
Supplementary information

Emotion regulation capacity lacks a shared neural signature

In the format provided by the
authors and unedited

Emotion Regulation Capacity Lacks a Shared Neural Signature

-Supplementary Information-

Table of Contents

Supplementary Tables	2
Table S1	2
Supplementary Figures	4
Figure S1. Statistical power for correlations between reappraisal capacity indices ..	4
Figure S2. Flowchart of literature search and screening procedure	5
Figure S3. Effect-size prior for Bayesian meta-analysis	6
Figure S4. Heterogeneity prior for Bayesian meta-analysis	7
Supplementary Methods	8
Testing the custom meta-analytic functions	8
Power Analysis	10
Meta- vs mega-analytic approaches	11
References	15

Supplementary Tables

Table S1

Characteristics of included primary studies

Study First author Publication year	N	age M (SD)	Female (%)	Questionnaire M (SD)	Stimulus Type	Task Ratings
Benzait et al. (2023) ⁷⁶	17	31.4 (9.1)	35%	ERQ: 4.5 (0.9)	pictures	
Berboth et al. (2021) ⁵⁵	25	22.8 (3.0)	84%	ERQ: 2.6 (0.7)	pictures	x
Brehl et al. (2021; partially unpublished) ^a	242	22.9 (5.1)	73%	FEEL-E: 22.7 (4.0)	pictures	x
Burghart et al. (unpublished)	31	23.4 (4.1)	100%	ERQ: 5.0 (0.8)	pictures	x
Diers, Gärtner et al. (2023) ^{*77}	39	24.6 (4.2)	66%	ERQ: 4.7 (0.8)	pictures	
Dörfel et al. (2014; distancing) ⁷⁸	16	23.7 (6.7)	100%	ERQ: 4.6 (1.1)	pictures	
Dörfel et al. (2014; reinterpretation) ⁷⁸	19	22.4 (2.7)	100%	ERQ: 4.4 (1.1)	pictures	
Gaebler et al. (2014) ⁷⁹	23	30.0 (8.0)	78%	ERQ: 4.8 (1.2)	pictures	
Diers et al. (2021) ⁸⁰	37	25.2 (4.4)	53%	ERQ: 4.6 (0.9)	pictures	
Gärtner et al. (2019) ⁸¹						
Gianaros et al. (2020; AHAB-II) ⁸²	163	42.7 (7.1)	48%	ERQ: 5.0 (0.9)	pictures	x
Gianaros et al. (2020; PIP) ⁸²	176	40.1 (6.3)	51%	ERQ: 5.0 (0.9)	pictures	x
Glosemeyer et al. (2020) ⁸³	15	24.0 (3.1)	53%	ERQ: 4.1 (0.9)	cyberball	x
Hofhansel et al. (2023) ⁸⁴	16	34.7 (10.2)	0%	NA	game	
Jentsch et al. (2019) ⁸⁵	30	23.7 (3.4)	47%	ERQ: 4.5 (1.3)	pictures	x
Powers et al. (2022) ⁸⁶	40	25.5 (4.7)	70%	ERQ: 4.8 (0.9)	pictures	x
Wessa et al. (unpublished)	89	30.0 (14.0)	55%	ERQ: 27.0 (6.0)	pictures	x
Kim et al. (unpublished)	33	23.0 (2.1)	52%	ERQ: 4.8 (1.1)	pictures	x
LaBar et al. (unpublished) ^b	53	55.0 (12.2)	62%	ERQ: 4.6 (1.0)	Autobio- graphical memory cues	x
Marín-Morales et al. (2022) ⁸⁷	29	38.3 (8.2)	0%	CERQ: 11.9 (11.9)	pictures	x
Min et al. (2022) ⁸⁸	105	22.8 (2.7)	49%	ERQ: 29.5 (6.0)	pictures	x
Morawetz et al. (2016) ⁸⁹	23	22.9 (3.6)	65%	ERQ: 2.9 (1.1)	videos	x
Morawetz et al. (2016; pictures) ^{c,56}	59	32.4 (11.2)	33%	ERQ: 3.0 (1.0)	pictures	x
Morawetz et al. (2016; videos) ^{c,56}	59	32.4 (11.2)	33%	ERQ: 3.0 (1.0)	videos	x

Morawetz et al. (2019) ⁹⁰	29	24.5 (5.2)	81%	ERQ: 2.9 (1.1)	pictures	x
Morawetz et al. (2020) ⁹¹	35	23.0 (3.4)	82%	ERQ: 3.1 (1.0)	pictures	x
Morawetz et al. (2021) ⁹²	37	22.0 (3.7)	87%	ERQ: 2.8 (1.1)	pictures	x
Mulej Bratec et al. (2015) ⁹³	20	24.8 (2.3)	100%	ERQ: 5.1 (0.5)	pictures	x
Müller-Pinzler et al. (unpublished)	15	24.0 (3.1)	53%	ERQ: 4.0 (0.9)	pictures	x
Huneke et al. (unpublished) ^d	18	25.6 (5.6)	67%	ERQ: 5.4 (0.7)	pictures	x
Pierce et al. (2022) ⁹⁴	110	43.6 (18.1)	57%	ERQ: 5.4 (1.2)	pictures	x
Paschke et al. (2016) ⁹⁵	115	26.1 (3.8)	51%	ERQ: 3.5 (1.1)	pictures	x
Rehbein et al. (2021; sample 1) ⁹⁶	15	23.9 (4.6)	100%	ERQ: 30.8 (3.8)	pictures	x
Rehbein et al. (2021; sample 2) ⁹⁶	16	23.1 (3.2)	100%	ERQ: 29.1 (5.3)	pictures	x
Sandner et al. (2021) ⁹⁷	38	25.0 (4.0)	50%	CERQ: 15.3 (3.6)	pictures	x
Doren et al. (unpublished)	139	39.5 (11.9)	6%	ERQ: 27.0 (5.5)	pictures	x
Guendelman et al. (2022) ⁹⁸	55	38.5 (10.0)	83%	CERQ: 13.9 (3.7)	pictures	x
Sokołowski et al. (2022) ⁹⁹	83	21.7 (1.8)	49%	CERQ: 14.6 (3.4)	pictures	x
Steward et al. (2021) ¹⁰⁰	92	20.0 (2.8)	54%	ERQ: 30.5 (6.4)	pictures	x
Lloyd et al. (2021) ¹⁰¹	59	69.5 (7.7)	50%	CERQ-SF: 6.1 (2.0)	pictures	x
Tupitsa et al. (2023) ¹⁰²	19	27.0 (5.0)	55%	CERQ-SF: 8.2 (1.4)	pictures	x

