The Placebo and Nocebo Effect on Sports Performance: A Systematic Review

Philip Hurst[a*], Lieke Schipof-Godart[b], Attila Szabo[c], John Raglin[d], Florentina Hettinga[e], Bart Roelands[f], Andrew Lane[g], Abby Foad[a], Damian Coleman[a] & Chris Beedie[h]

[a]School of Human and Life Sciences, Canterbury Christ Church University, Canterbury, UK
[b]Faculty of Health, Nutrition & Sports, The Hague University of Applied Sciences, Hague, the Netherlands
[c]Institute of Health Promotion and Sports Sciences, Eötvös Loránd University, Budapest, Hungary
[d]School of Public Health, Indiana University, Bloomington, IN, USA
[e]School of Sport, Rehabilitation and Exercise Sciences, University of Essex, Colchester, UK
[f]Department of Human Physiology, Vrije Universiteit Brussel, Brussels, Belgium
[g]Faculty of Education Health and Wellbeing, Institute of Sport, University of Wolverhampton, Walsall, UK
[h]School of Psychology, University of Kent, Canterbury, UK

*Correspondence: Philip Hurst, School of Human and Life Sciences, Canterbury Christ Church University, Canterbury, UK. Email: philip.hurst@canterbury.ac.uk

#	Name	Email address
1	Dr Philip Hurst	philip.hurst@canterbury.ac.uk
2	Dr Lieke Schipof-Godart	l.schiphof.godart@gmail.com
3	Professor Attila Szabo	szabo.attila@ppk.elte.hu
4	Professor John Raglin	raglinj@indiana.edu
5	Professor Florentina Hettinga	florentina.hettinga@northumbria.ac.uk
6	Dr Bart Roelands	bart.Roelands@vub.be
7	Professor Andrew Lane	a.m.Lane2@wlv.ac.uk
8	Dr Abby Foad	abby.foad@canterbury.ac.uk
9	Dr Damian Coleman	damian.coleman@canterbury.ac.uk
10	Professor Chris Beedie	chris.beedie@kcl.ac.uk

Abstract

The aim of this review was to determine the magnitude of the placebo and nocebo effect on sport performance. Articles published before March 2019 were located using Medline, Web of Science, PubMed, EBSCO, Science Direct, and Scopus. Studies that examined placebo and nocebo effects of an objective dependent variable on sports performance, which included a control or baseline condition, were included in the analysis. Studies were classified into two categories of ergogenic aids: 1) nutritional and 2) mechanical. Cohen's *d* effect sizes were calculated from 32 studies involving 1,513 participants. Small to moderate placebo effects were found for both placebo (*d* = 0.36) and nocebo (d = 0.37) effects and when separated by nutritional (*d* = 0.35) and mechanical (*d* = 0.47) ergogenic aids. The pooled effect size revealed a small to moderate effect size across all studies (*d* = 0.38). Results suggest that placebo and nocebo effects can exert a small to moderate effect on sports performance.

Key Words: belief, effectiveness, motor, nocebo, nutrition, supplements

Introduction

The placebo effect is a psychobiological response to a purported beneficial treatment (Hurst, Foad, Coleman, & Beedie, 2017). The nocebo effect is the opposite, a psychobiological response to a purported harmful treatment (Beedie et al., 2018). In the last two decades, research in sport and exercise science suggests that placebo and nocebo effects can significantly influence sport performance (Beedie et al., 2018). In this review, we aim to determine the magnitude of the placebo and nocebo effect on sport performance in studies published to date.

Historically, placebos are used as a control treatment that is theoretically indistinguishable from the experimental treatment, but without the essential biological or mechanical active component. The 'true' treatment effect is reported as the difference between the effect of the experimental drug/substance/method compared to the effect of the placebo, while the magnitude of placebo effects is the difference between the effect of the placebo and any change in performance in a control group given no treatment. However, the magnitude of the placebo effect can be misattributed to other phenomena, such as response biases, regression to the mean and natural history of the condition under investigation. It is therefore important that any estimate of the magnitude of the placebo and nocebo effect is interpreted in carefully designed investigations in which a placebo is compared with a natural history control group or baseline condition (i.e. where participants receive no treatment of any kind).

