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Placebo effects in cognitive training

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Although a large body of research shows that general cognitive ability is heritable and stable in young adults, there is recent evidence that fluid intelligence can be heightened with cognitive training. Many researchers, however, have questioned the methodology of the cognitive-training studies reporting improvements in fluid intelligence: specifically, the role of placebo effects. We designed a procedure to intentionally induce a placebo effect via overt recruitment in an effort to evaluate the role of placebo effects in fluid intelligence gains from cognitive training. Individuals who self-selected into the placebo group by responding to a suggestive flyer showed improvements after a single, 1-h session of cognitive training that equates to a 5- to 10-point increase on a standard IQ test. Controls responding to a nonsuggestive flyer showed no improvement. These findings provide an alternative explanation for effects observed in the cognitive-training literature and the brain-training industry, revealing the need to account for confounds in future research.

placebo effects | cognitive training | brain training | fluid intelligence

> What's more, working memory is directly related to intelligence—the more you train, the smarter you can be.
>
> NeuroNation (www.neuronation.com/, May 8, 2016)

The above quotation, like many others from the billion-dollar brain-training industry (1), suggests that cognitive training can make you smarter. However, the desire to become smarter may blind us to the role of placebo effects. Placebo effects are well known in the context of drug and surgical interventions (2, 3), but the specter of a placebo may arise in any intervention when the desired outcome is known to the participant—an intervention like cognitive training. Although a large body of research shows that general cognitive ability, *g*, is heritable (4, 5) and stable in young adults (6), recent research stands in contrast to this, indicating that intelligence can be heightened by cognitive training (7–12). General cognitive ability and IQ are related to many important life outcomes, including academic success (13, 14), job performance (15), health (16, 17), morbidity (18), mortality (18, 19), income (20, 21), and crime (13). In addition, the growing population of older people seeks ways to stave off devastating cognitive decline (22). Thus, becoming smarter or maintaining cognitive abilities via cognitive training is a powerful lure, raising important questions about the role of placebo effects in training studies.

The question of whether intelligence can be increased through training has generated a lively scientific debate. Recent research claims that it is possible to improve fluid intelligence (*Gf*: a core component of general cognitive ability, *g*) by means of working memory training (7–12, 23, 24); even meta-analyses support these claims (25, 26), concluding that improvements from cognitive training equate to an increase "…of 3–4 points on a standardized IQ test." (ref. 25; but cf. ref. 27). However, researchers have yet to identify, test, and confirm a clear mechanism underlying fluid intelligence gains after cognitive training (28). One potential mechanism that has yet to be tested is that the observed effects are partially due to positive expectancy or placebo effects.

Researchers now recognize that placebo effects may potentially confound cognitive-training [i.e., "brain training" (29)] outcomes and may underlie some of the posttraining fluid intelligence gains (24, 27, 29–32). Specifically, it has been argued that "overt" recruitment methods in which the expected benefits of training are stated (or implied) may lead to a sampling bias in the form of self-selection, such that individuals who expect positive results will be overrepresented in any sample of participants (29, 33). If an individual volunteers to participate in a study entitled "Brain Training and Cognitive Enhancement" because he or she thinks the training will be effective, any effect of the intervention may be partially or fully explained by participant expectations.

Expectations regarding the efficacy of cognitive training may be rooted in beliefs regarding the malleability of intelligence (34). Dweck's (34) work showed that people tend to hold strong implicit beliefs regarding whether or not intelligence is malleable and that these beliefs predict a number of learning and academic outcomes. Consistent with that work, there is evidence that individuals with stronger beliefs in the malleability of intelligence have greater improvements in fluid intelligence tasks after working-memory training (10). If individuals who believe that intelligence is malleable are overrepresented in a sample, the apparent effect of training may be related to the belief of malleability, rather than to the training itself.