Note. The task ratings column indicates whether task-based affective ratings were present. Only ratings given during the task were considered (e.g., not after the task). Sample size varied slightly depending on the outcome in some studies. Therefore, the sample sizes for questionnaires are reported in this table for simplicity, as these were the lower bound of sample sizes for the three outcomes. For questionnaires authors provided the subscales most indicative of cognitive reappraisal. These were for the reappraisal scale for the ERQ (Emotion Regulation Questionnaire), the positive reappraisal scale for the CERQ (Cognitive Emotion Regulation Questionnaire), and the re-evaluation scale for the FEEL-E (Fragebogen zur Erhebung der Emotionsregulation bei Erwachsenen). Sample sizes may differ between table entries and referenced studies due to multiple factors (e.g., different inclusion/exclusion criteria, missing data, continued data collection).

*first author used for study identifiers in case the sample was linked to multiple equivalent publications

^afMRI-specific results currently unpublished

^bPublished after submission as Graner et al. (2026)¹⁰³

^cBoth studies tested the same participants. Their effect estimates were therefore averaged for all analyses to avoid double-counting participants and inflating statistical precision.

^dPublished after submission as Huneke et al. (2026)¹⁰⁴.

Supplementary Figures

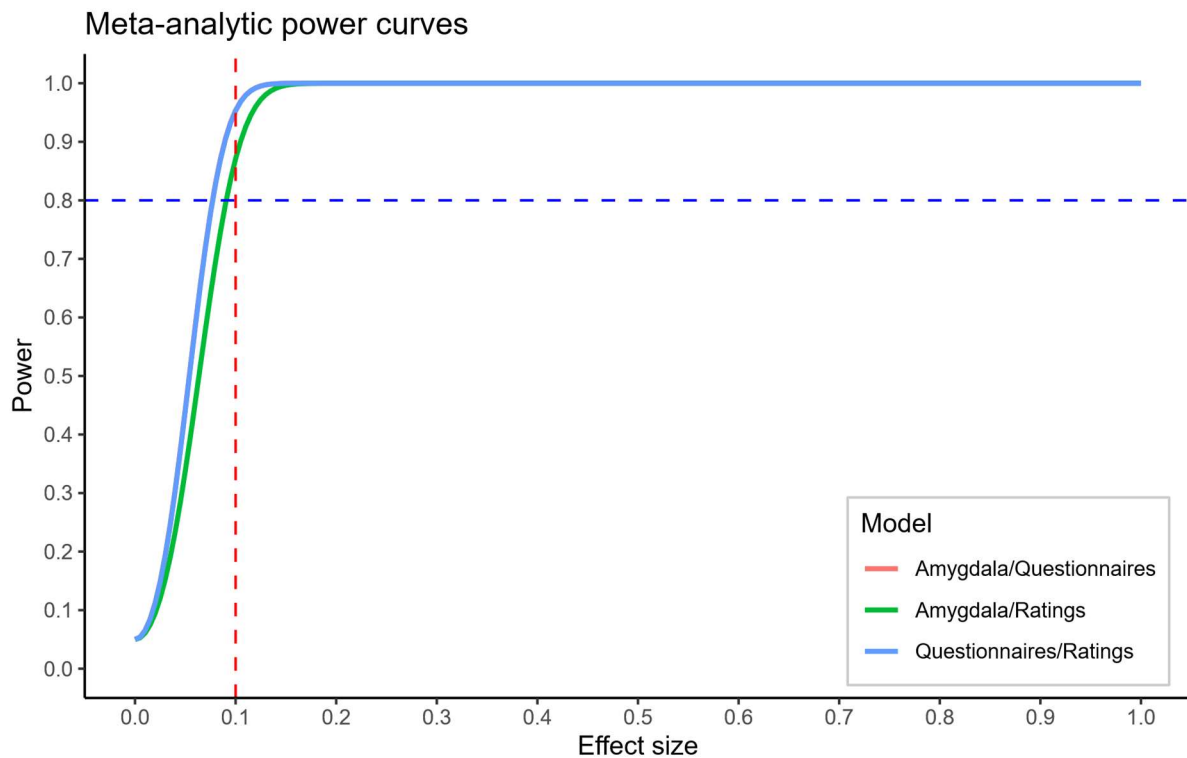


Figure S1. Statistical power for correlations between reappraisal capacity indices

Statistical power for random-effects meta-analyses as a function of hypothetical effect sizes for the correlations between the three outcomes at $\tau = .07$ based on an analytic approach¹⁰⁵. Sample sizes corresponded to the three pairwise meta-analyses: task-based affective ratings and amygdala down-regulation ($k = 26$, $N = 1,377$), trait questionnaires and task-based affective ratings ($k = 28$, $N = 1,397$), and trait questionnaires and amygdala down-regulation ($k = 33$, $N = 1,833$).

