

Closed-loop digital meditation improves sustained attention in young adults

David A. Ziegler^{1,2,3*}, Alexander J. Simon^{1,2,3}, Courtney L. Gallen^{1,2,3}, Sasha Skinner¹, Jacqueline R. Janowich⁴, Joshua J. Volponi^{1,2,3}, Camarin E. Rolle⁵, Jyoti Mishra⁶, Jack Kornfield⁷, Joaquin A. Anguera^{1,2,3,8} and Adam Gazzaley^{1,2,3,8,9*}

Attention is a fundamental cognitive process that is critical for essentially all aspects of higher-order cognition and real-world activities. Younger generations have deeply embraced information technology and multitasking in their personal lives, school and the workplace, creating myriad challenges to their attention. While improving sustained attention in healthy young adults would be beneficial, enhancing this ability has proven notoriously difficult in this age group. Here we show that 6 weeks of engagement with a meditation-inspired, closed-loop software program (MediTrain) delivered on mobile devices led to gains in both sustained attention and working memory in healthy young adults. These improvements were associated with positive changes in key neural signatures of attentional control (frontal theta inter-trial coherence and parietal P3b latency), as measured by electroencephalography. Our findings suggest the utility of delivering aspects of the ancient practice of focused-attention meditation in a modern, technology-based approach and its benefits on enhancing sustained attention.

Media and technology multitasking have become pervasive in the lives of young adults¹ and research has shown that this behaviour is associated with challenges to their attention abilities that present as increased distractibility^{2,3}, diminished attention span⁴, poorer academic performance and reduced personal contentment¹. Given that attention is a fundamental component process of all aspects of higher-order cognition (for example, memory, decision making, goal management and emotional regulation)^{5,6}, there exists a need for new methods to enhance attention abilities. Attempts to do so have largely focused on populations with documented attention deficits, such as children diagnosed with attention deficit hyperactivity disorder and older adults^{7–9}. Interestingly, a few approaches, such as playing action videogames¹⁰ and physical fitness training¹¹, have been associated with fostering improved attention in healthy young adults; however, the benefits of these activities are not sufficient on their own to completely address modern day challenges to attention. Furthermore, while readily available, these methods are not universally appealing, and attempts to boost attention using technology-based approaches, such as cognitive training software programs, have revealed only minimal value in this age group¹².

Although young adults display superior attention abilities than clinical or other vulnerable populations, who are typically most responsive to benefits of treatments^{7–9}, they nevertheless face numerous real-world challenges to attention. Perhaps as a result, the use of stimulants has skyrocketed in non-diagnosed college students attempting to increase their focus and boost school performance, with non-prescription use rates as high as 43% among college students^{13–15}. However, stimulant use in this population has not been found to actually improve sustained attention¹³ and

misuse is associated with a multitude of substance abuse complications and even life-threatening outcomes¹⁴. Thus, there is an important, and growing, need for new accessible and affordable approaches to enhance attention abilities in healthy young adults, which are validated by randomized controlled trials.

A growing scientific literature supports the positive effects of real-world practices of focused-attention meditation as a means of improving self-regulation processes, including the deployment of sustained and selective attention^{9,16,17}, as well as its beneficial influences on stress, mood and emotional regulation¹⁸. While the majority of these studies have been conducted on middle-aged and older adults, a few studies have shown that the practice of meditation may increase attention abilities in healthy young adults^{19,20}. However, traditional forms of meditation can be challenging, intimidating and expensive to learn, because they require access to trained expert facilitators and in-person meetings over multiple months¹⁸. Moreover, these practices do not offer quantifiable metrics of success, nor performance feedback during the programme—factors that are important for maintaining engagement and long-term compliance²¹. Furthermore, traditional meditation is not adaptive or tailored to individuals, making it overly challenging for some people. Although more recent studies have tested the delivery of website-accessible meditation programs²², these online practices tended to duplicate the procedures of the real-world practices and consequently had similar hurdles in terms of implementation and traction. Moreover, while meditation ‘apps’ on mobile devices have become increasingly ubiquitous²³, studies to date have either failed to characterize their effects on cognition or have shown equivocal results²⁴.

To extend the benefits of focused-attention meditation to larger numbers and more diverse populations, we adopted an ‘East meets

¹Department of Neurology, University of California San Francisco, San Francisco, CA, USA. ²Neuroscape, University of California San Francisco, San Francisco, CA, USA. ³Weill Institute for Neurosciences & Kavli Institute for Fundamental Neuroscience, University of California San Francisco, San Francisco, CA, USA. ⁴Department of Psychology, University of New Mexico, Albuquerque, NM, USA. ⁵Department of Psychiatry, Stanford University School of Medicine, Palo Alto, CA, USA. ⁶Department of Psychiatry, University of California San Diego, La Jolla, CA, USA. ⁷Spirit Rock Meditation Center, Woodacre, CA, USA. ⁸Department of Psychiatry, University of California San Francisco, San Francisco, CA, USA. ⁹Department of Physiology, University of California San Francisco, San Francisco, CA, USA. *e-mail: david.ziegler@ucsf.edu; adam.gazzaley@ucsf.edu

West' approach. Unlike other meditation apps in the consumer market place that offer a digital version of traditional methods to guided mediation, our approach involved designing, developing and testing a meditation-inspired software program that integrates key aspects of traditional focused-attention meditation with a neuroplasticity-based, closed-loop approach to cognitive enhancement that has proven successful in generating positive outcomes with other interventions⁷. The MediTrain program (see Supplementary Fig. 1 and Methods) is a standalone software application deployed on mobile devices, designed to make the practice of focused-attention meditation more accessible, deliverable and sustainable, notably to younger generations who navigate towards the use of mobile digital devices¹. To constrain interpretations regarding which active ingredients confer benefits, we restricted our approach to specific, but fundamental, aspects of focused-attention meditation (that is, focused internal attention to the breath, awareness of distractions and return of focus to the breath).

When engaging with MediTrain, users are first instructed on how to focus their attention on their breath (for example, sensation of air in the nostrils or movement of their chest) with their eyes closed. Following this lesson, they begin the training trials, during which they are instructed to be aware of their mind's wandering (either due to an attentional lapse or a distraction), and when it is detected, to shift their attention back to their breath (see Supplementary Information for detailed instructions). On the basis of their introspection and self-report of mind-wandering, the closed-loop aspect of MediTrain uses an adaptive staircase algorithm to adjust the difficulty of the next trial (that is, increase in duration when they report that their focus was maintained, and decrease when focus wavered from the breath). This closed-loop approach allows the delivery of a focused-meditation challenge in a personalized manner and offers regular feedback on the effectiveness of an individual's ability to sustain their attention to their breath, as well as metrics of improvement over time. Another strength of this program is its delivery on wireless mobile platforms (that is, tablets or phones), which has the practical benefit of broad accessibility and appeal to younger generations.

A common criticism of previous meditation research and cognitive training studies is the absence of suitable control groups to address potential placebo effects driven by participant expectations that an intervention will lead to positive effects^{12,18,25}. To address this concern, we conducted experiment 1 to select an appropriate control condition based on participant predictions of potential training-related cognitive gains. Specifically, we asked a large sample of young adults ($n=334$) to report their expectations regarding the extent that engaging with MediTrain or a selection of other mobile software applications (see Methods for details of experiment 1) would improve their performance on our specific outcome measures. This experiment revealed a set of apps that, compared to MediTrain, revealed no significant differences in participant expectations of improvement on any of our outcome measures, indicating that they would serve as an appropriate placebo control condition.

Results and discussion

On the basis of key findings from the traditional meditation literature, we hypothesized that MediTrain would lead to improvements in sustained attention¹⁹ beyond that of an expectancy-matched control group. To test this hypothesis, we designed experiment 2 as a randomized, double-blind, placebo-controlled study. We randomly assigned 59 healthy young adults (18–35 years of age) to receive 6 weeks of at home treatment with either MediTrain or the expectancy-matched placebo program (see Methods and Supplementary Fig. 2 for details of randomization). Of these participants, 24 participants in the MediTrain group and 20 participants in the placebo group completed the full treatment and the pre-outcome and post-outcome assessments. We excluded two participants in the

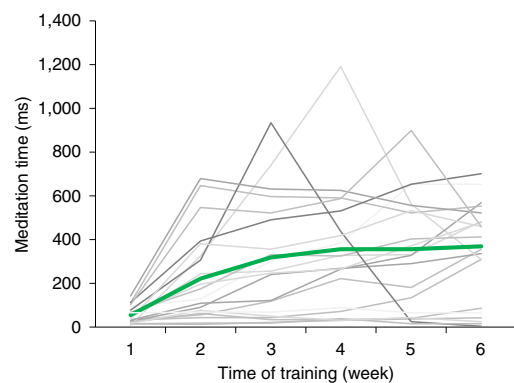


Fig. 1 | MediTrain training curves. Each grey line represents data from an individual participant in the MediTrain group ($n=20$) and the green bar represents the average of all participants. On average, the group went from an initial time of 20 s to 47.5 s at the end of the first week to a time of 346 s at the completion of the sixth week.

MediTrain group due to irregularities in their software usage (see Methods for details), resulting in a final analysed sample of 22 participants in the MediTrain group. We found that, on average, participants in the MediTrain group gradually increased the amount of time each day that they successfully sustained their attention to their breath without attentional lapses or distractions, averaging 20 s on the first day and progressing to an average of 6 min after 25 d of training (Fig. 1).

As our primary outcome measurement of interest, we evaluated intra-individual variability in response time across trials (RTVar) on a vigilance task that required a rapid response to a rare target stimulus (Fig. 2a). This well-studied measure of sustained attention assesses the consistency of attentional deployment during extended task engagement, as influenced by mind-wandering events and brief periods of distractibility^{19,26}. RTVar has been shown to be increased in children with attention deficit hyperactivity disorder²⁷ and older adults with mild cognitive impairment^{28,29} and dementia³⁰. This measure has also been shown to be reduced in middle-aged adults in response to traditional practices of meditation, and presented as evidence of meditation improving sustained attention abilities³¹.

Using an analysis of covariance (ANCOVA) to evaluate post-intervention scores while controlling for pre-intervention levels (see 'Statistical methods' in Methods), we found that RTVar differed significantly between the two groups following training, with less variability in response time (RT) across the task period in the MediTrain group ($F_{1,37}=6.4$, $P=0.016$, Cohen's $d=-0.66$; Fig. 2b). Analysis further revealed that participants in the MediTrain group showed a significant decrease in RTVar from pre-training to post-training (Fig. 2d–f; $\Delta=-7.98$ ms, paired $t_{21}=-3.5$, $P=0.002$, 95% CI: -12.7 to -3.1), whereas participants in the placebo group did not change ($\Delta=1.1$ ms, paired $t_{17}=0.29$, $P=0.78$, 95% CI: -7.0 to 9.1). Note that two participants in the placebo group were excluded from these analyses as they were extreme outliers (see Methods for details of outlier analyses and exclusions), resulting in a final as-treated sample of $n=22$ in MediTrain and $n=18$ in placebo for this task. To ensure that the decrease in RTVar was not related to a general RT slowing or trade-off in accuracy, we performed a post-hoc analysis of RT and the sensitivity index (d'). Controlling for pre-intervention levels, an ANCOVA revealed no significant group difference in RT at post-intervention ($F_{1,37}=0.14$, $P=0.71$, Cohen's $d=-0.15$, 95% CI: -189.5 to 276.5 ; Fig. 2c), but did reveal a significant difference in d' (ANCOVA $F_{1,37}=6.7$, $P=0.014$; Cohen's $d=0.39$, 95% CI: 0.09 – 0.79), with the MediTrain group performing significantly better than placebo controls at post-intervention. These