The present study was motivated by concerns about overt recruitment and self-selection bias (29, 33), as well as our own observation that few published articles on cognitive training provide details regarding participant recruitment. In fact, of the primary studies included in the meta-analysis of Au et al. (25) only two provided sufficient detail to determine whether participants were recruited overtly [e.g., "sign up for a brain training study" (10)] or covertly [e.g., "did not inform subjects that they were participating in a training study" (24)]. (We were able to assess 18 of the 20 studies.) We later emailed the corresponding authors from all of the studies in the Au et al. (25) meta-analysis for more detailed recruitment information. (This step was done at the suggestion of a reviewer and occurred after data collection

Significance

Placebo effects pose problems for some intervention studies, particularly those with no clearly identified mechanism. Cognitive training falls into that category, and yet the role of placebos in cognitive interventions has not yet been critically evaluated. Here, we show clear evidence of placebo effects after a brief cognitive training routine that led to significant fluid intelligence gains. Our goal is to emphasize the importance of ruling out alternative explanations before attributing the effect to interventions. Based on our findings, we recommend that researchers account for placebo effects before claiming treatment effects.

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Data deposition: The data have been achived on Figshare, https://figshare.com/articles/Placebo_csv/2062479.

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was complete. We chose to place this information here instead of the discussion to accurately portray the current recruitment standards within the field.) All but one author responded. We determined that 17 (of 19) studies used overt recruitment methods that could have introduced a self-selection bias. Specifically, 17 studies explicitly mentioned "cognitive" or "brain" training. Of those 17, we found that 11 studies further suggested the potential for improvement or enhancement. Only two studies didn't mention either (Table S1). A comparison of effect sizes listed in the Au et al. (25) meta-analysis by these three methods of recruitment (i.e., overt, overt and suggestive, and covert) lends further credence to the possibility of a confounded placebo effect. For all of the studies that overtly recruited, Hedge's $g = 0.27$; for all of the studies that overtly recruited and suggested improvement, Hedge's $g = 0.28$; and for the studies that covertly recruited, Hedge's $g = 0.11$. Lastly, we searched the internet (via Google) for the terms "participate in a brain training study" and "brain training participate." The top 10 results for both searches revealed six separate laboratories that are actively and overtly recruiting individuals to participants in either a "brain training study" or a "cognitive training study." Taken together, these findings provide clear evidence that suggestive recruitment methods are common and that such recruitment may contribute to the positive outcomes reported in the cognitive-training literature. We therefore hypothesized that overt and suggestive recruitment would be sufficient to induce positive posttraining outcomes.

Materials and Methods

We designed a procedure to intentionally induce a placebo effect via overt recruitment. Our recruitment targeted two populations of participants using different advertisements varying in the degree to which they evoked an expectation of cognitive improvement (Fig. 1). Once participants self-selected into the two groups, they completed two pretraining fluid intelligence tests followed by 1 h of cognitive training and then completed two posttraining fluid intelligence tests on the following day. Two individual difference metrics regarding beliefs about cognition and intelligence were also collected as potential moderators. The researchers who interacted with participants were blind to the goal of the experiment and to the experimental condition. Aside from their means of recruitment, all participants completed identical cognitive-training experiments. All participants read and signed an informed consent form before beginning the experiment. The George Mason University Institutional Review Board approved this research.

We recruited the placebo group ($n = 25$) with flyers overtly advertising a study for brain training and cognitive enhancement (d). The text "Numerous studies have shown working memory training can increase fluid intelligence" was clearly visible on the flyer. We recruited the control group ($n = 25$) with a visually similar flier containing generic content that did not mention brain training or cognitive enhancement. We determined the sample sizes for both groups based upon two a priori criteria: (*i*) Previous, significant training studies had sample sizes of 25 or fewer (7, 8); and (*ii*) statistical power analyses (power ≥ 0.7) on between-group designs dictated a sample size of 25 per group for a moderate to large effect size ($d \geq 0.7$). Our rationale for the first criterion was that we were trying to replicate previous training studies, but with the additional manipulation of a placebo that had been omitted in those studies. The second criterion simply allowed us a good chance to find a reasonably large and important effect with the sample size we selected. In sum, we felt that the sample size allowed for a good replication of prior studies, but restricted us to finding only worthwhile results to report. The final sample of participants consisted of 19 males and 31 females, with an average age of 21.5 y (SD = 2.3). The groups ($n = 50$; 25 for each condition) did not differ by age [$t(48) = 0.18$, $P = 0.856$] or by gender composition [$\chi^2(1) = 0.76$, $P = 0.382$].