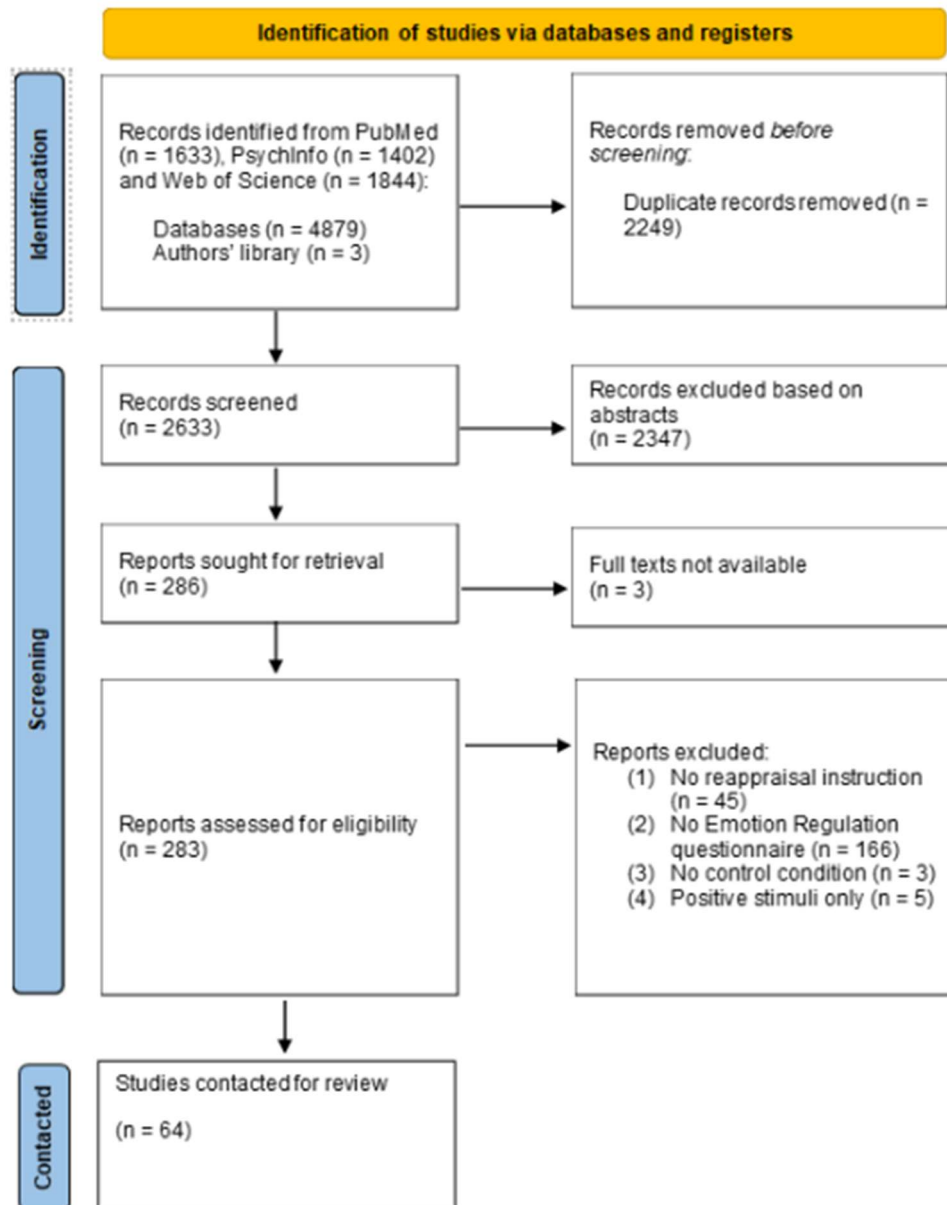


Figure S2. Flowchart of literature search and screening procedure

Flow diagram adapted from the PRISMA 2020 flow diagram template¹⁰⁶.

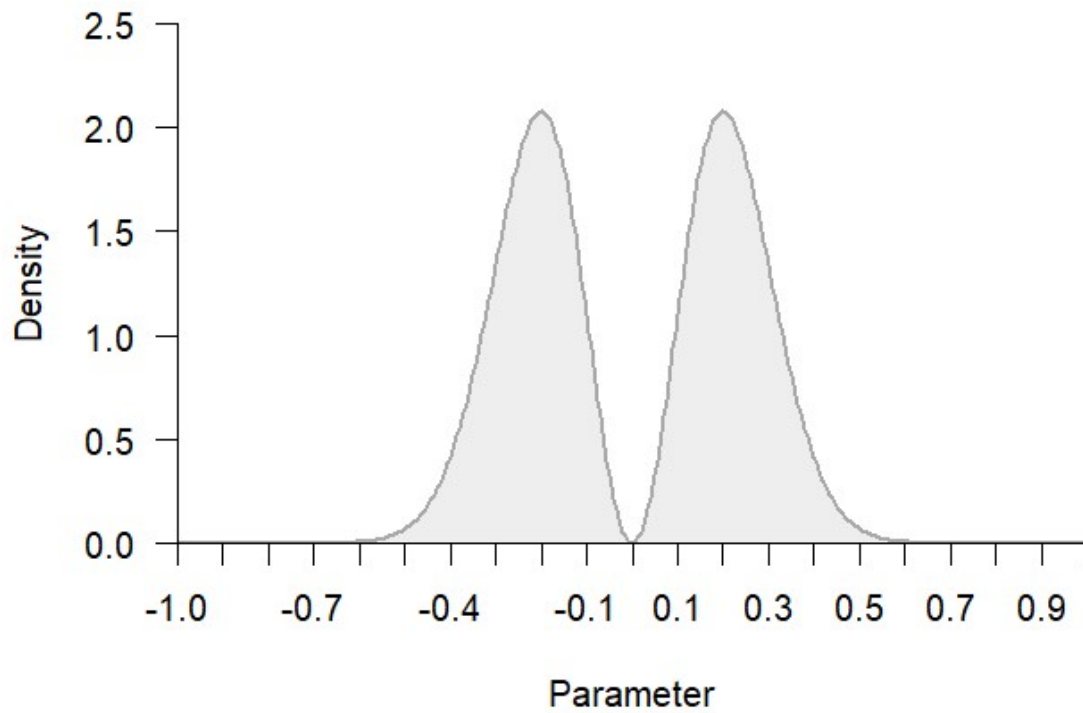


Figure S3. Effect-size prior for Bayesian meta-analysis

Prior distribution of effect sizes for Bayesian meta-analysis based on a normal moment prior¹⁰⁷ approximating the effect size distribution in individual difference research¹⁰⁸.

