Beyond Statistical Myopia:
Replying to a Misguided Critique of Mind-Body Research

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Abstract

In response to Gelman and Brown's recent critique of Aungle and Langer (2023), we argue that their article illustrates how narrow statistical reasoning and selective literature review can misrepresent and undermine credible scientific findings. Using their discussion of perceived time and physical healing as a case study, we identify three general problems: (a) a failure to accurately characterize the methods and results of the study they critique, (b) misinterpretations and omissions in their review of the relevant literature, and (c) a tendency to generalize from isolated statistical issues to sweeping claims about the invalidity of mind-body research. We adopt Gelman and Brown's recommended model and find that the main effect remains robust. We also document errors in their interpretations of other cited studies and demonstrate that they ignore decades of rigorous, well-replicated research on placebo effects and health mindsets. By examining their critique in detail, we highlight how methodological skepticism, when untethered from accurate reading and balanced appraisal, can mislead rather than clarify.

Introduction

Aungle and Langer (2023) published a paper titled *Physical healing as a function of perceived time*, in which they showed that mild bruises created by a standardized procedure objectively healed more when participants believed more time had passed, even though the elapsed time was always the same. Aungle and Langer employed a multipronged – or "swiss cheese" (Olson & Raz, 2021) – approach to make key aspects of the study design salient for participants and layer multiple elements the authors believed would influence participants' expectations about how quickly they would heal.

The study was advertised as a study on "personality and individual differences in healing after cupping therapy," so that the healing process was salient from the outset. Participants completed an enrollment survey that collected demographic information and measures of depression, anxiety, stress, and mindfulness, as well as an abbreviated measure of "the big five" personality dimensions, so that the cover story was maintained and the researchers could control for covariates known to influence physical healing. Participants then received cupping kits to practice the study procedure at home for one week before coming in for their three lab sessions.

The at-home exercises were included to create "conditioned expectations" of healing – i.e., anticipatory responses developed through repeated associations between the cupping procedure and the amount of healing observed over a 30-minute period – since conditioned expectations are known to facilitate placebo effects (e.g., Kirsch et al., 2014; Peciña et al., 2014; Schafer et al., 2015). The 30-minute timeframe was chosen because it was roughly equivalent to the 28-minute healing observation period participants would experience during their lab sessions. The at-home exercises required participants to perform the same cupping procedure Aungle and Langer applied in the lab; upload a photo of the bruise created by the cupping procedure; answer questions about how intense, irritated, discolored, severe, and visible the bruise looked; wait 30 minutes, upload another photo, and answer the same questions. Aungle and Langer wanted participants to develop implicit expectations of healing that were anchored to the length of the healing observation period they would experience during their lab sessions.

Participants then scheduled three separate lab sessions, to complete on different days during the week following their last day of the at-home exercises. Prior to receiving the cupping procedure, participants were told, “We’re varying a couple of things to help ensure we have a robust measure of healing, including how long we observe the process and the intervals between observations. The cupping procedure itself and the number of times you complete the healing observation survey will remain the same during each session.” A timer, which participants monitored to know when to complete each survey, was situated in front of the computer used to administer the healing observation surveys. Although participants were told they would go through three different healing observation periods – 14 minutes, 28 minutes, and 56 minutes – the elapsed time for all three healing observation periods was always 28 minutes. Aungle and Langer manipulated perceived time by programming the timer participants used for the healing observation surveys to run at half the speed of normal time in one session and twice the speed of normal time in another session.

Aungle and Langer made several efforts to ensure the perceived time manipulation would be both salient and difficult to detect. The timer only displayed the number of minutes that had passed to minimize the risk that participants would discover it had been manipulated. Upon arrival, participants were asked to relinquish all of their devices (including watches) and belongings to the experimenter for the duration of each session, so that they “would only be focused on the experiment.” Participants were told to expect each session to last one hour, so the study team could also collect pilot data for other experiments that were currently under development. The true reason “other experiments” were included was to balance participants’ perceived time with the true elapsed time for each session, so that their subjective duration was roughly equal to the one hour of actual elapsed time during each session. After the “56-minute” condition, participants watched and rated highly engaging YouTube videos, since people know they often underestimate time spent on highly absorbing activities: the authors wanted participants to assume they had spent less time watching the videos than they actually had. Similarly, after the “14-minute” condition, participants played a cognitively demanding game, since people tend to overestimate time spent on such activities. In the 28-minute condition (the true length of the healing observation period in all three conditions), participants were given the option to repeat the cupping procedure on their dominant forearm to see if that affected the healing process in any way.