After the pretests (*Gf* assessments described below), participants completed 1 h of cognitive training with an adaptive dual *n*-back task (*SI Materials and Methods*). We chose this task for two reasons: First, it is commonly used in cognitive training research, and, second, a high-face-validity task was required to maintain the credibility of the training regimen [compare placebo pain medication appearing identical to the real medication (35)]. In this task, participants were presented with two streams of information: auditory and

Fig. 1. Recruitment flyers for placebo (*Left*) and control (*Right*) groups.

visuospatial. There were eight stimuli per modality that were presented at a rate of 3 s per stimuli. For each stream, participants decided whether the current stimulus matched the stimulus that was presented *n* items ago. Our *n*-back task was an adaptive version in which the level of *n* changed as performance increased or decreased within each block.

Results

All analyses were conducted by using mixed-effects linear regression with restricted maximum likelihood. As expected, both groups' training performance improved over time [B = 0.016, SE = 0.002, $t(48) = 10.5$, $P < 0.001$]. All participants began at 2-back; 18% did not advance beyond a 2-back, 14% finished training at a 4-back, and 68% at a 3-back. Training performance did not differ by group [B = –0.002, SE = 0.002, $t(48) = -1.00$, P = 0.321): Both the placebo and control groups completed training with a similar degree of success. A placebo effect can occur in the absence of training differences between groups.

The placebo effect does, however, necessitate an effect on the outcome of interest. Pretraining and posttraining fluid intelligence was measured with Raven's Advanced Progressive Matrices (RAPM) and Bochumer Matrices Test (BOMAT), two tests of inductive reasoning widely used to assess *Gf* (36–38). No baseline differences were found between groups on either test [$t(48) = -0.063$, P = 0.939 and $t(48) = -0.123$, P = 0.938, respectively]. We observed a main effect of time on test performance in which scores on both intelligence tests increased from pretraining to posttraining. These main effects of time on both intelligence measures, however, were qualified by an interaction by group [RAPM: B = 0.65, SE = 0.19, $t(48) = 3.41$, P = 0.0013, d = 0.98; and BOMAT: B = 0.82, SE = 0.18, $t(48) = 4.63$, $P < 0.0001$, d = 1.34]. Specific contrasts showed that these moderation effects were entirely driven by the participants in the placebo group—the only individuals in the study to score significantly higher on posttraining compared with pretraining sessions for both RAPM [B = –1.04, SE = 0.19, $t(48) = -5.46$, $P < 0.0001$, d = 0.50] and for the BOMAT [B = –1.28, SE = 0.18, $t(48) = -7.22$, $P < 0.0001$, d = 0.39]. Extrapolating RAPM to IQ (25, 39, 40), these improvements equate to a 5- to 10-point increase on a standardized 100-point IQ test (*SI Materials and Methods*). In contrast, the pretraining and posttraining scores for participants in the control group were statistically indistinguishable, both for RAPM [B = –0.12, SE = 0.19, $t(48) = -0.63$, P = 0.922] and for the BOMAT [B = –0.12, SE = 0.18, $t(48) = -0.68$, P = 0.905]. The results are summarized in Tables S2–S4 and depicted in Fig. 2. Interestingly, pooling the data across groups to form one sample (combining the self-selection and control groups) revealed significant posttraining outcomes [B = 0.41, SE = 0.11, $t(49) = 3.90$, P = 0.0003, d = 0.28 (RAPM); and B = 0.50, SE = 0.15, $t(49)$ = 4.69, $P < 0.0001$, d = 0.21 (BOMAT)]. That is, the effect from the placebo group was strong enough to overcome the null effect from the control group (when pooled).