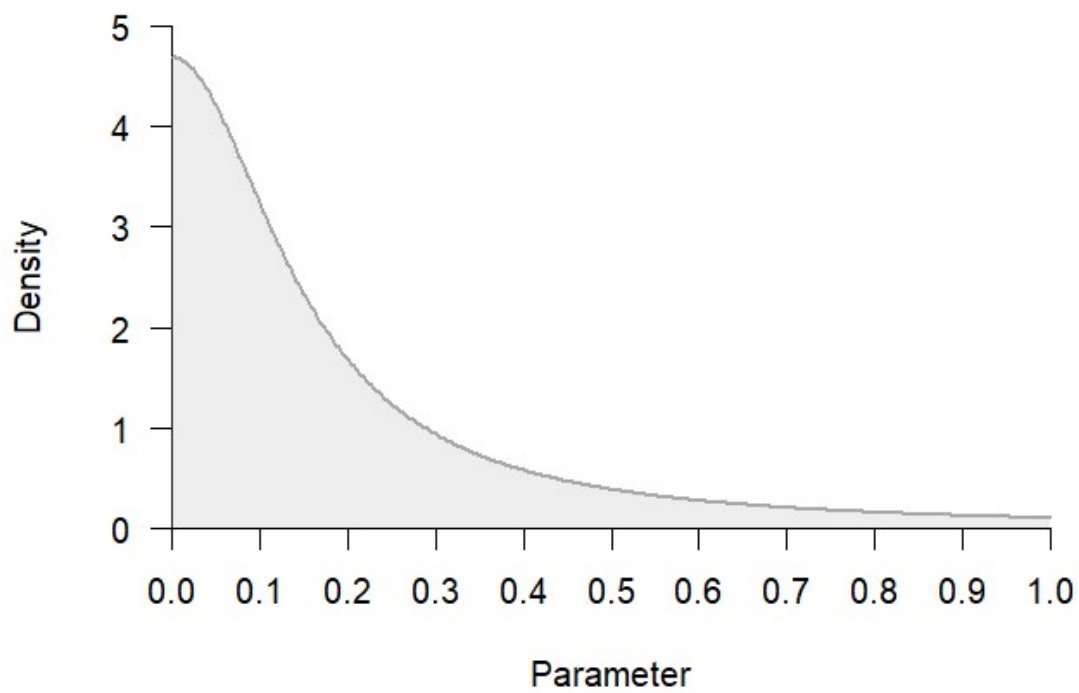


Figure S4. Heterogeneity prior for Bayesian meta-analysis

Prior distribution of between-study heterogeneity (τ) for Bayesian meta-analysis based on the empirical distribution of over 700 meta-analyses¹⁰⁹.

Supplementary Methods

Testing the custom meta-analytic functions

All steps for code quality checks can be found and reproduced using the MATLAB file “testMetaAnalysisFunction.m” on the GitHub repository. The meta-analytic code was tested in three steps. First, we tested the convergence of the core function with the results from the established R package *meta* and an in-built dataset¹¹⁰, indicating perfect agreement. Second, we assigned this dataset to an `fmri_data` object and tested whether our image-based function leads to identical results. Here, a deviation after seven decimal places was observed, which was due to the conversion from double to single precision arrays when using CanlabCore `fmri_data` objects. This is not an issue when using fMRI-based image data, which are loaded with single precision by default. Third, we searched for openly accessible study-wise group-level t-maps on NeuroVault for the contrast [painful stimulus - non-painful stimulus] to perform a small ad-hoc meta-analysis. We chose pain, as its neurobiology is relatively well understood and core regions replicate relatively well across different designs, making it a viable ground truth model. Here, we found seven matching images. One was removed due to insufficient coverage, leading to a sample size of $k = 6$ and $N = 279$. A list of permanent NeuroVault identifiers to retrieve the images can be found in the file “studyInfo_neurovault.xlsx” in the GitHub repository. The results of the final code for image-based random effects meta-analysis led to converging results with a recently published image-based meta-analysis on pain¹¹¹ (Figure S5) and were decoded as *pain* using Neurosynth at an image-topic correlation of $r = .44$, which is a relatively large correlation in our experience with this procedure. The previous meta-analytic results were slightly above this decoding precision ($r = .54$), which should be expected based on their larger sample size.

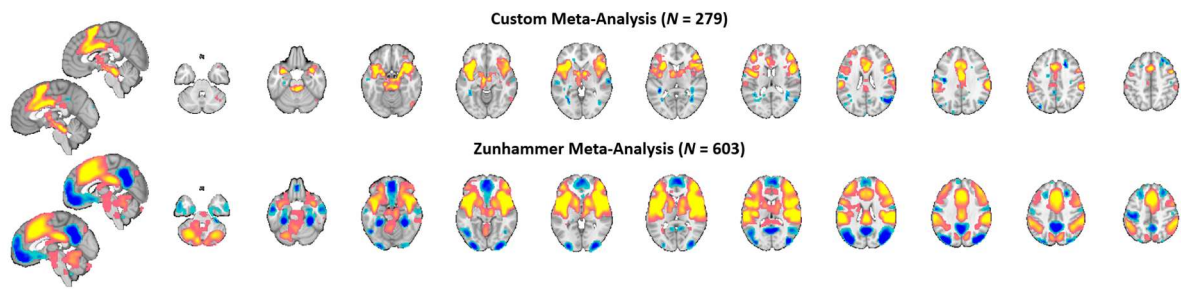


Figure S5. Validation of the custom image-based meta-analytic procedure

Comparison of a small ad-hoc meta-analysis with our custom code and previous meta-analytic results¹¹¹. Yellow voxels reflect activations, blue voxels reflect deactivations during pain processing (FDR-corrected at $q = .05$, two-sided). Note that the latter had a much larger sample size and was based on a systematic literature review, explaining the larger number of significant effects.