Consistent with their hypothesis, Aungle and Langer found that the bruises created by the cupping therapy procedure objectively healed – as rated by judges blind to the study conditions and hypothesis – significantly more the more time participants believed to have passed, even though the elapsed time was always the same. In other words, they found significantly more healing in the "56"-minute condition compared to the 28-minute condition and significantly more healing in the 28-minute condition compared to the "14"-minute condition. Aungle and Langer contextualized this result in terms of existing literature on placebos and health mindsets and made an argument for mind-body unity: simultaneous and bidirectional influences of mind on body and body on mind.

Andrew Gelman and Nicholas Brown subsequently published a critique of this research (Gelman & Brown, 2024), in which they took issue with both the specifically reported findings and methodology of Aungle and Langer (2023) and with the broader literature upon which the study was based. We have three reasons for writing this reply. First, we wish to comment on the alleged statistical and methodological issues highlighted by Gelman and Brown. Second, we believe it is important to rebut their claim that Aungle and Langer (2023), and the science of mind-body unity more broadly, "misinterpret the scientific literature" and produce "unreplicable research" (p. 9)[1]. Third, we believe the assertion that there is "no clear theoretical justification" (p. 2) for the reported effect does not stand up to scrutiny.

Statistics and Methods in Aungle and Langer (2023)

Statistics

In their critique, Gelman and Brown made a persuasive case for including random slopes in Aungle and Langer's model. We agree with that part of their argument; indeed, we

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[1] All page numbers without full citations refer to Gelman and Brown's 2024 critique

learned something from their clear and easy-to-follow rationale for doing so. We would, however, underscore the following: even if Aungle and Langer use the model recommended by Gelman and Brown, the main effect of perceived time on physical healing remains – all that changes are the pairwise comparisons. The t-statistics comparing the 56-minute to the 28-minute and 14-minute conditions in that model are 2.8 and 2.1, respectively. In Gelman and Brown's own words "these differences are 2 or 3 *SE* from zero, which would seem to be unlikely to occur by chance alone" (p. 8). Moreover, although the comparison between the 28-minute and 14-minute condition is no longer significant, the difference is still positive, indicating more healing in the 28-minute condition. The violin plot presented in the paper clearly shows this, and this pattern of results is well-accounted for by Aungle and Langer's reasoning and hypothesis. But these positive findings are hard to explain in terms of Gelman and Brown's argument that the authors' study is, "fatally flawed in the sense of not providing evidence to support their strong claims" (Gelman and Brown, abstract). Aungle and Langer specifically commented on the violin plot in their paper, noting the proportion of participants who had fully healed in each condition (15.6% in the 14-minute condition, 24.2% in the 28-minute condition, and 34.4% in the 56-minute condition). Nonetheless, we concede the point – Aungle and Langer should have included random slopes – and we thank Gelman and Brown for teaching us something in that part of their paper.

Gelman and Brown were not only concerned about the point estimates, however, but also with the stability of, and uncertainty around, those estimates (p. 4). To address this issue directly, we quantify uncertainty rather than spotlighting point estimates. Using the random-slopes model Gelman and Brown recommend, the main effect is 1.105 healing points ($SE = 0.37$; $z = 2.99$), but more important are diagnostics of stability: the Type-S (sign) error is effectively zero (<0.1%) and the expected Type-M (magnitude) exaggeration conditional on significance is ≈1.09×, indicating a secure direction with only modest inflation (Gelman & Carlin, 2014). Study sensitivity is adequate: the minimum detectable effect at 80% power ($\alpha = .05$) is ≈1.03, below the estimate, so the result is not a "lucky" detection. A conservative publication-bias adjustment (Andrews & Kasy, 2019) shrinks the coefficient to 1.013 with a widened 95% interval [0.122, 1.820], which remains positive. Calibrating evidence in Bayesian terms, Bayes factors under weakly informative normal priors (τ = 0.5–2.0) fall in the ~11–18 range (with a Sellke–Bayarri–Berger bound of ≤22.4 from $p \approx .0028$). Finally, participant fixed-effects, cluster-robust SEs, and randomization-inference yield similar magnitudes. In short, we agree that the magnitude is uncertain, but the direction is stable and the evidence is moderate-to-strong, not unstable and weak.