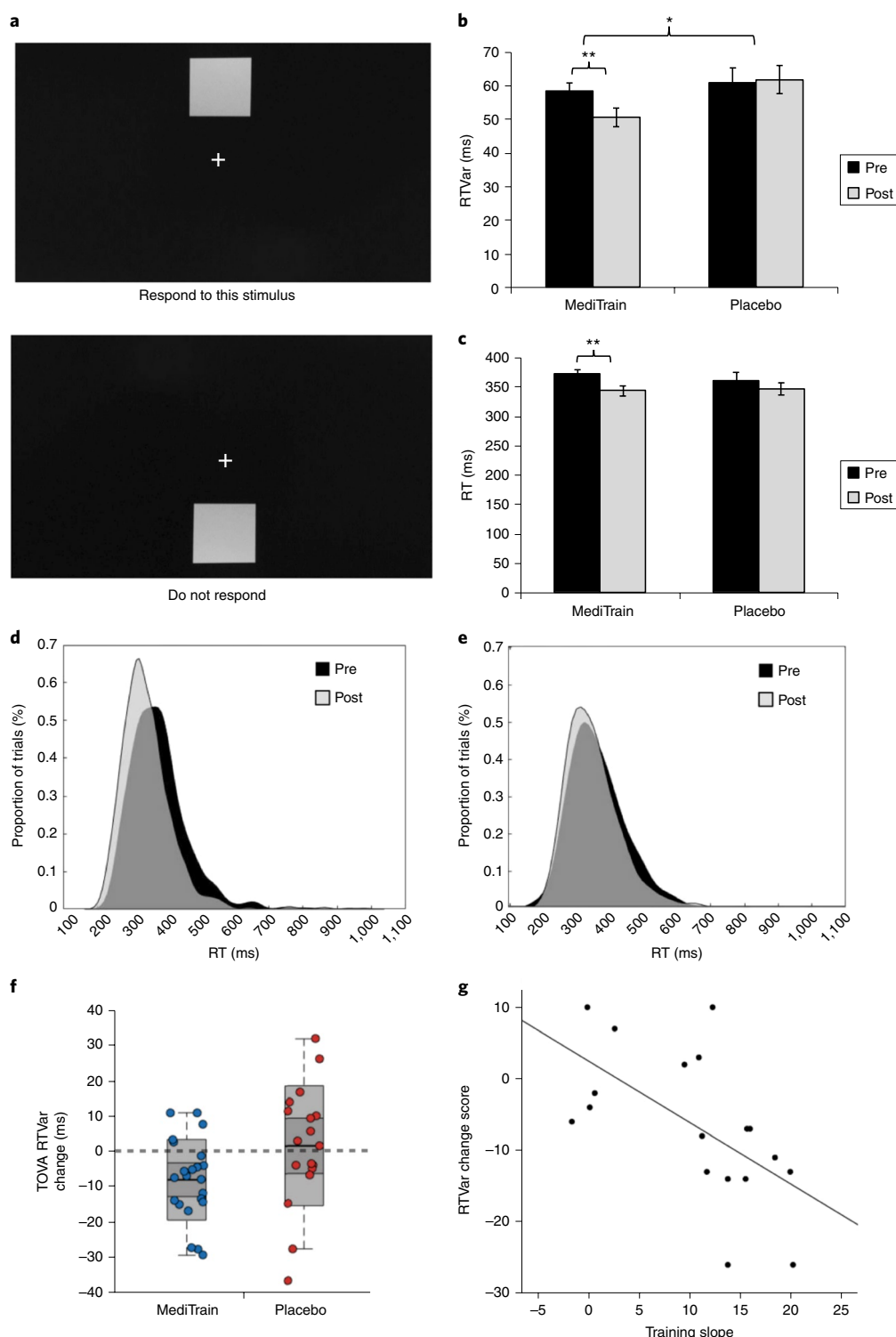


Fig. 2 | Improvements in sustained attention. **a**, Stimuli and protocol for the vigilance task. **b**, RTVar post-treatment was significantly lower (ANCOVA $F_{1,37} = 6.4$, $P = 0.016$, Cohen's $d = -0.66$, 95% CI: -17.8 to -2.0) in MediTrain ($n = 22$) than in placebo ($n = 18$), with participants in the MediTrain group showing a significant decrease in RTVar from pre-treatment (mean = 58.2 ms, s.e. = 2.6 ms) to post-treatment (mean = 50.5 ms, s.e. = 2.8 ms; $\Delta = -7.98$ ms, two-tailed paired $t_{21} = -3.5$, $P = 0.002$, 95% CI: -12.7 to -3.1), whereas control participants showed no significant difference in RTVar from pre-treatment (mean = 60.7 ms, s.e. = 4.6 ms) to post-treatment (mean = 61.8 ms, s.e. = 4.2 ms; $\Delta = 1.1$ ms, two-tailed paired $t_{17} = 0.29$, $P = 0.78$, 95% CI: -7.0 to 9.1). The grey dashed line indicates a change score of zero, meaning no change from pre- to post-intervention. **c**, While RTs did not differ between groups after treatment, only participants in the MediTrain group were significantly faster at post-treatment than baseline. **d,e**, Histograms of RT distributions for MediTrain (**d**) and placebo (**e**) at pre-treatment (black) and post-treatment (gray). **f**, Change scores (post-treatment – pre-intervention) for RTVar for individual participants in the MediTrain and placebo groups. Shaded boxes represent s.e. (dark grey) and s.d. (lighter grey) of the mean, and whiskers show the maximum value within 2 s.d. of the mean. **g**, Scatter plot and best-fit line for the correlation between training slopes and change in RTVar on the vigilance task in participants in the MediTrain. Error bars represent s.e.m. * $P < 0.05$, ANCOVA; ** $P < 0.01$, two-tailed, paired t -test. TOVA, test of variables of attention.

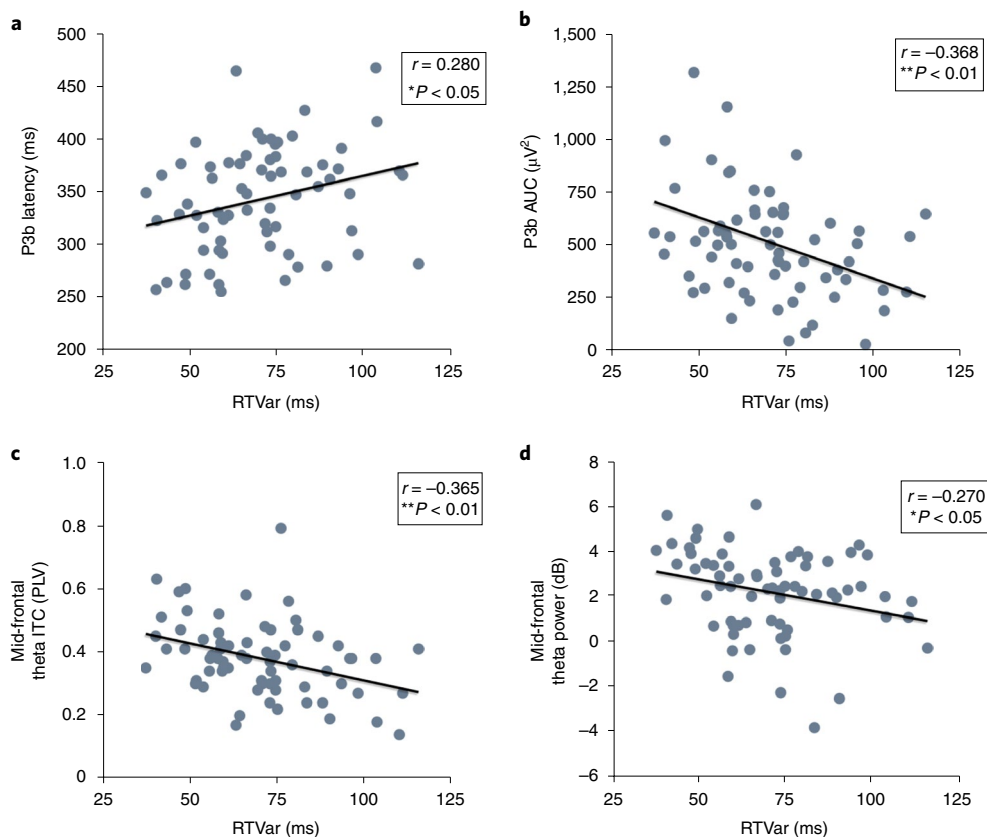


Fig. 3 | Correlations between RTVar and neural markers of attention for experiment 3. **a**, In an independent sample of participants ($n=69$) who completed a single EEG session, RTVar during vigilance task performance was significantly correlated with the latency of the P3b ERP at the parietal electrode Pz (Pearson $r_{67}=0.280$, $P=0.020$), such that participants with faster P3b latencies exhibited less-variable RTs. **b**, RTVar was also negatively correlated with the AUC for P3b at the parietal electrode Pz (Pearson $r_{67}=-0.368$, $P=0.002$), such that participants with greater P3b AUC values exhibited less-variable RTs. **c**, RTVar was also significantly correlated with frontal midline theta ITC from 200 to 300 ms after onset of infrequent target stimuli (Pearson $r_{67}=-0.365$, $P=0.002$), indicating that participants with greater frontal midline theta ITC values tend to have less-variable RTs. PLV, phase-locking value. **d**, We also found a correlation between RTVar and frontal midline theta power from 200 to 300 ms after onset of infrequent target stimuli (Pearson $r_{67}=-0.270$, $P=0.025$), indicating that participants with greater frontal midline theta power tend to have less-variable RTs.

data indicate that the decrease in RTVar in the MediTrain group was not associated with a trade-off in other performance metrics.

We next examined the relationship between the improvement of each participant on MediTrain breath focus itself over the 6 weeks of training and the change in their RTVar on the vigilance task after completion of the treatment period (Fig. 2g). We were able to extract complete training data sets from 20 participants in the MediTrain group (see Methods for details). We found a significant negative correlation (Pearson $r_{18}=-0.59$, $P=0.01$), indicating that the participants who advanced the most in the amount of time that they could maintain focus on their breath showed the greatest decrease in RTVar on the independent outcome measure (that is, improvement in sustained attention). These data support the conclusion that the mechanistic action driving the benefit in sustained attention is the improved ability to sustain focus on one's breath, fostered via the adaptive nature of MediTrain.

In addition to measuring cognitive performance, participants also underwent electroencephalography (EEG) recordings during the vigilance task, enabling us to examine the neural mechanisms underlying participants' increased ability to stabilize their attention (that is, reduce RTVar) following the MediTrain intervention. While vigilance tasks similar to ours (for example, other continuous performance tasks) have been frequently used to assess sustained attention and examine neural changes in older adults with cognitive

decline³², little is known about the neural correlates of the RTVar performance measure in healthy young adults. To constrain the number of neural markers examined, we sought to identify, a priori, key neural signatures that underlie differences in RTVar. To accomplish this goal, in experiment 3, we analysed EEG data from an independent sample of young adults ($n=73$) performing the same version of this vigilance task in other studies and assessed correlations with neural markers previously associated with attention performance (see Methods for details of experiment 3). We identified two relevant neural markers and two measurements for each them: parietal P3b event-related potential (ERP) latency (Fig. 3a) and area under the curve (AUC; Fig. 3b) and frontal midline theta (4–7 Hz) inter-trial coherence (ITC; Fig. 3c) and power (Fig. 3d) correlated significantly with RTVar on the vigilance task.

Having identified these neural markers of sustained attention, we then assessed whether they were differentially impacted by training with MediTrain compared to placebo. Complete EEG data sets for participants with complete pre-intervention and post-intervention measurements were obtained from 12 participants in each group (see Methods for details about missing data). Separate ANCOVAs of post-intervention measures revealed significantly greater levels of both mid-frontal theta ITC ($F_{1,21}=9.71$, $P=0.005$, Cohen's $d=1.27$, 95% CI: 0.33–0.42; Fig. 4a–c) and earlier parietal P3b latencies ($F_{1,21}=15.4$, $P=0.001$, Cohen's $d=1.02$, 95% CI: 327.5–353.0; Fig. 5a–c)