Power Analysis

We performed simulation-based sensitivity analyses to assess the statistical power of our voxel-wise analyses to detect a small population correlation of $r = .10$. We simulated scenarios with an increasing number of true clusters (1–4). Between-study heterogeneity was set to $\tau = .06$, corresponding to the third quartile in our empirical data, and supplemented by an additional simulation at $\tau = .08$ as a more conservative scenario.

Four anatomically distinct, non-contiguous clusters were defined within the preregistered network of interest. Cluster sizes were 800, 500, 300, and 200 voxels. To avoid artificially inflated power estimates that would arise from assuming independence across clusters, study-level cluster effects were simulated jointly according to the empirical correlation matrix of these four regions.

Power analyses were conducted for four nested scenarios: first, only the largest cluster was assigned a true effect; second, the two largest clusters; third, the three largest clusters; and fourth, all four clusters. Each scenario was simulated across 1,000 iterations. Power was quantified as the proportion of iterations in which at least one true cluster was detected after false discovery rate correction, either within the preregistered network of interest or across the whole brain. The results are shown below in Figure S6.

While the reliable ground truth of typical between-person effects in fMRI is unclear¹¹², our cluster sizes were guided by those reported in previous meta-analyses on clinical group differences in emotion regulation^{113,114}.

The necessary sample size for a single hypothetical study aiming to detect the reported correlation between amygdala down-regulation and task-based affective ratings ($r = .08416$) at 80% power was calculated using the *WebPower* package (v0.9.4)¹¹⁵.

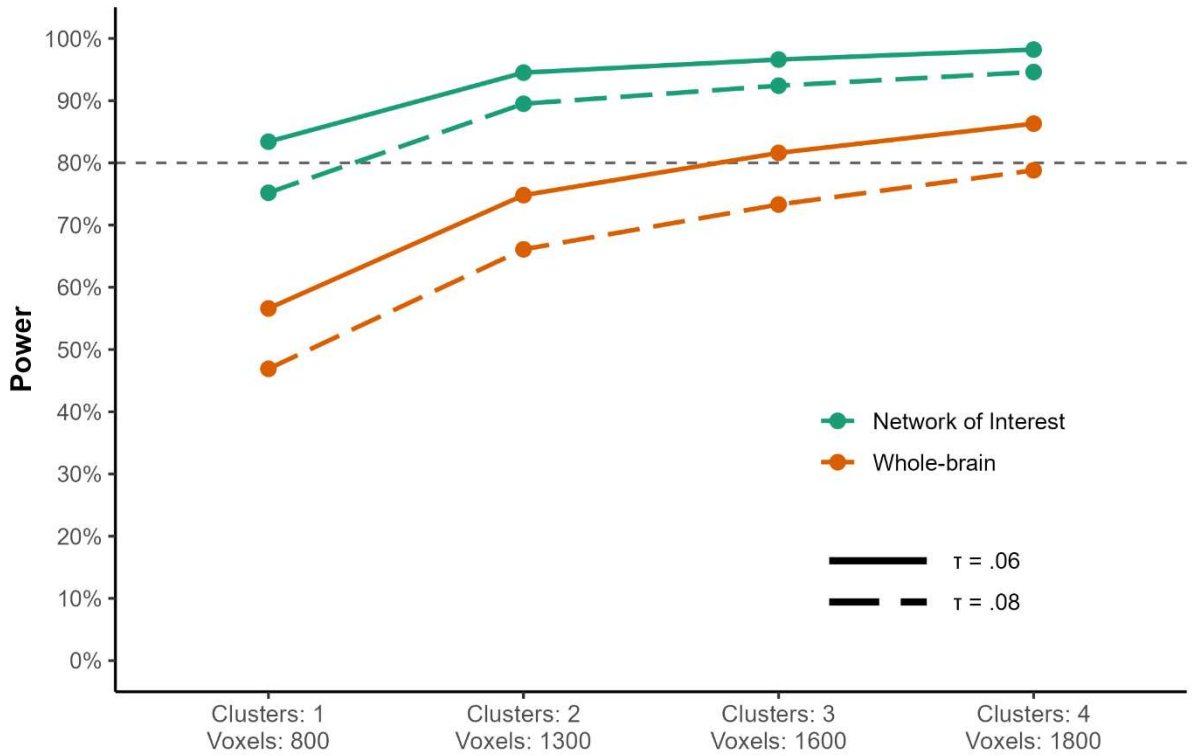


Figure S6. Statistical power for voxel-wise meta-analyses

Statistical power to detect at least one true cluster as a function of the number of simulated true clusters and their corresponding total voxel extent. Power was estimated across 1,000 simulation iterations for each scenario. Simulations were based on the empirical consortium design and assumed a population correlation of $r = .10$, with between-study heterogeneity of $\tau = .06$ or $\tau = .08$. Four non-contiguous clusters within the preregistered network of interest were considered, with sizes of 800, 500, 300 and 200 voxels. Results are shown separately for FDR correction within the preregistered network of interest and across the whole brain. Power reflects the proportion of simulation iterations in which at least one true cluster was detected after FDR correction ($q < .05$, two-sided).