Methods

We find Gelman and Brown's other critiques far less persuasive. For example, the authors suggested that Aungle and Langer may have tested a number of factors that influenced healing and only reported those that turned out significant:

> *"It would be easy to come up with hypotheses why such effects would occur only for patients with high or low levels of anxiety or stress or different levels of mindfulness, under only some conditions of mood, or for only some sorts of personality profiles" (Gelman and Brown, p. 5)*

This is entirely at odds with the planning of this study. For the benefit of Gelman, Brown, and their readers, we will explain the background to this work, and why the range of measures was chosen. This research was completed for Aungle's second year PhD project. As part of that process a faculty committee is formed, a proposal outlining the research question, methods, and analyses is submitted, and the committee's approval of that proposal is required before the student can proceed (Harvard University, 2024). The reasons for including measures of anxiety, depression, and stress were explained in Aungle's proposal and in Aungle and Langer's paper – these psychosocial variables have all been shown to affect physical healing (Walburn et al., 2009). Mindfulness was included because Langer's body of work indicates mindfulness influences a broad range of health outcomes (e.g., Langer, 2009, 2023; Langer et al., 2010; Langer & Rodin, 1976; Rodin & Langer, 1977). Aungle and Langer also included a highly abbreviated measure of personality – the Ten-Item Personality Inventory (Gosling et al., 2003) – because it is short and helped maintain their cover story with participants: the study was advertised as a study on "personality and individual differences in healing after cupping therapy." From the outset, the research question was whether manipulating perceived time objectively affects physical healing.

The study was not preregistered, and we accept that some may view the results as exploratory or as less trustworthy as a result. We acknowledge that the results are open to such a characterization in the absence of preregistration. We do not subscribe to that view ourselves and believe the experimental design itself - which revolves entirely around manipulating perceived time - as well as the PhD project proposal process, overseen by Aungle's faculty committee at Harvard, make clear that the hypothesis was clear from the outset. Whether the results reported by Aungle and Langer represent "weak" or "strong" evidence is open to interpretation. We believe they are "strong" evidence that our thoughts shape our health, because we understand them within the context of existing placebo and health mindset research. But if one evaluates them solely in terms of the effect of perceived time on physiological outcomes, we can appreciate that some may view the evidence as "weak," since the link between perceived time and physiology is a comparatively new line of inquiry. Notably, there are a couple of other studies that similarly reveal physiological effects of perceived time (e.g., Matta et al., 2024; Park et al., 2016).

Gelman and Brown also misread – or misunderstood – the study procedure:

> *"During the half hour of the experimental conditions, the participants were performing various activities on the computer that could affect blood flow, and these activities were different in each condition (watching videos under one condition, playing Tetris in another, playing a different video game in the third)" (Gelman and Brown, p. 6).*

The claim above is incorrect. Each condition was designed to last one hour. In all three conditions, participants played Tetris between healing observation surveys. The various activities that differed by condition were included to balance out perceived time with actual time, so that participants would not become suspicious of the time manipulation. Aungle and Langer had participants watch fun and entertaining videos after the healing observation period in the Fast Time condition, so that when participants left the lab it would be more believable that only one hour had passed. Similarly, participants were required to play a tedious, cognitively demanding game in the Slow Time condition for approximately 20 minutes after the healing observation period, so that their subjective sense of time (14-minutes + the time spent playing the tedious game) would plausibly equal the 1 hour of actual time that had passed by the time they left. *The procedure for the healing observation period itself was the same in all three conditions*.

It is concerning that this aspect of Gelman and Brown's critique was overlooked by the authors and their reviewers, given the allegation that Aungle and Langer were "uncritically citing work" (p. 10) and based their reasoning on "a literature […] built on sand" (p. 10). It begs the question, how closely did Gelman and Brown read Aungle and Langer's paper, let alone the literature they assert is built on sand?

The Literature Reviewed by Gelman and Brown

The title of Gelman and Brown's critique – "How statistical challenges and misreadings of the literature combine to produce unreplicable science: An example from psychology" – is a bold, provocative choice. When someone alleges that a literature has been misread, they suggest that they have 1) read the literature and 2) read it correctly. We have at least three reasons for doubt. First, Gelman and Brown reviewed just three studies in this literature. Second, they misread one of those studies, mischaracterized the replicability of another, and made incorrect assertions about the methodology of the third. Third, they provided no basis for making general assertions about the referenced literature as a whole.