We also observed differences between groups for scores on the Theories of Intelligence scale, which measures beliefs regarding the malleability of intelligence (34). The participants in the placebo group reported substantially higher scores on this index compared with controls [B = 14.96, SE = 1.93, $t(48) = 7.75$, $P <$ 0.0001, d = 2.15], indicating a greater confidence that intelligence is malleable. These findings indicate that our manipulation via recruitment flyer produced significantly different groups with regard to expectancy. We did not detect differences in Need for Cognition scores (41) [B = 0.56, SE = 5.67, $t(48)$ = 0.10, P = 0.922] (Fig. 3). Together, these results support the interpretation that participants self-selected into groups based on differing expectations.

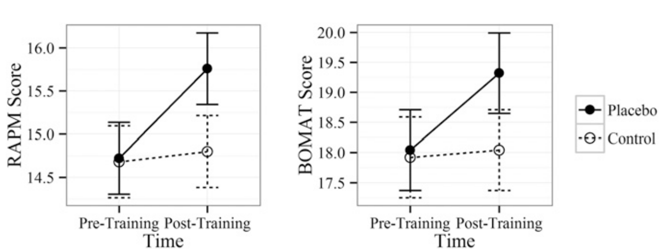

Fig. 2. Estimated marginal means of the RAPM (*Left*) and BOMAT (*Right*) scores by time and group; errors bars represent SEs.

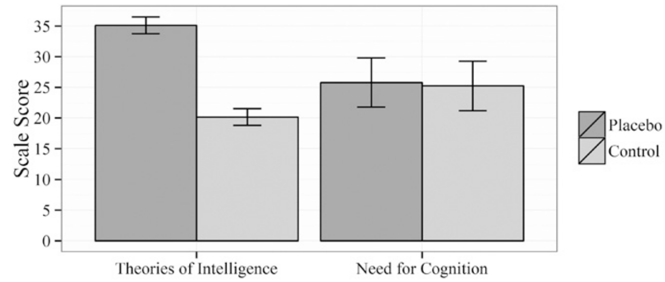

Fig. 3. Estimated marginal means of the Theories of Intelligence and Need for Cognition scales by group; error bars represent SEs.

We also tested whether the response time to volunteer for our study influenced the aforementioned findings. Specifically, we noticed that the placebo condition appeared to fill faster than the control condition did (366 vs. 488 h). It is possible that speed of signup might represent another measure for—or perhaps gradations within—the strength of the placebo effect. The volunteer response time differences by group failed to produce a significant effect on either the RAPM [B = 0.04, SE = 0.17, $t(46) = 0.23$, P = 0.819] or the BOMAT [B = 0.20, SE = 0.16, $t(46) = 1.28$, P = 0.201]. Volunteer response time also failed to explain the improvement observed within the placebo group alone, on RAPM [B = 0.20, SE = 0.20, $t(23) = 0.95$, P = 0.341] and BOMAT [B = 0.26, SE = 0.22, $t(23) = 1.22$, P = 0.237] (Fig. 4).

Researchers have hypothesized that a training dosage effect may exist, such that the quality of performance on a training task is associated with the degree of subsequent skill transfer (7). However, as discussed previously, no pre–post improvements occurred within the control group, even though all participants performed equally well on the training task. Consequently, training performance did not predict subsequent performance improvement on its own [B = 0.017, SE = 0.20, $t(46) = 0.09$, P = 0.930], nor did it moderate the effect of group on the observed test performance improvements [B = –0.16, SE = 0.28, $t(46) = -0.58$, P = 0.567] (Fig. 5). Therefore, our data do not support the dosage-effect hypothesis.

Discussion

We provide strong evidence that placebo effects from overt and suggestive recruitment can affect cognitive training outcomes. These findings support the concerns of many researchers (24, 27, 29–32), who suggest that placebo effects may underlie positive outcomes seen in the cognitive-training literature. By capitalizing on the self-selecting tendencies of participants with strong positive beliefs about the malleability of intelligence, we were able to induce an improvement in *Gf* after 1 h of working memory training. We acknowledge that the flyer itself could have induced the positive beliefs about the malleability of intelligence. Either way, these findings present an alternative explanation for effects reported in the cognitive-training literature and in the brain-training industry, demonstrating the need to account for placebo effects in future research.