Meta- vs mega-analytic approaches

We performed custom simulations to demonstrate that the relative equivalence of random effects mega- and meta-analysis holds for our targeted analyses. Datasets were simulated with a population correlation of $r = .20$ and between-study heterogeneity of $\tau = .10$. The number of simulated studies and their respective sample sizes were fixed to those for the voxel-wise correlations with trait questionnaires ($k = 37$, $N = 2091$). In total, 2000 iterations were simulated.

Mega-analysis was performed using lme4¹¹⁶. Study-wise data were z-transformed using their individual mean and standard deviation. This brings mega-analytic estimates to the same standardized correlation scale as the meta-analytic estimates to allow clear comparisons. Moreover, this is a reasonable preprocessing step in fMRI mega-analysis due to arbitrary scale differences between studies given different fMRI measurement protocols. It further removes the necessity for fixed and random study-wise intercepts, as they are set to 0 for individual studies by z-transformation, removing the disadvantage that mega-analysis would otherwise have to estimate a larger number of parameters than meta-analysis. The correlation-metric slopes were allowed to vary across studies (i.e., random slopes), which mirrors the assumption of between-study heterogeneity in effect sizes of random effects meta-analysis, which is a crucial step for both approaches^{117,118}.

The main results are shown below (Figure S7). The left panel shows the mean correlation estimate and its standard deviation across simulations. The discrepancy between mega- and meta-analytic estimates was minimal at $\Delta r = .006$. Both approximated the population effect size well. The right panel shows the precision of the estimates, operationalized as the width of 95% confidence intervals. The difference was again minimal at $\Delta 95\% \text{ CI} = 0.003$.

Figure S8 further shows how statistical power would have differed if a lower number of studies was included due to data sharing barriers, exemplary for a population correlation of $r = .10$ across 1000 true positive voxels with Bonferroni correction over 10,000 voxels to allow analytical solutions. For simplicity, each included study had a study-wise sample size of $n = 57$, corresponding to the average sample size in our empirical consortium sample. A lower number of studies would have substantially reduced statistical power, especially with increasing between-study heterogeneity.

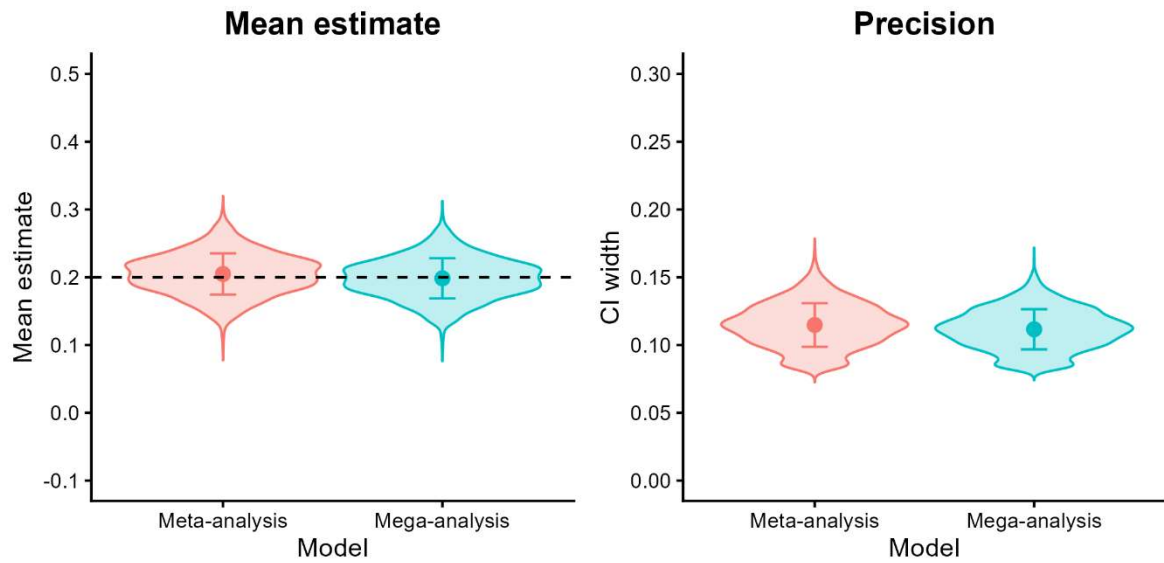


Figure S7. Comparison of meta- and mega-analytic estimates

Comparison of random-effects meta-analysis and random-effects mega-analysis across 2,000 simulated datasets. Each simulated dataset comprised $k = 37$ studies and $N = 2,091$ participants, matching the empirical consortium sample used for voxel-wise correlations with trait questionnaires. Data were simulated with a population correlation of $r = .20$ and between-study heterogeneity of $\tau = .10$. The left panel shows the distribution of estimated correlations across simulations; points indicate mean estimates and the dashed horizontal line indicates the true population correlation ($r = .20$). The right panel shows the distribution of 95% confidence-interval widths across simulations; points indicate mean interval width. Violin plots show the distribution across simulated datasets.