Of the 28 peer-reviewed publications relevant to mind-body effects and health cited by Aungle and Langer, Gelman and Brown reviewed only three in support of their argument that the literature on mind-body unity is built on sand. We have already pointed out their patchy reading of Aungle and Langer (2023). The authors also mischaracterized the results from Crum and Langer (2007). In that study, one group of hotel housekeepers were informed their work met federal exercise guidelines, while another group received no such information. Over four weeks, the informed group saw improvements in weight, blood pressure, and waist-to-hip ratios, despite no changes in behavior or activity levels. Gelman and Brown took a survey measure included as a manipulation check and mischaracterized it as contradictory evidence: "the data in Crum and Langer (2007) actually did show a large increase in perceived amount of exercise (the average going from 3.8 to 5.7 on a scale from 0 to 10), so if the survey responses are to be believed, this directly contradicts the claim that the participants 'maintained their same diet and level of physical activity'" (Gelman and Brown, p. 8). The purpose of that measure was to confirm that the housekeepers led to believe their work was exercise did in fact perceive themselves to be exercising more. In other words, perceived exercise increased, as intended, for those who received the intervention.

In alleging an "absence of any theoretical account of how the intervention might work without inducing change in diet and exercise," Gelman and Brown (2024, p. 8) ignored Crum and Langer's introductory discussion of placebo effects and their connections to physical and mental health. The mechanisms underlying placebo effects are often difficult to establish, in part because they vary by treatment context (e.g., the placebo effect for pain is mediated by different brain regions than the placebo effect for Parkinson's disease; see Ashar et al. (2017) for a detailed discussion). The difficulty in precisely identifying the biopsychological mechanisms underlying placebo effects does not undermine the reality of placebo effects. Gelman and Brown's decision to exclude the placebo phenomenon from their commentary both ignores Crum and Langer's proposed mechanism and undermines their own reasoning about mind-body science in general.

Claiming that Crum and Langer's study lacked a theoretical background ignores later work on the idea that beliefs about physical activity independently shape physical health. For example, using data from three large U.S. samples, Zahrt and Crum (2017) investigated whether perceptions of physical activity influenced mortality risk independent of actual physical activity. Participants who perceived themselves as less active, regardless of actual activity, had significantly higher mortality rates. Zahrt and Crum devoted the entire introduction to contextualizing their study in terms of the findings reported by Crum and Langer (2007) and discussed three ways in which the housekeepers' beliefs about adequate physical activity could have shaped their physical health: motivation (e.g., "making beds more energetically," Zahrt and Crum 2017, p. 1018), affect (e.g., reducing stress about adverse heath), and placebo (e.g., perceiving more exercise conferring some of the benefits of exercise itself). Their observational study then formed the basis for later experimental and theoretical work to understand these effects in more depth (Zahrt, 2020; Zahrt & Crum, 2020; Zahrt et al., 2023).

We question the basis of Gelman and Brown's claim that the findings of Crum and Langer (2007) are unreplicable. In the absence of replication attempts, no such claim makes sense. No exact replication has been attempted, so we do not know. As Gelman has noted on his blog (Gelman, 2024), there tends to be little appetite for exact replications – a systemic issue across many fields of academic publishing that needs to change if researchers are to invest the considerable time and resources required. If Gelman and Brown argue that there is no basis for asserting that beliefs about physical activity can independently affect physical health, this is a gross misrepresentation of the literature.

Finally, Gelman and Brown criticized Leibowitz et al. (2018), a study examining whether physician assurance accelerated recovery from skin prick irritation. They assumed that itchiness measures were collected retrospectively and argued that significantly undermined the conclusions drawn by Leibowitz et al. Neither author, however, contacted Leibowitz to confirm their understanding of the study's procedure. When Aungle reached out, he learned that the measures were collected in real time on lab iPads — not retrospectively. Gelman and Brown also implied that Leibowitz and colleagues selectively reported data: in fact, the dataset was divided for publication across two conceptually distinct papers. A larger project on open-label placebos tested how various components — such as the cumulative effect of a supportive provider relationship, medical ritual, positive expectations, and a rationale about the power of placebos — contributed to open-label placebo effects (Leibowitz et al., 2019). The 2018 and 2019 papers separately reported on different conditions from the same larger dataset.