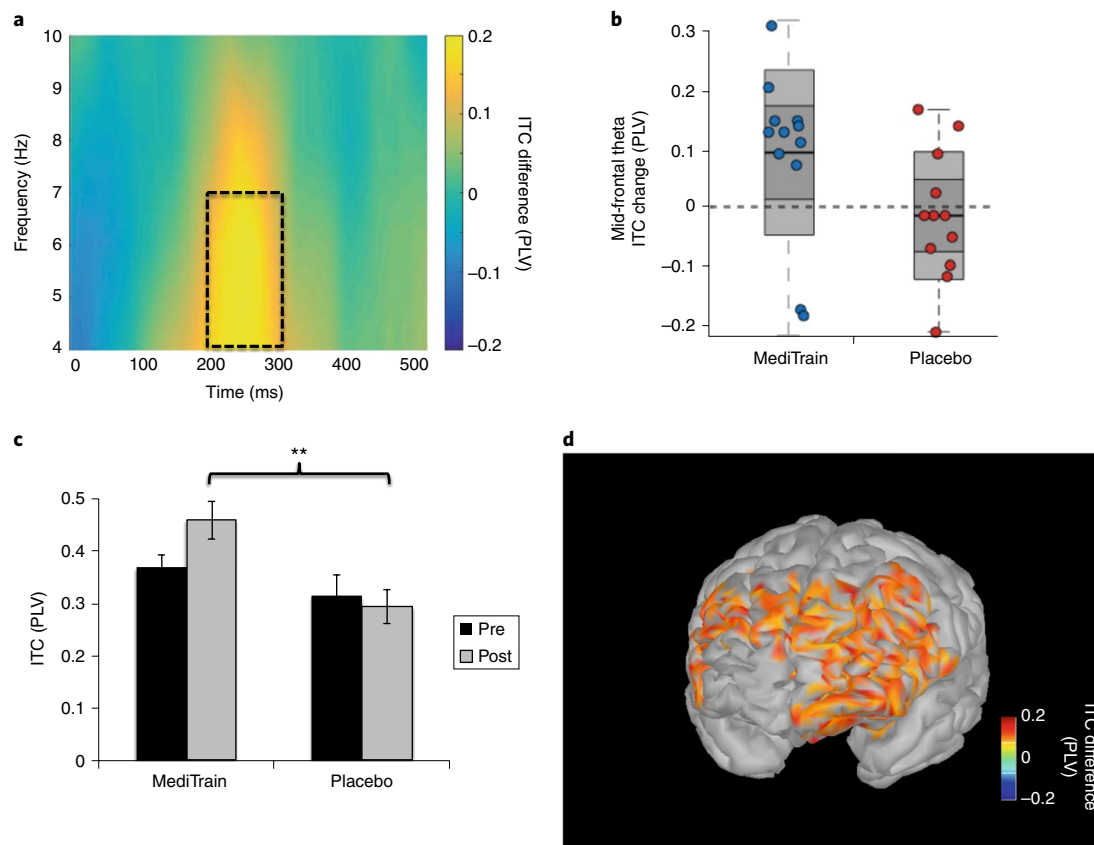


Fig. 4 | Changes in mid-frontal theta ITC. a, Time–frequency plot of the difference in theta-band ITC for MediTrain ($n=12$) versus placebo ($n=12$) groups at post-intervention while completing the vigilance task. **b**, Change scores (post-intervention – pre-intervention) for P3b latencies for individual participants in the MediTrain (blue circles) and placebo (red circles) groups. The shaded boxes represent s.e. (dark grey) and s.d. (lighter grey) of the mean, and whiskers show the maximum value within 2 s.d. of the mean; the grey dashed line indicates a change score of zero, meaning no change from pre- to post-intervention. **c**, ANCOVA of phase-locking values (PLVs; see Methods for details) revealed a significant difference in post-intervention theta-band ITC, corrected for pre-intervention levels (ANCOVA $F_{1,21}=9.71$, $P=0.005$, Cohen's $d=1.27$, 95% CI: 0.33–0.42); PLVs were computed for the time window depicted by the dotted rectangle in **a**. * $P<0.01$; error bars represent s.e.m. **d**, Theta-band ITC differences between the MediTrain group and placebo group at the post-intervention time point were source-localized to the medial and lateral prefrontal cortex.

in the MediTrain group than in placebo, while controlling for pre-intervention levels. Both of these group differences were driven by post-intervention improvements in the MediTrain group (see Figs. 4c and 5c). There was no significant effect of the intervention on overall frontal midline theta power at post-intervention (ANCOVA $F_{1,21}=0.01$, $P=0.92$, Cohen's $d=0.069$, 95% CI: 1.17–2.90). In addition, there was a trend towards a greater P3b AUC post-intervention in the MediTrain group than in placebo ($F_{1,21}=3.54$, $P=0.07$, Cohen's $d=0.66$, 95% CI: 467.27–676.36). An exploratory analysis revealed a significant correlation between change in P3b latency and change in RTVar in the MediTrain group (Pearson $r_{10}=0.568$, $P=0.05$; Supplementary Fig. 6).

To determine the anatomical substrate of these EEG markers and to link our findings to the broader literature, we performed source localization using individual neuroanatomy derived from magnetic resonance imaging (MRI) from all 12 participants from the MediTrain group who were included in the EEG analysis. Source analysis of frontal theta ITC (Fig. 4d) revealed that the peak signal was centralized to the medial prefrontal cortex (an area typically associated with the default mode network³³) and to the lateral prefrontal cortex (an area more commonly associated with frontoparietal attention networks⁶). Consistent with previous studies³⁴, we found that the P3b ERP component arose from a broadly distributed posterior temporoparietal network that included the precuneus (Fig. 5d), which is often considered a ‘hub’ within the default mode network³³.

These neural findings provide insights into the underlying mechanisms by which this closed-loop approach to focused-attention meditation with MediTrain leads to improvements in sustained attention, and are largely consistent with previous findings of neural changes following months of traditional, intensive meditation practice³¹. The frontal theta rhythm has been established as a marker of attentional control³⁵, and we have previously shown that frontal theta power changes in response to interventions that enhanced cognitive control^{7,36}. Studies have also shown that frontal midline theta power is inversely correlated with neural activity in the default mode network³³, where deactivation has been associated with increased task performance¹¹. Here, we show that the trial-by-trial consistency of this metric (that is, ITC) improves following MediTrain treatment, mirroring the greater trial-by-trial consistency found for task performance (that is, RTVar). Thus, our data indicate that enhanced stability of this network over time underlies the improved sustained attentional focus that we observed following MediTrain. While other research has pointed towards power in the frontal theta signal as being an important marker of cognitive control^{7,36}, our findings reveal that consistency in the phase of the theta oscillation from trial to trial, rather than simple activation of this top-down network, is critical for the ability to maintain attentional focus from moment to moment, and that this process is to strengthened by our meditation approach. The neural changes observed in the P3b are thought to reflect improved allocation of attentional resources. Recent evidence

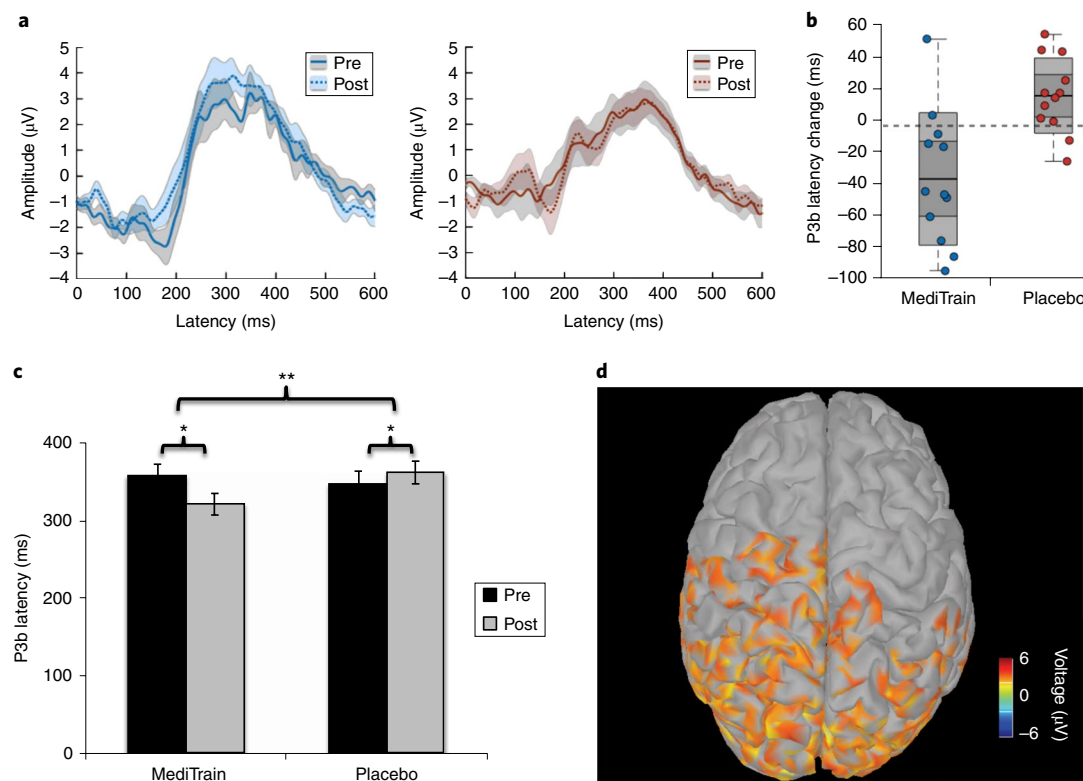


Fig. 5 | Changes in P3b latencies. **a**, ERP waveforms from the Pz electrode during the vigilance task for MediTrain (left) and placebo (right) at pre-intervention and post-intervention. **b**, Change scores (post-intervention – pre-intervention) for theta ITC for individual participants in the MediTrain and placebo groups. Shaded boxes represent s.e. (dark grey) and s.d. (lighter grey) of the mean, and whiskers show the maximum value within 2 s.d. of the mean. The grey dashed line indicates a change score of zero, meaning no change from pre- to post-intervention. **c**, ANCOVA revealed a significant difference between training groups in the post-intervention P3b peak latencies ($F_{1,21} = 15.4$, $P = 0.001$, Cohen's $d = 1.02$, 95% CI: 328–353). Post hoc analyses showed that participants in the MediTrain group exhibited significantly faster P3b peaks (two-tailed, paired $t_{11} = 3.083$, $P = 0.010$, Cohen's $d = 0.89$, 95% CI: 10.4–62.2) at post-intervention (mean = 319.8 ms, s.e. = 13.9 ms) than at pre-intervention (mean = 356.1 ms, s.e. = 15.2 ms), whereas participants in the placebo group had significantly slower P3b peaks (two-tailed, paired $t_{11} = -2.236$, $P = 0.047$, Cohen's $d = -0.65$, 95% CI: -29.71 to -0.24) at post-intervention (mean = 360.7 ms, s.e. = 14.7 ms) than at pre-intervention (mean = 345.7 ms, s.e. = 16.7 ms). **d**, Topographical distribution on P3b at peak latency (350 ms) collapsed across all participants at pre-intervention. The error bars and shading represent s.e.m. ** $P < 0.01$, ANCOVA; * $P < 0.05$, two-tailed, paired t -test.

suggests that a complex interplay between activity in the default mode network and the precuneus underlies attentional stability and flexibility, respectively³⁷. In addition, the default mode network has been shown to be more deactivated at rest in experienced meditation practitioners³⁸, and the P3b is also modulated by meditation training³⁹. In aggregate, the neural data indicate that both frontal and parietal networks drive these benefits on sustained attention.

In addition to our primary cognitive outcome task, we sought to test whether MediTrain led to more consistent performance on an attention task (Fig. 6a) that was more challenging than the vigilance task, that is, complex visual discrimination amid distractions (see Methods for task details). Confirming this hypothesis, an ANCOVA revealed that RTVar post-intervention was significantly lower in MediTrain ($n = 22$) than in placebo ($n = 19$; $F_{1,38} = 5.5$, $P = 0.024$, Cohen's $d = -0.73$, 95% CI: 0.2–0.3; Fig. 6b), with participants in the MediTrain group showing a significant reduction in RTVar ($\Delta = -91.1$ ms, paired-sample $t_{21} = 5.8$, $P < 0.001$, 95% CI: -0.12 to -0.06; Fig. 6c), whereas the placebo group did not change significantly ($\Delta = -9.1$ ms, paired-sample $t_{18} = 0.15$, $P = 0.88$, 95% CI: 0.079–0.069). We did not find a significant difference in overall RT ($F_{1,38} = 1.9$, $P = 0.18$, Cohen's $d = -0.31$, 95% CI: -0.02 to 0.09) or accuracy (d' : $F_{1,38} = 2.5$, $P = 0.12$, Cohen's $d = 0.37$, 95% CI: -0.08 to 0.7) at post-intervention, confirming that the decrease in RTVar was not achieved at a cost in performance. The finding that RTVar

was diminished in an identical manner on a second, more demanding, task with external distractions, provides supporting evidence that MediTrain has a generalizable positive effect on sustained selective attention.

Finally, we asked whether the benefits of MediTrain generalized to improvements in working memory, an important internally oriented form of attention⁴⁰. On a test of working memory capacity (Fig. 6d), we found significantly higher capacity at post-intervention in participants in the MediTrain group ($n = 20$) than in placebo controls ($n = 19$; see Methods for sample size details), while controlling for pre-intervention levels (ANCOVA: $F_{1,36} = 4.4$, $P = 0.04$, Cohen's $d = 0.66$, 95% CI: 0.006–0.35; Fig. 6e,f). Further analysis revealed that the MediTrain group showed increased working memory capacity from pre-intervention to post-intervention ($\Delta = 0.17$, paired-sample $t_{19} = 3.4$, $P = 0.003$, 95% CI: 0.067–0.28), whereas the placebo group did not change ($\Delta = -0.01$, paired-sample $t_{18} = -0.15$, $P = 0.89$, 95% CI: -0.16 to 0.14).

