		<i>b1</i>		<i>b2</i>		<i>b3</i>		<i>b4</i>		<i>b5</i>		<i>b6</i>	
	Exp	Con	Exp	Con	Exp	Con	Exp	Con	Exp	Con	Exp	Con	Exp	Con
ERQ Capacity 1	2.4	3.3	-2.6	-2.2	-1.8	-1.5	-1.1	-1.1	-0.8	-0.8	0.1	-0.0	1.1	0.9
ERQ Capacity 2	3.3	3.3	-2.1	-2.1	-1.2	-1.4	-0.7	-0.8	-0.4	-0.5	0.4	0.2	1.3	1.2
ERQ Capacity 3	2.1	2.2	-2.5	-2.3	-1.7	-1.6	-0.9	-0.9	-0.5	-0.6	0.4	0.2	1.5	1.4
ERQ Capacity 4	3.9	3.9	-2.3	-2.4	-1.4	-1.5	-0.8	-0.9	-0.5	-0.7	0.4	0.2	1.4	1.1
ERQ Capacity 5	2.9	2.8	-2.6	-2.7	-1.7	-1.9	-1.1	-1.2	-0.7	-0.8	0.2	0.1	1.3	1.0
ERQ Capacity 6	2.3	2.3	-2.8	-2.7	-1.6	-1.7	-1.0	-1.0	-0.4	-0.5	0.6	0.4	1.6	1.4
ERQ Capacity 7	1.9	1.7	-3.1	-3.1	-2.1	-2.4	-1.5	-1.6	-1.0	-1.0	-0.0	0.0	1.2	1.1
ERQ Capacity 8	3.8	3.4	-2.4	-2.6	-1.7	-1.8	-1.1	-1.2	-0.7	-0.8	0.2	0.2	1.2	1.1

Supplementary Figure 1. Plots of exploratory IRT analyses on the ERQ Capacity measure.

Note. Top left panel depicts item-level response curves (item scoring function), top right panel depicts item-level information (information trace lines), bottom left panel depicts overall response curve (total score), and bottom right panel depicts overall information compared to standard error (test information and standard errors). Boxes in the top row of figures contain curves for each one of the eight items on the ERQ capacity measure.

Supplementary Figure 2. Plots of confirmatory IRT analyses on the ERQ Capacity measure.

Note. Top left panel depicts item-level response curves (item scoring function), top right panel depicts item-level information (information trace lines), bottom left panel depicts overall response curve (total score), and bottom right panel depicts overall information compared to standard error (test information and standard errors). Boxes in the top row of figures contain curves for each one of the eight items on the ERQ capacity measure.

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