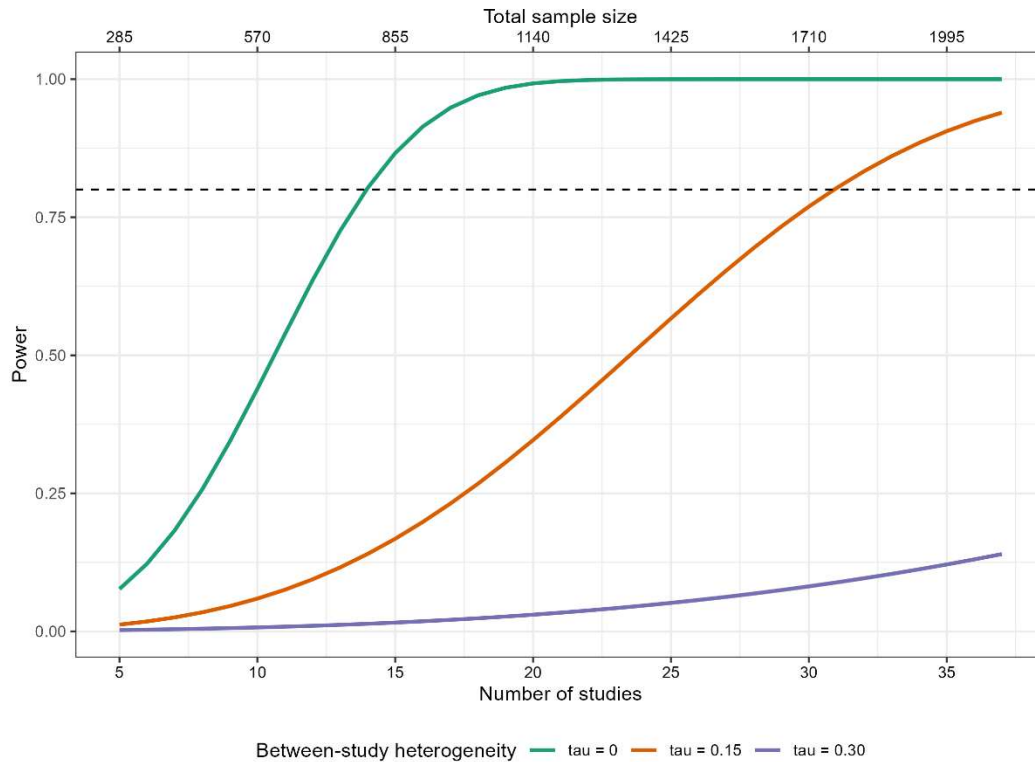


Figure S8. Statistical power as a function of the number of included studies

Statistical power to detect at least one true effect as a function of the number of included studies, with corresponding total sample sizes shown on the upper x-axis assuming $n = 57$ participants per study. Power was calculated for a population correlation of $r = .10$ and three levels of between-study heterogeneity ($\tau = 0, .15$, and $.30$). The calculation assumed 1,000 true-positive voxels, corresponding to 45 effective independent tests after accounting for the spatial covariance structure. Statistical significance was assessed using a two-sided Bonferroni-corrected threshold of $\alpha = .05/10,000$. The dashed horizontal line indicates the conventional 80% power threshold.

References

76. Benzait, A. *et al.* Hemodynamic correlates of emotion regulation in frontal lobe epilepsy patients and healthy participants. *Hum. Brain Mapp.* **44**, 1456–1475 (2023).
77. Diers, K. *et al.* Short- and long-term effects of emotion up- and down-regulation. *Imaging Neurosci.* **1**, 1–24 (2023).
78. Dörfel, D. *et al.* Common and differential neural networks of emotion regulation by Detachment, Reinterpretation, Distraction, and Expressive Suppression: A comparative fMRI investigation. *NeuroImage* **101**, 298–309 (2014).
79. Gaebler, M., Daniels, J. K., Lamke, J.-P., Fydrich, T. & Walter, H. Behavioural and neural correlates of self-focused emotion regulation in social anxiety disorder. *J. Psychiatry Neurosci.* **39**, 249–258 (2014).
80. Diers, K. *et al.* Should we keep some distance from distancing? Regulatory and post-regulatory effects of emotion downregulation. *PLOS ONE* **16**, e0255800 (2021).
81. Gärtner, A. *et al.* Impact of FAAH genetic variation on fronto-amygdala function during emotional processing. *Eur. Arch. Psychiatry Clin. Neurosci.* **269**, 209–221 (2019).
82. Gianaros, P. J. *et al.* Affective brain patterns as multivariate neural correlates of cardiovascular disease risk. *Soc. Cogn. Affect. Neurosci.* 1–12 (2020).
83. Glosemeyer, R. W. *et al.* Selective suppression of rapid eye movement sleep increases next-day negative affect and amygdala responses to social exclusion. *Sci. Rep.* **10**, 17325 (2020).
84. Hofhansel, L., Weidler, C., Clemens, B., Habel, U. & Votinov, M. Personal insult disrupts regulatory brain networks in violent offenders. *Cereb. Cortex* **33**, 4654–4664 (2023).
85. Jentsch, V. L., Merz, C. J. & Wolf, O. T. Restoring emotional stability: Cortisol effects on the neural network of cognitive emotion regulation. *Behav. Brain Res.* **374**, 111880 (2019).
86. Powers, J. P., Kako, N., McIntosh, D. N. & McRae, K. Competitive interactions between cognitive reappraisal and mentalizing. *Int. J. Psychophysiol.* **174**, 17–28 (2022).
87. Marín-Morales, A., Pérez-García, M., Catena-Martínez, A. & Verdejo-Román, J. Emotional Regulation in Male Batterers When Faced With Pictures of Intimate Partner Violence. Do They Have a Problem With Suppressing or Experiencing Emotions? *J. Interpers. Violence* **37**, NP10271–NP10295 (2022).
88. Min, J. *et al.* Emotion Downregulation Targets Interoceptive Brain Regions While Emotion Upregulation Targets Other Affective Brain Regions. *J. Neurosci.* **42**, 2973–2985 (2022).
89. Morawetz, C., Bode, S., Baudewig, J., Kirilina, E. & Heekeren, H. R. Changes in Effective Connectivity Between Dorsal and Ventral Prefrontal Regions Moderate Emotion Regulation. *Cereb. Cortex* **26**, 1923–1937 (2016).
90. Morawetz, C., Mohr, P. N. C., Heekeren, H. R. & Bode, S. The effect of emotion regulation on risk-taking and decision-related activity in prefrontal cortex. *Soc. Cogn. Affect. Neurosci.* **14**, 1109 (2019).
91. Morawetz, C., Steyrl, D., Berboth, S., Heekeren, H. R. & Bode, S. Emotion Regulation Modulates Dietary Decision-Making via Activity in the Prefrontal–Striatal Valuation System. *Cereb. Cortex N. Y. NY* **30**, 5731 (2020).
92. Morawetz, C., Berboth, S. & Bode, S. With a little help from my friends: The effect of social proximity on emotion regulation-related brain activity. *NeuroImage* **230**, 117817 (2021).