In conclusion, we found that MediTrain led to improvement in the ability of healthy young adults to stabilize their attention from moment to moment across two independent tasks. These results suggest a transfer of benefits from 6 weeks of engagement in a self-paced, internally directed attention practice (that is, MediTrain) to enhanced sustained attention while performing externally directed attention tasks that demanded rapid processing speed. Coupled

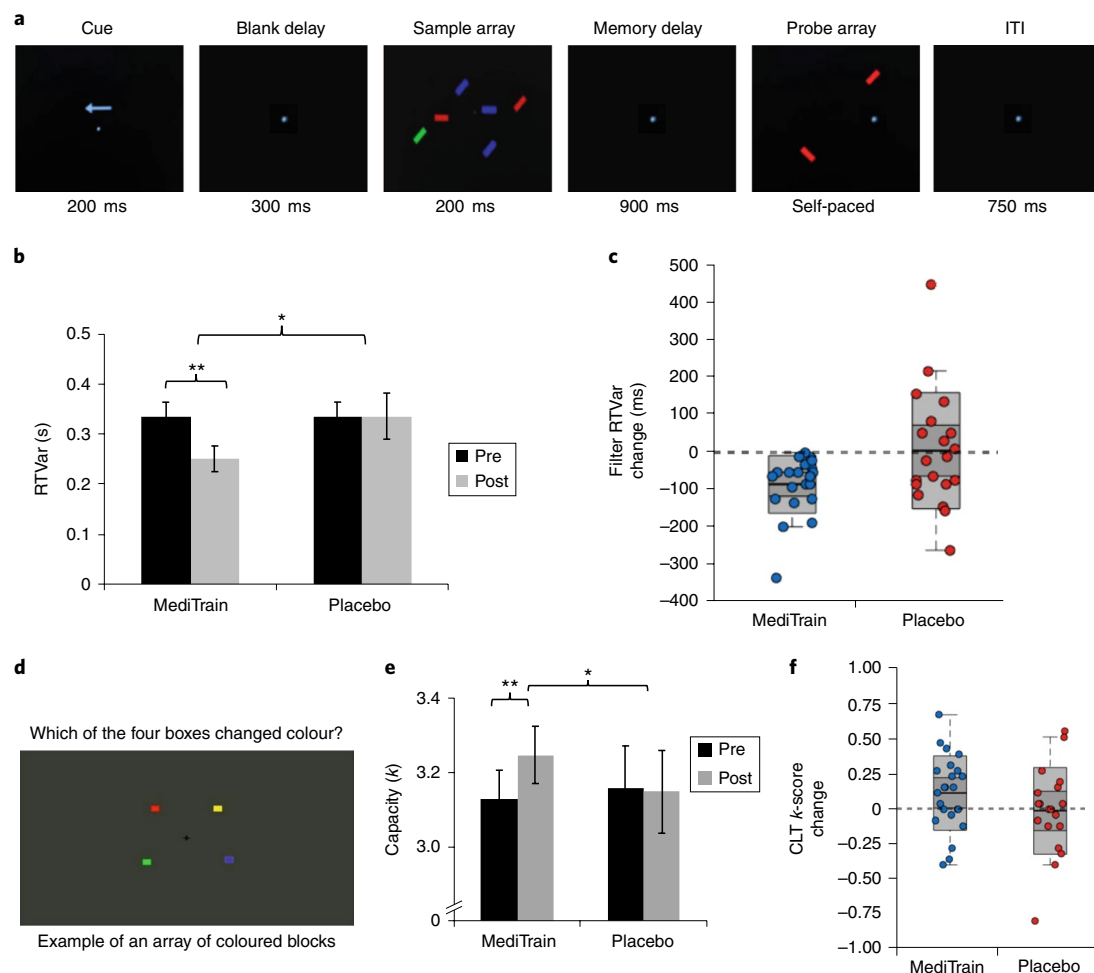


Fig. 6 | Improvements in visual discrimination and working memory. **a**, Visual discrimination and distractor filtering task stimuli and protocol. ITI, inter-trial interval. **b**, RTVar post-treatment was significantly lower in MediTrain than in placebo ($F_{1,38} = 5.5$, $P = 0.024$, Cohen's $d = -0.73$, 95% CI: 0.2–0.3), with participants in the MediTrain group showing a significant decrease in RTVar from pre-treatment (mean = 329 ms, s.e. = 3.1 ms) to post-treatment (mean = 248 ms, s.e. = 2.9 ms; $t_{21} = -5.7$, $P < 0.0001$, 95% CI: -0.12 to -0.06), whereas control participants showed no significant difference in RTVar from pre-treatment (mean = 336 ms, s.e. = 3.0 ms) to post-treatment (mean = 345 ms, s.e. = 5.1 ms; $t_{18} = -0.13$, $P = 0.9$, 95% CI: 0.079–0.069). **c**, Change scores (post-treatment – pre-treatment) for filter RTVar for individual participants in the MediTrain and placebo groups. Shaded boxes represent s.e. (dark grey) and s.d. (lighter grey) of the mean, and whiskers show the maximum value within 2 s.d. of the mean. **d**, Change localization task stimuli and protocol. **e**, ANCOVA showed a significant group difference in capacity (k -score: the number of items a participant is able to keep in mind during a delay) at post-training ($F_{1,36} = 4.4$, $P = 0.04$, Cohen's $d = 0.66$, 95% CI: 0.006–0.35), with participants in the MediTrain group showing a significant increase in k from pre-training (mean = 3.11, s.e. = 0.09) to post-training (mean = 3.3, s.e. = 0.08, paired-sample $t_{19} = 3.4$, $P = 0.003$, 95% CI: 0.067–0.28), whereas the placebo control group did not show a change in k -score from pre-training (mean = 3.16, s.e. = 0.11) to post-training (mean = 3.15, s.e. = 0.11, $\Delta = -0.02$, paired-sample $t_{18} = -0.15$, $P = 0.89$, 95% CI: -0.16 to 0.14). **f**, Change scores (post-training – pre-training) for change localization task (CLT) k -scores for individual participants in the MediTrain and placebo groups. The error bars represent the s.e.m. * $P < 0.05$, ANCOVA; ** $P < 0.01$, paired-samples t -test. In **c** and **f**, grey dashed lines indicate a change score of zero, meaning no change from pre- to post-intervention.

with the finding of increased working memory capacity, and further supported by neural data showing improved markers of attention and neural consistency, these findings offer converging evidence of improvements in attention in response to a closed-loop, digital meditation program. Critically, we observed these MediTrain-related cognitive and neural improvements in a population of young adults who face myriad modern-day challenges to their attention, further underscoring the timeliness and importance of this approach.

Limitations. While the observed improvements in sustained attention and working memory were present on average 1 week following the completion of the training period (that is, post-intervention cognitive testing visits were scheduled within 2 weeks after the last day of training), no conclusions can be made regarding the long-term

durability of the observed beneficial effects of MediTrain. In addition, future replication studies on larger and more diverse populations will be important to confirm the robustness and generalizability of the positive effects reported here.

Methods

Experiment 1: expectancy-matched placebo control selection. All methods were carried out in accordance with relevant guidelines and regulations approved by the UCSF Committee on Human Research's Institutional Review Board and all participants gave informed consent and received monetary compensation for their participation. We first conducted a study using Mechanical Turk⁴¹ to identify apps that were matched to MediTrain in terms of participants' expectations regarding potential beneficial outcomes of cognition or behaviour^{12,42}, but we did not hypothesize that they would improve attention²⁵. We used an empirical assessment of participants' expectations⁴³ of training-related gains in cognition to

find a combination of apps that would act as an appropriate placebo control for our MediTrain intervention. Expectancy matching here involved showing participants a recording of either MediTrain or the placebo apps, followed by a video describing each of the cognitive outcomes, and then asked them to rate the extent to which training with the displayed intervention would lead to improvements on each of the outcomes (see Methods for experiment 2 below for detailed descriptions of treatment programmes and cognitive outcomes). We used an iterative process of comparing 10 different apps to MediTrain in a sample of 261 participants. No statistical methods were used to pre-determine sample sizes, but we aimed for a target sample of 25 participants on each app. Each participant was randomly assigned to view a video of MediTrain or one of the 10 potential apps ($n=15\text{--}30$ per app). On the basis of this first round of testing, we selected three apps that had the highest potential for expectancy matching and ran a final comparison of this combination of apps against MediTrain. In our final comparison on a sample of 73 young adults, also recruited through Mechanical Turk (yielding a total $n=334$ for experiment 1), we found that these 3 apps ($n=37$) revealed no significant differences, compared to MediTrain ($n=36$), in participant expectations of improvement on our primary outcome of sustained attention (vigilance task, $t_{71}=0.39$, $P=0.7$, Cohen's $d=0.1$, 95% CI: -0.7 to 1.1) or any secondary outcome (distractor filtering, $t_{71}=0.45$, $P=0.65$, Cohen's $d=0.1$, 95% CI: -0.7 to 1.2 ; working memory capacity, $t_{71}=-0.22$, $P=0.83$, Cohen's $d=0.05$, 95% CI: -1.1 to 0.9 ; or working memory fidelity, $t_{71}=0.53$, $P=0.6$, Cohen's $d=0.1$, 95% CI: -0.7 to 1.3), indicating the identification of an appropriate placebo control condition. These apps included a foreign language learning app, a stretching app and a logic games app (see details below).

Experiment 2: randomized, placebo-controlled trial of MediTrain. *Participants and randomization.* We recruited 59 healthy young adults (18–35 years of age) who met our inclusion criteria for this longitudinal study. Participants were recruited from the local community using flyers posted at local colleges and universities, through online advertisements, and via word of mouth. All participants were screened for a history of neurological disease or current psychiatric illness and current use of psychotropic medications. We only enrolled participants who did not have a history of meditation experience, defined as currently practicing 1 or more days per week, having practiced on a weekly basis in the past, or having attended a meditation retreat of 3 or more days. These criteria for defining a sample as meditation naïve are consistent with previous studies⁴⁴ and were developed through conversations with expert colleagues familiar with such designs (S. Eisendrath, personal communication). We performed vision testing with a Snellen chart and acuity differences were corrected to 20/40 or better. All participants gave informed consent to participate in the study according to procedures approved by the Committee for Human Research at the University of California San Francisco and were compensated US\$15 per hour of time spent in the laboratory for cognitive testing or EEG recordings and for each hour spent training at home with a treatment programme, as well as a US\$50 'completion bonus' for completing all required training days and outcomes (for a possible total compensation of ~US\$600 for the entire study).

Participants were then randomized to either the MediTrain group or the placebo control group (see Supplementary Fig. 2 for the consort table). The study was designed in a double-blinded manner, such that all participants received the same instructions and laboratory personnel who were collecting data were unaware of group assignments. Double-blinding began at the point of recruitment, in which all participants were informed that they were being recruited for a study designed to test the efficacy of software interventions for improving cognition in various domains. As such, participants in both the MediTrain and the placebo groups had equal expectations that they were part of an active treatment group (see below for details on establishing matched expectancy of improvement across conditions). All staff that collected data were blind to group assignment; one study coordinator (S.S.) was informed of the treatment assignments to provide technical and other support during the training. Randomization resulted in 32 participants assigned to MediTrain and 27 participants assigned to placebo control. With two time points and assuming a moderate repeated measures correlation ($r=0.5$) and a two-tailed test ($\alpha=0.05$), we calculated that $n=30$ per group would yield 75%^{45,46} to detect a change with a medium effect size (0.5)⁴⁷. Over the course of the study, 8 participants in the MediTrain group and 7 controls voluntarily withdrew from the study for various personal reasons (but not due to adverse events), resulting in complete pre-training and post-training data sets from 24 participants in the MediTrain group (13 female) and 20 placebo controls (12 female).

We excluded the data of two participants in the MediTrain group from all analyses due to obvious irregularities in their software usage. Given the likelihood that these participants were not accurately engaging in the MediTrain treatment, we excluded them from all further analyses, resulting in a final as-treated sample of $n=22$ in the MediTrain group. We were also unable to recover complete week-by-week training data from two participants in the MediTrain group due to malfunctions with the iPads (but we were able to confirm in the app that both had completed the required number of sessions); data from these participants were not included in the training curve correlations (Figs. 1 and 2g). Two placebo control participants were excluded from analysis of the vigilance task behavioural data because they were extreme outliers (that is, >2.5 s.d. above or below the mean),

resulting in a final as-treated sample of $n=22$ in the MediTrain group and $n=18$ in the placebo group for that task. For the filter task, one control participant did not complete the task at post-training due to a software malfunction, resulting in a total sample of $n=22$ in the MediTrain group and $n=19$ in the placebo group. For the change localization task, one control participant and two participants in the MediTrain group did not complete the task at post-training due to a software malfunction, resulting in a total sample of $n=20$ in the MediTrain group and $n=19$ in the placebo group.