Importantly, we do not claim that our study revealed a population of individuals whose intelligence was truly changed by the

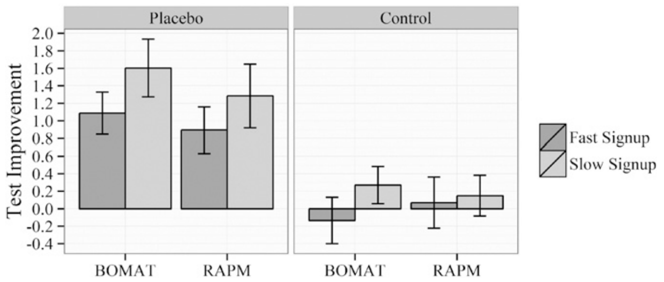

Fig. 4. Improvement in test scores from pretraining to posttraining by group and speed of participant sign up, split into fast and slow ($z = -1$ and $z = 1$ of minutes since experiment onset, respectively). Error bars represent SE.

training that they received in our study. It is extremely unlikely that individuals in the placebo group increased their IQ by 5–10 points with 1 h of cognitive training. Three elements of our design and results support this position. First, a single, 1-h training session is far less than the traditional 15 or more hours spread across weeks commonly used in training studies (8, 10, 23). We argue that the use of a very short training period was sufficient to avoid a true training effect. Second, we observed similar baseline scores on both of the fluid intelligence tests between groups, suggesting that both groups were equally engaged in the experiment. Thus, initial nonequivalence between groups or regression artifacts are likely absent from our design. Third, equivalent performance on the training task between groups suggests that the differences in posttraining intelligence were not the (direct) result of training. If groups showed dramatically different training effects on the dual *n*-back task, it might follow that one group showed higher posttraining scores on the test of general cognitive ability.

Therefore, our study, to our knowledge, is the first to explicitly model the main effect of expectancy effects while controlling for the effect of training. That is, because our design was unlikely to have produced true training effects, our positive effects on *Gf* are solely the result of overt and suggestive recruitment. Although posttraining gains in fluid intelligence are typically discussed in terms of a main effect of training (7, 8, 10, 11), we argue that such studies cannot rule out an interaction between training and effects from overt and suggestive recruitment. Furthermore, based on the evidence we reviewed above, we are unaware of any previous studies that obtained a positive main effect of training in the absence of expectation or self-selection. Indeed, to our knowledge, the rigor of double-blind randomized clinical trials is nonexistent in this research area.

Moving forward, we suggest that researchers exercise care in their design of cognitive training studies. Our findings raise philosophical concerns and questions that merit discussion within the scientific field so that this area of inquiry can advance. We discuss two different schools of thought about how to recruit participants and design training studies. We hope that this work can begin a conversation leading to a consensus on how to best design future research in this field.

First, following in the tradition of randomized controlled trials used in medicine, one approach suggests that recruitment and study design should be as covert as possible (29, 32). Specifically, several research groups have argued for the need to remove study-specific information from the recruitment and briefing procedures, avoid providing the goals of the research to participants, and omit mention of any anticipated outcomes (29, 32, 33). The purpose of such a design would be to minimize any confounding effects (e.g., placebo or expectation). Our earlier review of the Au et al. (25) meta-analyses revealed two studies that followed this approach.

Alternatively, the second approach suggests that we should only recruit participants who believe that the training will work and that we should do this using overt methods. Such a screening process would eliminate participants whose prior beliefs would prevent an otherwise effective treatment from having an effect. That is, if a participant does not care about the training, puts little effort in, and/or is motivated solely by something else (e.g., money), they are not likely to improve with any intervention, including cognitive training. Although positive expectancies would be overrepresented in such an overtly recruited sample, proper use of active controls should allow for training effects to be isolated from expectation. This view is in line with some from the medical domain who argue that researchers can make use of participant expectation to better test treatment effects in randomized controlled trials (42). This view is also in line with some from the psychotherapy domain who argue that motivation is important for treatment effectiveness (43).