Two Major Gaps in Gelman and Brown's Literature Review

In addition to errors in their analysis of the papers they chose to discuss, Gelman and Brown's selective engagement with the literature cited in Aungle and Langer (2023) undermines their claims about mind-body science. They focused on three studies while ignoring the most well-established and empirically validated research that supports the claim that psychological states influence physiological outcomes (e.g., Crum, Leibowitz, et al., 2017; Howe et al., 2017; Kivimäki & Steptoe, 2018; Levy et al., 2009; Rosenzweig et al., 2010; Wager & Atlas, 2015; Walburn et al., 2009).

Indeed, an analysis of the literature reviews in Aungle and Langer (2023) and Gelman and Brown (2024) underscores this asymmetry. For Gelman and Brown (2024), the gap between their cited papers and the broader set of "connected" papers is roughly 0.10, whereas for Aungle and Langer (2023) the gap is only about 0.03 (see Figure 1). A larger gap indicates that the authors preferentially cite studies that are more semantically similar to their own work than the wider pool of relevant research identified by the Connected Papers algorithm (Smolyansky, 2020). In other words, Gelman and Brown's references reflect a narrower slice of the conceptual neighborhood, consistent with selective "cherry-picking," whereas Aungle and Langer's citations more closely approximate the diversity of the surrounding literature. Because the connected set is algorithmically derived from co-citation and bibliographic coupling patterns in the Semantic Scholar corpus, it provides a neutral baseline for what a broad, non-selective literature search might reveal. Against this baseline, Gelman and Brown's literature review appears significantly more restricted, relative to its universe of connected papers.

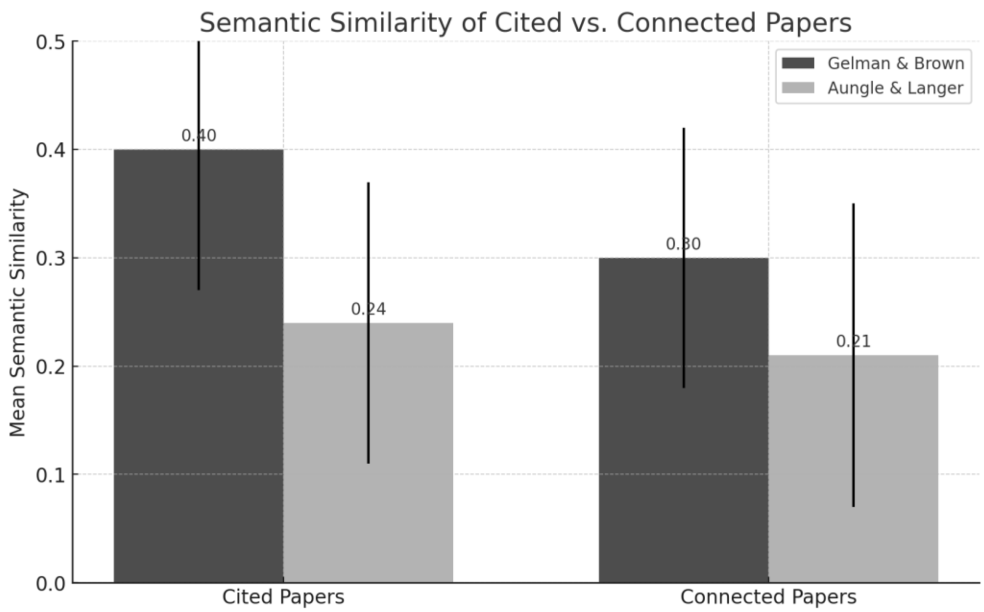

Figure 1

More specifically, their critique failed to acknowledge decades of research on placebo effects or health mindsets — phenomena that have been replicated extensively across medical and psychological sciences. Placebo effects are among the most robust and well-replicated findings in medical and psychological research, with studies consistently demonstrating that expectations, beliefs, and social cues can produce measurable physiological changes (e.g., Ashar et al., 2017; Wager & Atlas, 2015). The documented mechanisms underlying placebo effects range from endogenous opioid release in pain reduction to changes in dopamine and serotonin pathways in Parkinson's disease (Benedetti & Amanzio, 1997; Benedetti et al., 2005; de la Fuente-Fernández et al., 2001). We do not understand how Gelman and Brown can make broad claims about mind-body science without addressing *any* placebo research.