Treatment programme. All participants completed the intervention at home using an iPad Mini 2 (iOS version 8.2; Apple) that was supplied by the UCSF Neuroscape Center. Each software program included self-contained instructional videos and practice modules. Participants were provided access to a website containing instruction reminders, a calendar and e-mail support throughout the treatment period. Each tablet was configured to transmit data automatically to our secure Neuroscape server wirelessly as each session was completed, allowing us to monitor compliance and data integrity in real time. Throughout the treatment period, technical support for the iPads and all software was provided via e-mail, phone and in-person contact, when needed.

Meditation group. The MediTrain program was designed as an integration of meditation-based practices and approaches from plasticity-based, attention-training methods, including quantifiable goals, feedback and adaptivity. Participants were instructed to engage in the training in a quiet location, free of external auditory distractions, with headphones on and eyes closed, and to attend to the sensations of their breath. Before the initial day of treatment, the program required participants to listen to and/or read detailed instructions about how to engage in the treatment and use the iPad (approximately 15 min). Following these technical instructions, participants listened to and/or read a short 'lesson' about mindful breathing practices (see Supplementary Information), written and narrated by J. Kornfield, an expert teacher of meditation and mindfulness. While focusing on their breath, they were asked to monitor the quality of their attention and to be particularly aware of any internal distracting thoughts that may arise. When these thoughts did occur, participants were instructed to acknowledge the distraction, disengage from it and shift their attention back to their breathing.

For the treatment, the length of the initial trial was set at 20 s, based on feedback and results from pilot testing. At the end of each trial, participants were asked to report, via button-press, whether their attention remained on their breathing throughout the trial, or whether their attention was diverted, even once, by distracting thoughts (that is, mind wandering). If they successfully attended to their breathing without distraction for the entire trial, the duration of the next trial was increased by 10%; if unsuccessful, the duration of the next trial was decreased by 20%. By adaptively modifying the duration of the trials based on this criterion, we thresholded each participants' ability to self-regulate internal attention. Training sessions were linked, such that the next session begins at the level attained at the end of the previous session, and participants started at the same level at which they ended on subsequent training days. Participants were provided two types of feedback: (1) real-time feedback, indicating whether the participant successfully detected or classified the target, and (2) punctuated feedback, in which participants advance through a series of 'levels' that are reported at the beginning and end of each run.

MediTrain training curves and slopes. To correlate MediTrain app performance improvements with cognitive outcome improvement, we calculated training slopes. Training curves for each participant in the MediTrain group are shown in Fig. 1, with the final meditation time achieved during the final session of each week plotted for all 6 weeks. We were unable to obtain full weekly data for two participants due to device malfunctions. As an index of how participants improved on MediTrain, we calculated the improvement slope as the final session meditation time minus the meditation time achieved at the end of their first day of training divided by 30 (the total number of sessions). We then performed correlations between these slopes and RTVar on our primary outcome measure, the vigilance task (Fig. 2g).

Placebo control apps. As described above, in experiment 1, we identified a set of three commercially available iOS apps that were matched to our MediTrain program in terms of expectation of improvement on our cognitive outcome measures. Those apps were a foreign language learning app (Duolingo; <https://www.duolingo.com>), a Tai Chi app (Tha Chi Step by Step; <http://www.imoblif.net>) and a logic games app (100 Logic Games; <https://www.andreasabbatini.com/LogicGames.aspx>). For Duolingo, participants were given a choice of which foreign language they wanted to learn from those available on the app store. Within the app, we set a 10-min training time per day. During training, the app takes users through a series of modules that increase in difficulty and are only unlocked sequentially following completion of an earlier module. Modules are organized topically (for example, food, animals, phrases, and so on) and each module contains listening, speaking, vocabulary and translation tasks and culminates with a topic quiz. At the end of each lesson, the app provides a progress report showing learning 'streaks' and the accumulation of 'lingots' (Duolingo currency).

These feedback features are meant to keep participants motivated. For Tai Chi, users simply open the app and select from a series of modules that provide detailed and easy-to-follow instructions on how to perform many basic, intermediate and advanced Tai Chi movements and is geared towards beginners with no Tai Chi experience. Each description can be read or listened to and is accompanied by an animation. Users are instructed to then practice the exercise themselves several times after each lesson. The logic games app comprises a series of 'puzzle sets' that revolve around a particular theme and which get progressively more difficult as people advance. The puzzles are similar to the more well-known Sudoku puzzles, but provide a more engaging experience with colourful icons, unique rule sets for each theme and increasing difficulty. For each puzzle, users are given a task (for example, plant trees according to specific rules), a time limit and a number of hints that they can unlock.

Because these apps were commercial apps and not designed to send data to our server, we took additional steps to track compliance in the placebo control group. We wrote two custom survey apps and installed them on each placebo iPad along with the placebo apps themselves. Participants were instructed to open the check-in app, complete a couple of questions about their alertness and nature of their training environment. When they submitted the survey, it automatically sent the results, with a timestamp, to our server and it also started a 30-min timer on the iPad. The timer was to help the participants keep track of their total training time. They were instructed to try to split their time equally among the three apps. When the timer went off, the participants then opened the check-out app and completed another set of questions pertaining to their impression of how they felt about their training that day; the result and a timestamp were then sent to us, allowing us to monitor the start and end training times for every session each participant completed. Participants were instructed to spend approximately 10 min with each app each training day (5 d per week for 6 weeks). For Duolingo, the time was set internally in the app. For the other two apps, participants self-timed their training, but with the overall amount of time monitored by the timer app.

Cognitive outcome measures. We pre-selected one primary cognitive outcome measure (sustained attention), two secondary cognitive outcome measures (distraction filtering and working memory capacity) and an exploratory measure (working memory fidelity; see Supplementary Information for results) to quantify the extent to which each training task exhibited generalization, or transfer, of benefits. All participants were brought back into the laboratory for cognitive testing and neural recordings 1–2 weeks following the completion of their training regime.

Primary behavioural outcome: sustained attention. Our primary outcome measure was a modified version of a well-validated vigilance task (Fig. 2a), the Test of Variables of Attention (TOVA)⁴⁸, which provides an index of sustained attention. We have used this task as an outcome measure in previous intervention studies from Neuroscape^{7,36}. The experiment was programmed in Presentation (<https://neurobs.com>) and the stimuli were presented on a cathode ray tube (CRT) monitor. For the present study, we adapted the task for use with EEG recordings. In this task, participants maintain fixation on a central crosshairs and grey squares are shown on a black background at the top or bottom of the field of view. To test sustained attention, stimuli are presented frequently at the top of the screen and participants are instructed to only respond to the infrequently occurring square in the bottom of the screen. The target-to-non-target ratio is 1/4 (1 target for every 4 non-targets), thus requiring participants to sustain their attention over a long period. Participants completed 2 blocks of 125 trials with 25 targets per block, yielding 50 total targets.

Secondary behavioural outcome: complex visual discrimination amid distractions. We used the filter task⁴⁹ (Fig. 6a) to assess another aspect of sustained attention: complex visual discrimination amid distractions. The version that we used was modified to evaluate how well participants were able to ignore task-irrelevant information^{50,51}. The experiment was programmed in MATLAB's psychophysics toolbox (<http://psychtoolbox.org/>) and the stimuli were presented on a CRT monitor. In this task, participants must attend to an array of different numbers of items (either one or three red rectangles) with or without the presence of two visual distractors (two blue or green rectangles): set size one no distractors (one total item), set size one with distractors (three total items), set size three no distractors (three total items), set size three with distractors (five total items). Half of each of the trials for each condition began with a cue indicating that the participant should attend to either the left or the right side of the screen. The procedure for each trial began with a 750-ms fixation cross followed by a right or left cue (200 ms) and then a 300-ms blank inter-stimulus interval. Next, a sample set from one of the four conditions was shown for 200 ms followed by a 900-ms blank delay and then a probe set containing the same number of red rectangles as in the sample in either the same orientation or with a single rectangle of altered orientation (50% of each). The probe screen remained visible until participants responded with a 'Yes' or 'No' button press, indicating whether one of the attended rectangles changed orientation. Participants completed 8 blocks of 80 trials, yielding 160 trials per condition. Participants were instructed to respond as fast as possible without sacrificing accuracy.

Secondary behavioural outcome: working memory capacity. To assess working memory capacity, all participants completed 60 trials of the change localization task⁵² (Fig. 6d). The experiment was programmed in E-Prime (<https://pstnet.com/products/e-prime/>) and the stimuli were presented on a CRT monitor. The stimuli were coloured squares ($0.7^\circ \times 0.7^\circ$) that were presented on a grey background. On each trial, the colour of each of the four squares was selected randomly without replacement from six possible colours. Each square was presented at a random location within an imaginary circle with a 3° radius. On each trial, participants were first presented with a fixation cross for 500 ms, followed by an array of four coloured squares, which remained visible for 100 ms and was followed by a 900-ms delay/retention interval where only the fixation cross was visible. The delay was followed by a test array of four squares in which the colour of three squares was unchanged and the colour of one square was different. Participants were instructed to select the square that did change colour. A mouse was used to make the selection, and thus, we were not able to reliably measure RTs in this experiment. This response was self-paced and trials were separated by a 2,000-ms inter-trial interval. A 'k-score' was then calculated for each participant ($k = \% \text{ correct} \times \text{the number of items in the memory array}$), providing an index of their overall working memory capacity before and after training.

Exploratory behavioural outcomes: working memory fidelity. We used a delayed recognition paradigm designed to measure changes in participants' ability to maintain an accurate mental representation of items in working memory either in the presence or the absence of distracting or interfering information. We have used versions of this task in numerous previous studies^{53,54}, including one other cognitive training study⁷ in which the methods are described in detail. To summarize briefly, this paradigm consisted of four different conditions that were presented in blocks: (1) no distraction, (2) ignore distractor (the distractor was present, but participants were informed that the distractor was to be ignored), (3) attend distractor (participants were required to make a judgement about the interfering stimulus), and (4) a passive view control condition that did not have a memory component and participants simply viewed face or scene stimuli, after which they were instructed to respond as quickly as possible to a left or right arrow with a left or right button press. Each run was preceded by an instruction slide informing the participant which condition they would be performing. Each trial began with the presentation of a face displayed for 800 ms, followed by a delay period (3 s), the presentation of a face stimulus as a distractor in the ignore distractor and attend distractor conditions (800 ms), a second delay period (3 s) and the presentation of a face probe (1 s). The participants were instructed to make a match/non-match button-press response at the probe as quickly as possible, without sacrificing accuracy. This was followed by a self-paced inter-trial interval. The experiment was programmed in E-Prime (<https://pstnet.com/products/e-prime/>) and the stimuli were presented on a CRT monitor. The results from this outcome are presented in Supplementary Information.

Post-intervention survey of training expectancy. In addition to using the results from experiment 1 to guide our choice of placebo control, we also sought to confirm that expectations of improvements were matched in our actual training sample. Thus, immediately following the completion of all experimental tasks on the participants' post-training laboratory session, we ended by collecting survey data about the participants' expectations that their assigned intervention would improve their performance on each of the outcome tasks (the procedure was the same as experiment 1, but simply performed after the end of the post-training experimental session). We did not find a significant difference in the expectation of improving on any of our four outcome measures (see Supplementary Table 1 for means and statistics). These data confirmed that there was no significant difference between the MediTrain and placebo control groups in their expectations that they would improve on each cognitive task.