93. Mulej Bratec, S. *et al.* Cognitive emotion regulation enhances aversive prediction error activity while reducing emotional responses. *NeuroImage* **123**, 138–148 (2015).
94. Pierce, J. E., Blair, R. J. R., Clark, K. R. & Neta, M. Reappraisal-related downregulation of amygdala BOLD activation occurs only during the late trial window. *Cogn. Affect. Behav. Neurosci.* **22**, 777–787 (2022).
95. Paschke, L. M. *et al.* Individual differences in self-reported self-control predict successful emotion regulation. *Soc. Cogn. Affect. Neurosci.* **11**, 1193–1204 (2016).
96. Rehbein, E. *et al.* Estradiol administration modulates neural emotion regulation. *Psychoneuroendocrinology* **134**, 105425 (2021).
97. Sandner, M., Zeier, P., Lois, G. & Wessa, M. Cognitive emotion regulation withstands the stress test: An fMRI study on the effect of acute stress on distraction and reappraisal. *Neuropsychologia* **157**, 107876 (2021).
98. Guendelman, S., Bayer, M., Prehn, K. & Dziobek, I. Towards a mechanistic understanding of mindfulness-based stress reduction (MBSR) using an RCT neuroimaging approach: Effects on regulating own stress in social and non-social situations. *NeuroImage* **254**, 119059 (2022).
99. Sokołowski, A., Folkierska-Żukowska, M., Jednoróg, K., Wypych, M. & Dragan, W. Ł. It is Not (Always) the Mismatch That Beats You—On the Relationship Between Interaction of Early and Recent Life Stress and Emotion Regulation, an fMRI Study. *Brain Topogr.* **35**, 219–231 (2022).
100. Steward, T. *et al.* Dynamic Neural Interactions Supporting the Cognitive Reappraisal of Emotion. *Cereb. Cortex* **31**, 961–973 (2021).
101. Lloyd, W. K. *et al.* Longitudinal change in executive function is associated with impaired top-down frontolimbic regulation during reappraisal in older adults. *NeuroImage* **225**, 117488 (2021).
102. Tupitsa, E. *et al.* Heart rate variability covaries with amygdala functional connectivity during voluntary emotion regulation. *NeuroImage* **274**, 120136 (2023).
103. Graner, J. L. *et al.* Regulating Negative Autobiographical Memories: An fMRI Investigation of Reappraisal and Distraction in Middle-aged and Older Adults. *J. Cogn. Neurosci.* **38**, 299–318 (2026).
104. Huneke, N. T. M. *et al.* Effects of opioid antagonism on functional MRI correlates of explicit and implicit threat processing in healthy volunteers. *Br. J. Psychiatry* **228**, 530–538 (2026).
105. Jackson, D. & Turner, R. Power analysis for random-effects meta-analysis. *Res. Synth. Methods* **8**, 290–302 (2017).
106. Page, M. J. *et al.* The PRISMA 2020 statement: an updated guideline for reporting systematic reviews. *The BMJ* **372**, n71 (2021).
107. Pramanik, S. & Johnson, V. E. Efficient alternatives for Bayesian hypothesis tests in psychology. *Psychol. Methods* **29**, 243–261 (2024).
108. Gignac, G. E. & Szodorai, E. T. Effect size guidelines for individual differences researchers. *Pers. Individ. Dif.* **102**, 74–78 (2016).
109. van Erp, S., Verhagen, J., Grasman, R. P. P. & Wagenmakers, E.-J. Estimates of between-study heterogeneity for 705 meta-analyses reported in Psychological Bulletin from 1990–2013. *J. Open Psychol. Data* **5**, 4 (2017).
110. Balduzzi, S., Rücker, G. & Schwarzer, G. How to perform a meta-analysis with R: A practical tutorial. *Evid. Based Ment. Health* **22**, 153–160 (2019).

111. Zunhammer, M., Spisák, T., Wager, T. D., Bingel, U., & Placebo Imaging Consortium. Meta-analysis of neural systems underlying placebo analgesia from individual participant fMRI data. *Nat. Commun.* **12**, 1391 (2021).
112. Cremers, H. R., Wager, T. D. & Yarkoni, T. The relation between statistical power and inference in fMRI. *PLoS ONE* **12**, 1–20 (2017).
113. Morawetz, C., Hemetsberger, F. J., Laird, A. R. & Kohn, N. Emotion regulation: From neural circuits to a transdiagnostic perspective. *Neurosci. Biobehav. Rev.* **168**, 105960 (2025).
114. Picó-Pérez, M., Radua, J., Steward, T., Menchón, J. M. & Soriano-Mas, C. Emotion regulation in mood and anxiety disorders: A meta-analysis of fMRI cognitive reappraisal studies. *Prog. Neuropsychopharmacol. Biol. Psychiatry* **79**, 96–104 (2017).
115. Zhang, Z. & Mai, Y. WebPower: Basic and Advanced Statistical Power Analysis [R package]. (2023).
116. Bates, D., Mächler, M., Bolker, B. & Walker, S. Fitting Linear Mixed-Effects Models Using lme4. *J. Stat. Softw.* **67**, 1–48 (2015).
117. Barr, D. J., Levy, R., Scheepers, C. & Tily, H. J. Random effects structure for confirmatory hypothesis testing: Keep it maximal. *J. Mem. Lang.* **68**, 255–278 (2013).
118. Costafreda, S. G. Pooling fMRI Data: Meta-Analysis, Mega-Analysis and Multi-Center Studies. *Front. Neuroinformatics* **3**, 33 (2009).