It is similarly unclear why Gelman and Brown ignored the growing body of work on health mindsets – arguably the research most relevant to the claims made in Aungle and Langer (2023). Health mindset research demonstrates that the health-relevant processes underlying placebo effects extend far beyond receiving inert medical treatments (e.g., Cohen et al., 2006; Crum, Akinola, et al., 2017; Crum et al., 2013; Crum et al., 2023; Feinstein et al., 2017; Petrie et al., 2002; Scheier et al., 1999; Segerstrom et al., 1998). For example, Levy and colleagues' research on aging stereotypes and their effects on health and longevity has repeatedly demonstrated that internalized beliefs about aging predict long-term health outcomes, independent of baseline health, socioeconomic status, and other confounding variables (Levy, 2009; Levy & Myers, 2004; Levy et al., 2002). In one particularly striking study, Levy et al. (2009) found that individuals who held more negative stereotypes about aging earlier in life had significantly higher rates of cardiovascular disease decades later, even when controlling for known risk factors such as smoking, cholesterol levels, and family history. This body of research provides evidence that not only do psychological factors influence responsiveness to medical treatments but they also have concrete, measurable effects on physiological health and disease progression in everyday life. By neglecting this literature, Gelman and Brown (2024) misrepresented the breadth and depth of mind-body research, opting to critique a narrow subset of studies while ignoring some of the most compelling evidence in the field.

The failure of Gelman and Brown (2024) to engage with replicable placebo research and health mindset research undermines their claim that Aungle and Langer (2023) is based on misinterpretations of the literature. Their focus on a selected handful of studies suggests that their critique is not a comprehensive evaluation of mind-body research, but rather a strategically narrow attack designed only to undermine confidence in the field. A more thorough and balanced assessment would have required them to acknowledge the extensive evidence supporting psychological influences on physical health, particularly in areas where the mechanisms are well-documented and widely accepted within the relevant scientific communities.

Theoretical justification for mind-body unity

Gelman and Brown assert that there is "no clear theoretical justification" for Aungle and Langer's findings, even though they are explicitly grounded in prior research on placebo effects and health mindsets. The idea that cognitive processes can impact physiological outcomes is supported by decades of rigorous, well-replicated studies in psychology and medicine. These studies demonstrate that our beliefs, expectations, and attention produce measurable changes in the body. Aungle and Langer (2023) argue that the breadth of psychological influences on physical health remains understudied and underappreciated, and they offer their results as a case-in-point. Although Aungle and Langer do not offer a formal theoretical account of their findings in their paper, there are several such accounts of placebo effects (e.g., Ashar et al., 2017; Pagnini et al., 2023; Pagnini et al., 2024). Moreover, forthcoming work by Aungle and colleagues does provide a directly applicable theoretical framework, Embodied Models of Health (EMH) (Aungle, Matta, Chen, et al., in prep; Aungle, Matta, Loecher, et al., in prep). This model integrates three previously disconnected factors – beliefs, expectations, and attention – into a unified explanation of mind-body effects on health. The EMH framework provides a generalizable, mechanistic account of mind-body unity that connects otherwise fragmented findings. By formally accounting for mental factors (beliefs, expectations, attention) and known biological pathways, it addresses long-standing critiques that mind-body research lacks a unifying theory or plausible mechanism.

Conclusion

Had Gelman and Brown confined their critique to the statistical modeling choices in Aungle and Langer (2023), we would have welcomed their input without feeling the need to formally respond. Their point about random slopes is well taken and strengthens the analysis without altering the central result. Our concern arises not from such constructive suggestions, but from the broader generalizations they draw — generalizations that, in our view, rest on selective readings and some misunderstandings of both our work and the wider literature.

Mind–body research rests on a robust foundation, built over decades of rigorous studies on placebo effects, health mindsets, and related phenomena. These literatures converge on the conclusion that beliefs, expectations, and attention can shape physiological outcomes. Our study contributes to this tradition and is consistent with emerging theoretical frameworks, including our Embodied Models of Health framework. We are grateful for the opportunity to clarify our methods and reasoning, and we appreciate

Gelman and Brown's contributions to improving statistical practice. At the same time, we encourage future critiques of mind–body research to adopt the same standards of balance and accuracy that we all expect in scientific discourse. In this way, disagreements can serve their best purpose: advancing shared understandings of how mind and body interact to shape health over time.

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