EEG recordings and analyses. We collected EEG data while participants performed the vigilance task before and after the 6-week treatment programmes. We were unable to collect EEG data from three participants at pre-training due to equipment malfunctions, and due to technical issues with photodiodes, we were unable to time-lock the EEG to the event onsets for several other participants ($n = 4$ at pre-training, $n = 4$ at post-training). Furthermore, we excluded data sets in which excessive noise led to 30% of target trials being rejected, with the rejection criteria being anything greater than a $\pm 100\text{-}\mu\text{V}$ voltage deflection within an epoch⁵⁵ ($n = 4$ at pre-training, $n = 3$ at post-training). In the MediTrain group, there were ultimately 14 participants with usable EEG data at pre-intervention and 21 participants at post-intervention. In the placebo group, there were 16 participants with usable data at pre-intervention and 15 participants at post-intervention. Owing to the longitudinal design of the experiment, the ANCOVA could only be performed on participants who had usable data at both pre-intervention and post-intervention. Thus, the MediTrain group had 12 participants that were included in the analysis and the placebo group had 12 participants. To ensure that this sub-sample did not differ significantly from the full cohort, we performed an ANCOVA of post-intervention RTVar for the vigilance task and found that participants with and without EEG data did not differ significantly ($F_{1,36} = 0.021$, $P = 0.88$, Cohen's $d = 0.013$, 95% CI: 66.7–83.4). Furthermore, to ensure that this subset was representative of the larger samples,

we performed additional ANCOVAs of RTVar for the vigilance task in these participants and found comparable effects: compared to placebo, MediTrain showed lower RTVar at post-intervention, when covarying for pre-training levels ($F_{1,21} = 4.9$, $P = 0.037$, Cohen's $d = 0.76$, 95% CI: 65.5–86.5). Post-hoc within-group t -tests showed that this effect is driven by a decrease within MediTrain (paired-sample $t_{11} = 2.47$, $P = 0.031$, Cohen's $d = 0.71$, 95% CI: 1.3–22.8; pre-mean = 69.8 ms, s.e. = 4.2 ms; post-mean = 57.7 ms, s.e. = 5.0 ms), whereas placebo did not change (paired-sample $t_{11} = -0.86$, $P = 0.411$, Cohen's $d = -0.25$, 95% CI: -27.4 to 12.1; pre-mean = 86.6 ms, s.e. = 9.6 ms; post-mean = 94.2 ms, s.e. = 11.7 ms).

All EEG data were recorded and analysed using identical methods to those used in experiment 3 (see below). On the basis of the results from experiment 3, we tested for differences at post-training, while controlling for baseline values using an ANCOVA, for the following measures:

- (1) Frontal midline theta analysis: the results from experiment 3 showed that frontal midline theta (4–7 Hz) ITC from 200 to 300 ms after the onset of infrequent target stimuli was significantly correlated with RTVar in the vigilance task (Fig. 3c). In addition, meditation engagement has been shown to increase theta-band ITC in frontal midline electrode sites during a sustained attention task, and this change was accompanied by a change in RTVar³¹. There is a rich literature associating frontal midline theta power with attentional control abilities³⁵, and we have previously shown that this measure is sensitive to change in response to cognitive interventions^{3,36}. Thus, we investigated whether the average theta-band ITC and power from 200 to 300 ms post-stimulus onset in a cluster of frontal midline electrodes (FCz, Fz, FPz, AF3, AF4 and AFz) were modulated as a result of the intervention in experiment.
- (2) P3b ERP analysis: the P3b is hypothesized to reflect the allocation of attentional resources³⁶. Results from experiment 3 demonstrated that the P3b AUC and latency were significantly correlated with RTVar (Fig. 3a,b). In addition, previous studies have demonstrated that the P3b can be modulated by meditation training³⁹. Thus, we examined whether the speed and quantity of attentional resources deployed following target stimulus detection, as indexed by the P3b latency and AUC, respectively, were modulated by the MediTrain intervention. P3b AUC was calculated at the Pz electrode from a time window of 250–500 ms after stimulus onset³⁷, and P3b latency was calculated as the time point within this window when maximum voltage was reached. We also conducted a post-hoc analysis to determine the test–retest reliability of the P3b latencies and found the intraclass correlations to be high ($r_{22} = 0.7$, $P < 0.01$; Supplementary Fig. 5).

EEG source localization. For the purposes of source localization, we collected a high-resolution T1-MPRAGE structural MRI scan from each participant for whom we had a complete EEG data set ($n = 12$ per group). All MRIs were obtained on a Siemens 3T Magnetom Trio equipped with a 12-channel matrix head coil using the following sequence parameters: voxel size = 1.0 mm isotropic, repetition time = 2,300 ms, echo time = 2.98 ms, inversion time = 900 ms and flip angle = 9°. T1-weighted MRI data were processed using the FreeSurfer (<https://surfer.nmr.mgh.harvard.edu>) morphometric analysis tools. Cortical surfaces were reconstructed using a semi-automated procedure that has been described at length in previous work^{38–40}.

We source localized each participant's EEG data to their FreeSurfer cortical surface to visualize where the signals of interest are arising from. To achieve this, we used the openMEEG software⁶¹, implemented through the Brainstorm⁶² MATLAB toolbox, to perform boundary element method forward modelling. Once forward models were constructed, we used sLORETA to estimate the sources of EEG activity recorded at the scalp. Source estimations of the P3b were projected onto a 15,000 vertex cortical surface reconstruction. We computed the grand average of the source projected ERP collapsed across both groups at the pre-intervention assessment, and visualized the cortical distribution of the P3b ERP component at the latency at which the maximum amplitude occurs (350 ms). Before computing the source estimation of the frontal midline theta ITC, we downsampled the cortical surface reconstruction to 2,500 vertices to reduce the computational burden during subsequent processing steps without significantly altering the spatial resolution of the data. The ITC time series was computed by resolving 4–40-Hz activity using EEGLAB's fast Fourier transform on the source-projected EEG. Using this method, we were able to obtain the ITC time series at each cortical vertex in the source-reconstructed EEG data. We then subtracted the mean ITC at each time point in the placebo group from the mean ITC in the MediTrain group and visualized this contrast at the latency at which the group contrast is greatest (260 ms). Thus, using this technique, we were able to visualize the cortical distribution of where there is an intervention-related effect on ITC. We did not use this same procedure when visualizing the P3b, because the intervention-related effects on the P3b amplitude were more diffuse than for frontal midline theta ITC. Thus, we only visualized the source-projected topography of the P3b to demonstrate its parietal origins, rather than visualizing intervention-related effects on this component.

Intervention protocols. After completing their in-laboratory cognitive and EEG testing sessions, all participants were supplied with an iPad with their assigned

training regime pre-loaded. An experimenter who was not involved in data collection provided detailed instructions for training; these instructions were also accessible to the participants at any time via a customized website, and a study coordinator was available by e-mail or phone to answer questions and troubleshoot the technology throughout the study. Both groups engaged with their apps for 6 weeks, starting with 20 min per day in weeks 1 and 2, 25 min per day in weeks 3 and 4, and ending with 30 min per day in weeks 5 and 6. This gradual increase in engagement time was meant to mimic the progression in a more traditional meditation training. Because the placebo group engaged in off-the-shelf apps that did not send data to our servers, we required the placebo control participants to submit a 'check-in' and 'check-out' survey directly from their iPads. The purpose of the check-in and check-out was to have a timestamp of when the participants began and ended their training to ensure compliance. After 6 weeks of training, participants returned to the laboratory for another cognitive testing session that was identical to the pre-treatment visit.

Statistical methods. All initial analyses were conducted by researchers blind to group membership. All statistical tests were two-tailed, and each behavioural and neural measure met the assumptions of normality for parametric statistics (testing using the Shapiro–Wilk test). To test for training effects on our cognitive outcomes, we used an ANCOVA approach, in which the dependent variable is the post-training performance, with group (MediTrain versus placebo) as the primary independent variable and the pre-training performance on the same task included in the analysis as a continuous covariate. This approach is considered to be preferable to analysis of gain scores or to repeated measures or mixed model analyses of variance (ANOVAs) when post-test performance that is not conditional on pre-test performance is the primary outcome in a test of a treatment or intervention^{63,64}. This is an approach we have used in previous intervention studies^{7,36}. Given that our population was healthy young adults who tend to perform at near-ceiling levels on many cognitive tasks and because our primary hypothesis was that MediTrain would improve sustained attention abilities, we focused on RTVar as our primary metric, with additional secondary analyses of RT and a discrimination index (d'). All data met the assumptions required for parametric statistics and were screened for outliers. For post-hoc analysis of the within-group changes following a significant main effect in each ANCOVA, we performed two-tailed, paired-sample t -tests on each group separately to test for significant differences from pre-training to post-training. Because these tests were performed separately on independent outcome variables, and only one test for each group (MediTrain and placebo), we did not perform a statistical correction for multiple comparisons. To derive Cohen's d effect-size scores that reflected the significant group by time interactions, we computed change scores for RTVar from pre-treatment to post-treatment on all measures of interest. We also used these change scores to test for outliers, defined as falling more than 3 s.d. beyond the group mean. On the vigilance task, change scores for RTVar revealed two participants who qualified as outliers, with change scores greater than 4 s.d. for the rest of the group. As a result, we excluded two control participants from the vigilance task analyses, in addition to the two participants in the MediTrain group who were excluded from all analyses based on analysis of training slopes, described above. To test whether there was a relationship between the training curves and changes in RTVar, we computed a slope for each participant in the MediTrain group (final meditation duration – initial meditation duration/6 weeks of training) and calculated Pearson correlations between the slopes and change in RTVar.

We used the same ANCOVA approach to test for training effects on our EEG outcome measures, with post-intervention metrics set as dependent variables, and group (MediTrain versus placebo) as the primary independent variable, with pre-intervention metrics set as a continuous covariate. All of these data were screened for parametric assumptions, and any outliers that were excluded from the behavioural analysis above were also excluded from the EEG outcomes analyses. Group-wise post-hoc tests were performed in a manner analogous to that used for behavioural data (described above).

Experiment 3: selecting EEG measures for analysis. To generate an a priori hypothesis of which neural measures we would expect to change with treatment (experiment 2), we analysed EEG data from 73 healthy younger adults (19–32 years of age) while performing the vigilance task. These data were collected as part of other experiments conducted at Neuroscape, and thus constitute an independent sample. No statistical methods were used to pre-determine sample sizes. All data were processed using the same parameters as the data from experiment 3. Traditional ERP and spectral markers of attentional control (that is, P1/N1, P3b, frontal midline theta power and ITC, and posterior alpha power)^{7,34,35,37,65,66} were extracted from the data and correlated with RTVar on the vigilance task. We found two neural markers, and two measurements from each of these were significantly correlated with RTV: (1) frontal midline theta ITC and power, and (2) P3b latency and AUC. Thus, these neural measures were analysed for intervention-related change in the MediTrain study (see experiment 2 above).

EEG recordings. Neurophysiological data were recorded during cognitive outcome testing using an active two head cap (Cortech Solutions) with a BioSemiActiveTwo 64-channel EEG acquisition system in conjunction with BioSemiActiView software

(Cortech Solutions). Signals were amplified and digitized at 1,024 Hz with a 16-bit resolution. Anti-aliasing filters were used and data were band-pass filtered between 0.01 and 100 Hz during data acquisition. For each EEG recording session, a 1 × 1-inch white box was flashed for 10 ms at one of the corners on the stimulus presentation monitor at the start of each trial. A photodiode (<https://www.gtec.at/Products/Hardware-and-Accessories/g.TRIGbox-Specs-Features>) captured this change in luminance to facilitate precise time-locking of the neural activity associated with each sign event. During the experiment, these corners were covered with tape to prevent participants from being distracted by the flashing light.

EEG preprocessing. Preprocessing was conducted using the EEGLAB software⁶⁷. Noisy channels were identified on initial visual inspection, were removed from the data and interpolated using a spherical spline interpolation, using the average signal of the surrounding channels to reconstruct the data in the removed channel. The data were then downsampled to 1,024 Hz to reduce the computational demand without losing any important information in the data. A finite impulse response filter with a high-pass cut-off of 1 Hz was applied to remove drift, and then a low-pass filter at 40 Hz was applied to remove high-frequency noise. Ocular correction was performed by using independent component analysis (ICA) to isolate and remove activity induced by eye blinks and lateral eye movements from the signal. The data were then re-referenced to the average signal of all channels. Epochs of −1,000 ms to +1,000 ms were generated for each stimulus type for subsequent analyses. Epochs containing excessive peak-to-peak deflections ($\pm 100 \mu\text{V}$) were removed.