One interesting consideration is the likelihood that these two design approaches recruit from different subpopulations. Dweck (34) has shown that individuals hold implicit beliefs regarding whether or not intelligence is malleable and that these beliefs predict a number of learning and academic outcomes. Thus, it is possible that the benefits from cognitive training occur only in individuals who believe the training will be effective. That being said, this possibility is not applicable to our data because our design eliminated a main effect of training. It will be important in future work to investigate the relation between expectation and processes of learning during cognitive training.

Our data do not allow us to understand the field as a whole; instead, they allow us to understand existing limitations to current research that require further exploration. To wit, we identified expectancy as a major factor that needs to be considered for a fuller understanding of training effects. More rigorous designs such as double-blind, block randomized controlled trials that measure multiple outcomes may offer a better "test" of these cognitive training effects. Blinding subjects to cognitive training may be the biggest obstacle in these designs—as pointed out by Boot et al. (29), because participants become aware of the goals of the study. Furthermore, assessing expectancy and personal theories of intelligence malleability (cf. ref. 34) before randomization to ensure adequate representation in all groups would allow us to better assess the true training effects and the potential for expectancy to produce effects alone or in interaction with training. Finally, researchers should use more measures of *Gf* to determine whether positive outcomes are the result of latent changes or changes in test-specific performance. We are aware of no study to date—including the present one—that uses these rigorous methods. (We include the present one by design. Our goal was to determine whether a main effect of expectation existed using methods similar to published research.) By using such methods, we can begin to understand whether true training effects exist and

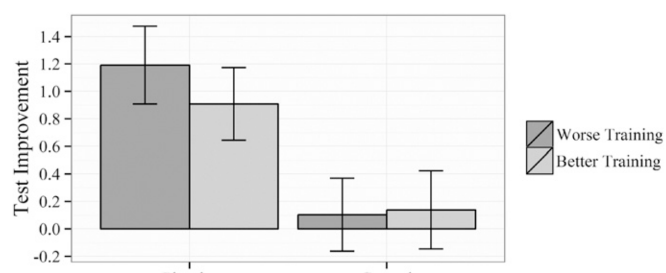

Fig. 5. Improvement in test scores from pretraining to posttraining by group and performance on training task ($z = -1$ and $z = 1$ of training performance, respectively). Error bars represent SE.

are generalizable to samples (and perhaps populations) beyond those who expect to improve.

Conclusion

Our findings have important implications for cognitive-training research and the brain-training industry at large. Previous cognitive-training results may have been inadvertently influenced by placebo effects arising from recruitment or design. For the field of cognitive training to advance, it is important that future work report recruitment information and include the Theories of Intelligence Scale (34) to determine the relation between observed effects of training and of expectancy. The brain-training industry may be advised to temper their claims until the role of placebo effects is better understood. Many commercial brain-training websites make explicit claims about the effectiveness of their training that are not currently supported by many in the scientific community (ref. 44; cf. ref. 45). Consistent with that concern, one of the largest brain-training companies in the world agreed in January 2016 to pay a $2 million fine to the Federal Trade Commission for deceptive advertising about the benefits of their programs (46). The deception—exaggerated claims of training efficacy—may be fueling a placebo effect that may contaminate actual brain-training effects.

We argue that our findings also have broad implications for the advancement of science of human cognition; in a recent replication effort published in *Science*, only 36% (35 of 97) of the psychological science studies (including those that fall under the broad category of neuroscience) were successfully replicated (47). Failure to control or account for placebo effects could have contributed to some of these failed replications. Our goal in any experiment should be to take every step possible to ensure that the effects we seek are the result of manipulated interventions—not confounds that go unreported or undetected.

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