Frontal midline theta analysis. Frontal midline ITC and power have both been implicated in sustained attention abilities^{31,35,65}, including correlating with RTVar across the lifespan⁶⁶. ITC is a measure that reflects the extent to which synchronization occurs from trial to trial in EEG at a particular frequency and latency⁶⁸. In other words, it is a measure of electrophysiological response consistency. ITC is quantified by the unit ‘phase-locking value’, which ranges anywhere between 0 and 1, with a value of 0 indicating that the phase synchrony is completely random, and a value of 1 indicating that the phase locking is perfectly synchronized across trials. The spectral power and ITC time series were created by resolving 4–40-Hz activity using a fast Fourier transform in EEGLAB. We found that frontal midline theta power and ITC (4–7 Hz) from 200 to 300 ms after the onset of infrequent target stimuli are both significantly correlated with RTVar in the vigilance task (Fig. 3a,b). We selected a cluster of frontal electrodes (FCz, Fz, FPz, AF3, AF4 and AFz) based on previous literature that has used this same electrode cluster for frontal midline theta analyses⁶⁹, and selected the 200–300-ms time window based on when frontal midline theta reaches its peak power (247 ms). Thus, the 200–300-ms time window captures peak power and with approximately ± 50 ms on both ends. We did not use a larger time window to avoid potentially introducing motor-related activity into the signal of interest, as average RTs occur at 358 ms.

ERP analysis. ERP time-locked to target trials were generated from the preprocessed EEG data recorded from participants while they performed the vigilance task. ERPs were baseline corrected to the average voltage 200 ms preceding the stimulus onset. Before artefact rejection, there were a total of 50 trials that contained the target stimulus. The median number of trials that met the criteria for being excluded from further analysis was one, with no participant having more than seven trials that contained supra-threshold artefacts.

We found that the AUC of the P3b ERP component elicited by rare target stimuli is highly correlated with RTVar during vigilance task performance (Fig. 3c). Infrequent target stimuli during visual sustained attention tasks have been shown to reliably evoke strong P3b ERP components⁶⁵, which is hypothesized to reflect allocation of attention resources, and have been shown to be modulated by meditation training³⁹. Thus, we focused our ERP analysis on the P3b component in the Pz electrode, which is the location that the P3b is commonly reported to reach its maximum amplitude⁶⁹. We computed the AUC and the peak latency from a time window of 250–500 ms after stimulus onset, which is a time window commonly used to define the P3b⁶⁵. The P3b peak latency was calculated at the time point at which maximum voltage was reached within this 250-ms second window.

Reporting Summary. Further information on research design is available in the Nature Research Reporting Summary linked to this article.

Data availability

The data that support the findings of this study are available from the corresponding authors on reasonable request.

Code availability

The code used in the analysis of EEG data reported in this paper is available from the corresponding authors on reasonable request.

Received: 20 February 2019; Accepted: 16 April 2019;
Published online: 3 June 2019

References

- Rideout, V. J., Foehr, U. G. & Roberts, D. F. *Generation M2: Media in the Lives of 8- to 18-Year-Olds* (Henry J. Kaiser Family Foundation, 2010).
- Moisala, M. et al. Media multitasking is associated with distractibility and increased prefrontal activity in adolescents and young adults. *Neuroimage* **134**, 113–121 (2016).
- Ophir, E., Nass, C. & Wagner, A. D. Cognitive control in media multitaskers. *Proc. Natl Acad. Sci. USA* **106**, 15583–15587 (2009).
- Ralph, B. C., Thomson, D. R., Cheyne, J. A. & Smilek, D. Media multitasking and failures of attention in everyday life. *Psychol. Res.* **78**, 661–669 (2014).
- Chun, M. M., Golomb, J. D. & Turk-Browne, N. B. A taxonomy of external and internal attention. *Annu. Rev. Psychol.* **62**, 73–101 (2011).
- Posner, M. I. & Petersen, S. E. The attention system of the human brain. *Annu. Rev. Neurosci.* **13**, 25–42 (1990).
- Anguera, J. A. et al. Video game training enhances cognitive control in older adults. *Nature* **501**, 97–101 (2013).
- Cortese, S. et al. Cognitive training for attention-deficit/hyperactivity disorder: meta-analysis of clinical and neuropsychological outcomes from randomized controlled trials. *J. Am. Acad. Child Adolesc. Psychiatry* **54**, 164–174 (2015).
- Tang, Y. Y. & Posner, M. I. Attention training and attention state training. *Trends Cogn. Sci.* **13**, 222–227 (2009).
- Bediou, B. et al. Meta-analysis of action video game impact on perceptual, attentional, and cognitive skills. *Psychol. Bull.* **144**, 77–110 (2018).
- Brefczynski-Lewis, J. A., Lutz, A., Schaefer, H. S., Levinson, D. B. & Davidson, R. J. Neural correlates of attentional expertise in long-term meditation practitioners. *Proc. Natl Acad. Sci. USA* **104**, 11483–11488 (2007).
- Simons, D. J. et al. Do “brain-training” programs work? *Psychol. Sci. Public Interest* **17**, 103–186 (2016).
- Weyandt, L. L. et al. Prescription stimulant medication misuse: where are we and where do we go from here? *Exp. Clin. Psychopharmacol.* **24**, 400–414 (2016).
- Benson, K., Flory, K., Humphreys, K. L. & Lee, S. S. Misuse of stimulant medication among college students: a comprehensive review and meta-analysis. *Clin. Child Fam. Psych.* **18**, 50–76 (2015).
- Herman, L. et al. The use of prescription stimulants to enhance academic performance among college students in health care programs. *J. Physician Assist. Educ.* **22**, 15–22 (2011).
- Lutz, A., Slagter, H. A., Dunne, J. D. & Davidson, R. J. Attention regulation and monitoring in meditation. *Trends Cogn. Sci.* **12**, 163–169 (2008).
- Chiesa, A., Calati, R. & Serretti, A. Does mindfulness training improve cognitive abilities? A systematic review of neuropsychological findings. *Clin. Psychol. Rev.* **31**, 449–464 (2011).
- Zeidan, F., Johnson, S. K., Gordon, N. S. & Goolkasian, P. Effects of brief and sham mindfulness meditation on mood and cardiovascular variables. *J. Altern. Complement. Med.* **16**, 867–873 (2010).
- Morrison, A. B., Goolsarran, M., Rogers, S. L. & Jha, A. P. Taming a wandering attention: short-form mindfulness training in student cohorts. *Front. Hum. Neurosci.* **7**, 897 (2014).
- Tang, Y. Y. et al. Short-term meditation training improves attention and self-regulation. *Proc. Natl Acad. Sci. USA* **104**, 17152–17156 (2007).
- Deci, E. L., Koestner, R. & Ryan, R. M. A meta-analytic review of experiments examining the effects of extrinsic rewards on intrinsic motivation. *Psychol. Bull.* **125**, 627–668 (1999).
- Spjorkerman, M. P., Pots, W. T. & Bohlmeijer, E. T. Effectiveness of online mindfulness-based interventions in improving mental health: a review and meta-analysis of randomised controlled trials. *Clin. Psychol. Rev.* **45**, 102–114 (2016).
- Mani, M., Kavanagh, D. J., Hides, L. & Stoyanov, S. R. Review and evaluation of mindfulness-based iPhone apps. *JMIR Mhealth Uhealth* **3**, e82 (2015).
- Noone, C. & Hogan, M. J. A randomised active-controlled trial to examine the effects of an online mindfulness intervention on executive control, critical thinking and key thinking dispositions in a university student sample. *BMC Psychol.* **6**, 13 (2018).
- Boot, W. R., Simons, D. J., Stothart, C. & Stutts, C. The pervasive problem with placebos in psychology: why active control groups are not sufficient to rule out placebo effects. *Perspect. Psychol. Sci.* **8**, 445–454 (2013).
- MacDonald, S. W., Nyberg, L. & Backman, L. Intra-individual variability in behavior: links to brain structure, neurotransmission and neuronal activity. *Trends Neurosci.* **29**, 474–480 (2006).
- Karalunas, S. L., Geurts, H. M., Konrad, K., Bender, S. & Nigg, J. T. Annual research review: reaction time variability in ADHD and autism spectrum disorders: measurement and mechanisms of a proposed trans-diagnostic phenotype. *J. Child Psychol. Psychiatry* **55**, 685–710 (2014).
- Gorus, E., De Raedt, R., Lambert, M., Lemper, J. C. & Mets, T. Reaction times and performance variability in normal aging, mild cognitive impairment, and Alzheimer’s disease. *J. Geriatr. Psychiatry Neurol.* **21**, 204–218 (2008).

29. Tales, A. et al. Intra-individual reaction time variability in amnesic mild cognitive impairment: a precursor to dementia? *J. Alzheimers Dis.* **32**, 457–466 (2012).
30. Kalin, A. M. et al. Intraindividual variability across cognitive tasks as a potential marker for prodromal Alzheimer's disease. *Front. Aging Neurosci.* **6**, 147 (2014).
31. Lutz, A. et al. Mental training enhances attentional stability: neural and behavioral evidence. *J. Neurosci.* **29**, 13418–13427 (2009).
32. Braverman, E. R. et al. Delayed P300 latency correlates with abnormal test of variables of attention (TOVA) in adults and predicts early cognitive decline in a clinical setting. *Adv. Ther.* **23**, 582–600 (2006).
33. Scheeringa, R. et al. Frontal theta EEG activity correlates negatively with the default mode network in resting state. *Int. J. Psychophysiol.* **67**, 242–251 (2008).
34. Bledowski, C. et al. Localizing P300 generators in visual target and distractor processing: a combined event-related potential and functional magnetic resonance imaging study. *J. Neurosci.* **24**, 9353–9360 (2004).
35. Clayton, M. S., Yeung, N. & Cohen Kadosh, R. The roles of cortical oscillations in sustained attention. *Trends Cogn. Sci.* **19**, 188–195 (2015).
36. Mishra, J., de Villers-Sidani, E., Merzenich, M. & Gazzaley, A. Adaptive training diminishes distractibility in aging across species. *Neuron* **84**, 1091–1103 (2014).
37. Sali, A. W., Courtney, S. M. & Yantis, S. Spontaneous fluctuations in the flexible control of covert attention. *J. Neurosci.* **36**, 445–454 (2016).
38. Brewer, J. A. et al. Meditation experience is associated with differences in default mode network activity and connectivity. *Proc. Natl Acad. Sci. USA* **108**, 20254–20259 (2011).
39. Slagter, H. A. et al. Mental training affects distribution of limited brain resources. *PLoS Biol.* **5**, e138 (2007).
40. Kiyonaga, A. & Egner, T. Working memory as internal attention: toward an integrative account of internal and external selection processes. *Psychon. Bull. Rev.* **20**, 228–242 (2013).
41. Paolacci, G. & Chandler, J. Inside the Turk: understanding Mechanical Turk as a participant pool. *Curr. Dir. Psychol. Sci.* **23**, 184–188 (2014).
42. Reijnders, J., van Heugten, C. & van Boxtel, M. Cognitive interventions in healthy older adults and people with mild cognitive impairment: a systematic review. *Ageing Res. Rev.* **12**, 263–275 (2013).
43. Boot, W. R. et al. Video games as a means to reduce age-related cognitive decline: attitudes, compliance, and effectiveness. *Front. Psychol.* **4**, 31 (2013).
44. Eisendrath, S. J. et al. A randomized controlled trial of mindfulness-based cognitive therapy for treatment-resistant depression. *Psychother. Psychosom.* **85**, 99–110 (2016).
45. Stevens, J. C. *Applied Multivariate Statistics for the Social Sciences* (Erlbaum Associates, 1986).
46. Cohen, J. *Statistical Power Analysis for the Behavioral Sciences* 2nd edn (Erlbaum Associates, 1988).
47. Hedeker, D., Gibbons, R. D. & Waternaux, C. Sample size estimation for longitudinal designs with attrition: comparing time-related contrasts between two groups. *J. Educ. Behav. Stat.* **24**, 70–93 (1999).
48. Greenberg, L. M. *TOVA Continuous Performance Test Manual* (The TOVA Company, 1996).
49. Vogel, E. K., McCollough, A. W. & Machizawa, M. G. Neural measures reveal individual differences in controlling access to working memory. *Nature* **438**, 500–503 (2005).
50. Jost, K., Bryck, R. L., Vogel, E. K. & Mayr, U. Are old adults just like low working memory young adults? Filtering efficiency and age differences in visual working memory. *Cereb. Cortex* **21**, 1147–1154 (2011).
51. Lee, E. Y. et al. Visual working memory deficits in patients with Parkinson's disease are due to both reduced storage capacity and impaired ability to filter out irrelevant information. *Brain* **133**, 2677–2689 (2010).
52. Johnson, M. K. et al. The relationship between working memory capacity and broad measures of cognitive ability in healthy adults and people with schizophrenia. *Neuropsychology* **27**, 220–229 (2013).
53. Clapp, W. C., Rubens, M. T., Sabharwal, J. & Gazzaley, A. Deficit in switching between functional brain networks underlies the impact of multitasking on working memory in older adults. *Proc. Natl Acad. Sci. USA* **108**, 7212–7217 (2011).
54. Clapp, W. C. & Gazzaley, A. Distinct mechanisms for the impact of distraction and interruption on working memory in aging. *Neurobiol. Aging* **33**, 134–148 (2012).
55. Cohen, M. X. & Cavanagh, J. F. Single-trial regression elucidates the role of prefrontal theta oscillations in response conflict. *Front. Psychol.* **2**, 30 (2011).
56. Wickens, C., Kramer, A., Vanasse, L. & Donchin, E. Performance of concurrent tasks: a psychophysiological analysis of the reciprocity of information-processing resources. *Science* **221**, 1080–1082 (1983).
57. Polich, J. & Kok, A. Cognitive and biological determinants of P300: an integrative review. *Biol. Psychol.* **41**, 103–146 (1995).
58. Dale, A. M., Fischl, B. & Sereno, M. I. Cortical surface-based analysis. I. Segmentation and surface reconstruction. *Neuroimage* **9**, 179–194 (1999).
59. Fischl, B., Sereno, M. I. & Dale, A. M. Cortical surface-based analysis. II: inflation, flattening, and a surface-based coordinate system. *Neuroimage* **9**, 195–207 (1999).
60. Fischl, B., Sereno, M. I., Tootell, R. B. & Dale, A. M. High-resolution intersubject averaging and a coordinate system for the cortical surface. *Hum. Brain Mapp.* **8**, 272–284 (1999).
61. Gramfort, A., Papadopoulos, T., Olivi, E. & Clerc, M. OpenMEEG: opensource software for quasistatic bioelectromagnetics. *Biomed. Eng. Online* **9**, 45 (2010).
62. Tadel, F., Baillet, S., Mosher, J. C., Pantazis, D. & Leahy, R. M. Brainstorm: a user-friendly application for MEG/EEG analysis. *Comput. Intell. Neurosci.* **2011**, 879716 (2011).
63. Knapp, T. R. & Schafer, W. D. From gain score t to ANCOVA F (and vice versa). *Pract. Assess. Res. Eval.* **14**, 1–7 (2009).
64. Locascio, J. J. & Cordray, D. S. A reanalysis of Lord paradox. *Educ. Psychol. Meas.* **43**, 115–126 (1983).
65. Polich, J. Updating P300: an integrative theory of P3a and P3b. *Clin. Neurophysiol.* **118**, 2128–2148 (2007).
66. Papenberg, G., Hammerer, D., Muller, V., Lindenberger, U. & Li, S. C. Lower theta inter-trial phase coherence during performance monitoring is related to higher reaction time variability: a lifespan study. *Neuroimage* **83**, 912–920 (2013).
67. Delorme, A. & Makeig, S. EEGLAB: an open source toolbox for analysis of single-trial EEG dynamics including independent component analysis. *J. Neurosci. Methods* **134**, 9–21 (2004).
68. Tallon-Baudry, C., Bertrand, O., Delpuech, C. & Pernier, J. Stimulus specificity of phase-locked and non-phase-locked 40 Hz visual responses in human. *J. Neurosci.* **16**, 4240–4249 (1996).
69. Johnson, R. Jr. On the neural generators of the P300 component of the event-related potential. *Psychophysiology* **30**, 90–97 (1993).

Acknowledgements

We thank H. Cho, S. Corona, A. Ho, K. Huang, J. Kang, D. Kingsbrook, R. LoPilato, M. Kim, J. Martin, L. Martin, A. Recinos and M. Torres for help with data collection and T. Zanto for advice on EEG data analysis and interpretation. Thanks to A. Denison-Afifi, A. Speight, K. Stern, K. Weber and numerous other volunteers at Zynga.org for assistance in designing and building the MediTrain software and to A. Duanmu for critical programming support of the application during the study. We also thank R. Campusano, J. Gazzaley, A. Leggett and H. Weng for helpful discussions. Thanks to all of our participants and to Apple who generously provided many of the iPads used in this study. Jamie Gates, Evan and Sara Williams, Zynga.org and NIH grants R21 AG041071 and R01 AG049424 provided financial support for this research. The funders had no role in study design, data collection and analysis, decision to publish or preparation of the manuscript.

Author contributions

D.A.Z., A.J.S., S.S., J.M., J.A.A. and A.G. designed the experiments. D.A.Z., J.R.J., J.K. and A.G. developed the MediTrain software. D.A.Z., A.J.S., S.S., J.J.V. and C.E.R. collected the data. D.A.Z., A.J.S., C.L.G., S.S., J.J.V. and J.A.A. analysed the data. D.A.Z., A.J.S. and A.G. wrote the paper. All authors discussed the results and contributed to editing the manuscript.

Competing interests

A.G. is co-founder, shareholder, BOD member and advisor for Akili Interactive, a company that produces therapeutic video games. MediTrain and the apps used for the control condition are not currently associated with Akili. The other authors declare no competing interests.

Additional information

Supplementary information is available for this paper at <https://doi.org/10.1038/s41562-019-0611-9>.

Reprints and permissions information is available at www.nature.com/reprints.

Correspondence and requests for materials should be addressed to D.A.Z. or A.G.

Publisher's note: Springer Nature remains neutral with regard to jurisdictional claims in published maps and institutional affiliations. © The Author(s), under exclusive licence to Springer Nature Limited 2019

Reporting Summary

Nature Research wishes to improve the reproducibility of the work that we publish. This form provides structure for consistency and transparency in reporting. For further information on Nature Research policies, see [Authors & Referees](#) and the [Editorial Policy Checklist](#).

Statistical parameters

When statistical analyses are reported, confirm that the following items are present in the relevant location (e.g. figure legend, table legend, main text, or Methods section).

n/a Confirmed

- ☐ ☒ The exact sample size (n) for each experimental group/condition, given as a discrete number and unit of measurement
- ☐ ☒ An indication of whether measurements were taken from distinct samples or whether the same sample was measured repeatedly
- ☐ ☒ The statistical test(s) used AND whether they are one- or two-sided
Only common tests should be described solely by name; describe more complex techniques in the Methods section.
- ☐ ☒ A description of all covariates tested
- ☐ ☒ A description of any assumptions or corrections, such as tests of normality and adjustment for multiple comparisons
- ☐ ☒ A full description of the statistics including central tendency (e.g. means) or other basic estimates (e.g. regression coefficient) AND variation (e.g. standard deviation) or associated estimates of uncertainty (e.g. confidence intervals)
- ☐ ☒ For null hypothesis testing, the test statistic (e.g. F , t , r) with confidence intervals, effect sizes, degrees of freedom and P value noted
Give P values as exact values whenever suitable.
- ☒ ☐ For Bayesian analysis, information on the choice of priors and Markov chain Monte Carlo settings
- ☒ ☐ For hierarchical and complex designs, identification of the appropriate level for tests and full reporting of outcomes
- ☐ ☒ Estimates of effect sizes (e.g. Cohen's d , Pearson's r), indicating how they were calculated
- ☐ ☒ Clearly defined error bars
State explicitly what error bars represent (e.g. SD, SE, CI)

Our web collection on [statistics for biologists](#) may be useful.

Software and code

Policy information about [availability of computer code](#)

Data collection

Experiment 1: Amazon's Mechanical Turk; Experiment 2: Eprime V2.0, Matlab Psychophysics-3 Toolbox; Presentation (<http://neurobs.com>); Biosemi's ActiView EEG recording software; Experiment 3: Presentation (<http://neurobs.com>); Biosemi's ActiView EEG recording software

Data analysis

Matlab + EEGLab toolbox; IBM SPSS v20

For manuscripts utilizing custom algorithms or software that are central to the research but not yet described in published literature, software must be made available to editors/reviewers upon request. We strongly encourage code deposition in a community repository (e.g. GitHub). See the Nature Research [guidelines for submitting code & software](#) for further information.

Data

Policy information about [availability of data](#)

All manuscripts must include a [data availability statement](#). This statement should provide the following information, where applicable:

- Accession codes, unique identifiers, or web links for publicly available datasets
- A list of figures that have associated raw data
- A description of any restrictions on data availability

The data that support the findings of this study are available from the corresponding author upon reasonable request.

Field-specific reporting

Please select the best fit for your research. If you are not sure, read the appropriate sections before making your selection.

☒ Life sciences ☐ Behavioural & social sciences ☐ Ecological, evolutionary & environmental sciences

For a reference copy of the document with all sections, see [nature.com/authors/policies/ReportingSummary-flat.pdf](https://www.nature.com/authors/policies/ReportingSummary-flat.pdf)

Life sciences study design

All studies must disclose on these points even when the disclosure is negative.

Sample size	Experiment 1: N = 334 total; no power analysis performed Experiment 2: N = 59 randomized; With two time points and assuming a moderate repeated measures correlation ($r = 0.5$) and a two-tailed test ($\alpha = .05$), we calculated that N = 30 per group would yield 75% power^[1,2] to detect a change with a medium effect size (.5)^[3]. Of the 59, 24 participants completed MediTrain and 20 completed placebo training. We excluded two MediTrain participants due to irregularities in their software usage and two placebo control participants from analysis of the vigilance task behavioral data because they were extreme outliers, resulting in a final as-treated sample of n=22 in Meditrain and n = 18 in placebo. Complete EEG datasets were obtained from 12 participants in each group (MediTrain or placebo) with both pre- and post-intervention measurements in each group (see Methods for details about missing data). Experiment 3: N = 73; no power analysis performed 1. Stevens JC. Applied Multivariate Statistics for the Social Sciences. Hillsdale, NJ: Erlbaum Associates; 1986. 2. Cohen J. Statistical Power Analysis for the Behavioral Sciences. 2 ed. Hillsdale, NJ: Erlbaum Associates; 1988. 3. Hedeker, Gibbons, Waternaux. Sample size estimation for longitudinal designs with attrition. Journal of Educational and Behavioral Statistics. 1999;24:70-93.
Data exclusions	For behavioral data, we excluded 4 participants (2 per group) who were defined as extreme outliers (defined as falling more than three SD beyond the group mean). For EEG data, we excluded datasets where excessive noise led to 30% of target trials being rejected, with the rejection criteria being anything greater than a +/- 100µV voltage deflection within an epoch 50 (n = 4 at pre-training, n = 3 at post-training). In the MediTrain group, there were ultimately 14 participants with usable EEG data at pre- intervention, and 21 participants at post. In the placebo group, there were 16 participants with usable data at pre-intervention, and 15 participants at post.
Replication	Given the longitudinal nature of the study and human subjects population, no attempts were made at replication.
Randomization	Participants were randomized to either the MediTrain group or the placebo control group (see Supplementary Figure 4 for Consort Table).
Blinding	The study was designed in a double-blinded manner, such that all participants received the same instructions and laboratory personnel who were collecting data were unaware of group assignments. Double-blinding began at the point of recruitment, where all participants were informed that they were being recruited for a study designed to test the efficacy of software interventions for improving cognition in a variety of domains. As such, participants in both the MediTrain and placebo groups had equal expectations that they were part of an active treatment group (see below for details on establishing matched expectancy of improvement across conditions). All staff that collected data were blind to group assignment; one study coordinator (S.S.) was informed of the treatment assignments in order to provide technical and other support during the training.

Reporting for specific materials, systems and methods

Materials & experimental systems

n/a	Involved in the study
☒	☐ Unique biological materials
☒	☐ Antibodies
☒	☐ Eukaryotic cell lines
☒	☐ Palaeontology
☒	☐ Animals and other organisms
☐	☒ Human research participants

Methods

n/a	Involved in the study
☒	☐ ChIP-seq
☒	☐ Flow cytometry
☒	☐ MRI-based neuroimaging

Human research participants

Policy information about [studies involving human research participants](#)

Population characteristics	We recruited 59 healthy young adults (18-35 years of age) who met our inclusion criteria for this longitudinal study. All participants were screened for a history of neurological disease or current psychiatric illness and current use of psychotropic medications. We only enrolled participants who did not have a history of meditation experience, defined as currently practicing one or more days per week, having practiced on a weekly basis in the past, or having attended a meditation retreat of three or more days. These criteria for defining a sample as meditation-naïve are consistent with prior studies and were developed through conversations with expert colleagues familiar with such designs (Stuart Eisendrath, personal communication). We performed vision testing with a Snellen chart and acuity differences were corrected to 20/40 or better. All participants gave informed consent to participate in the study according to procedures approved by the Committee for Human Research at the University of California San Francisco.
Recruitment	Participants were recruited from the local community using flyers posted at local colleges and universities, through online ads, and via word of mouth.