To the authors' knowledge, there have only been two reviews investigating placebo effects on sport performance (Beedie & Foad, 2009; Bérdi, Köteles, Szabó, & Bárdos, 2011). While both papers report the significant influence placebo effects have on performance in sport, these reviews focus only on nutritional ergogenic aids (e.g. caffeine, carbohydrate and sodium bicarbonate). This arguably limits our understanding of the full range and magnitude of placebo and nocebo effects in sports performance. We therefore aimed to examine the placebo and nocebo effects of a wider range of ergogenic aids on sports performance. We hope that the review will provide useful information regarding the findings and quality of research. While the term 'placebo effect' is often used to describe a range of psychologically-mediated factors that influence sport performance (e.g. participant-practitioner relationship, white coat syndrome and Hawthorne effects; Hróbjartsson & Gøtzsche, 2010), in this review, we aim to examine the magnitude of the placebo and nocebo effect elicited by a direct experimental manipulation when compared to a no-treatment control group.

Materials and methods

This systematic review followed the guidelines provided by the Preferred Reporting Items for Systematic Review and Meta-Analyses statement (PRISMA; Moher et al., 2015).

Types of studies

Studies that aimed to analyse the placebo and nocebo effect on sports and motor performance were considered for inclusion. Both within- and between-participant design studies reported in English were considered for inclusion. Conference proceedings, reviews, abstracts, book chapters, dissertations and unpublished manuscripts were excluded.

Types of participants

Only studies including participants described as "apparently healthy" or "athletes" were included.

Types of interventions

Studies were only included if they assessed the effect of a placebo or nocebo ergogenic aid, which had no biological or mechanical capacity to directly modify the dependent variable. Ergogenic aids were categorised into nutritional (e.g. caffeine) and mechanical (e.g. Transcutaneous nerve stimulation [TENS]).

Outcome measures
Studies that did not report at least one direct measure of performance (e.g. power output, speed, or time to completion) and those reporting only subjective outcomes (e.g. pain, fatigue, and perceived exertion) were excluded.

Experimental controls
To enable the quantification of the placebo and nocebo effect, only studies that reported either a no-treatment control, or in repeated measures studies, a baseline in which participants' own performance acted as a no-treatment control, were included. We excluded studies that reported experimental or control treatments of questionable rigour, for example when participants' knowledge concerning whether or not they had been given a placebo or the 'real' treatment was not measured and reported.

Search strategy
Six electronic databases were searched from their earliest entries up until March 2019 (i.e. Medline, Web of Science, PubMed, EBSCO, Science Direct, and Scopus). Reference lists of studies that were considered for inclusion and studies citing these articles were examined. The following search strategy was applied: "placebo effect" OR "nocebo effect" OR "belief effect" OR "placebo response" OR "nocebo response" OR "deceptive" OR "deception" OR "patient expectation" AND "sport" OR "performance" OR "motor" OR "exercise."

Data collection and analysis
Abstracts of records identified through electronic databases were read by the lead author, who excluded all records that did not meet inclusion criteria. The lead and second author read all other records in full and decided on study inclusion independently. Disagreements were resolved by discussion with a third author.

Study characteristics were extracted from all eligible reports including author name, year published, dependent variable, type of placebo treatment, sample size, type of participant and study design. In papers reporting more than one dependent variable, decisions about which one to choose were made by the lead, second and last author, with disagreements resolved via discussion.

A thorough assessment of bias was performed by the lead and second author for all eligible studies. As recommended by The Cochrane Collaboration (Higgins et al., 2011), each potential risk of bias was graded as high, low or unclear. If studies reported random sequence generation (e.g. participants randomised to intervention), allocation concealment (e.g. researchers are unaware of which treatment participant is in), complete outcome data (e.g. systematic differences between groups in withdrawals from a study) and were free of selective reporting (e.g. results that are not significant are deliberately left out), the overall risk of bias was considered "low." All other studies were categorised as "unclear" or "high risk" of bias, for example when researchers or participants were aware of which treatment was given to the participant. However, given that in placebo effect research a key methodological characteristic is that participants are led to believe the treatment they receive will influence their performance, we also assessed whether the study measured participants' belief in the effectiveness of the placebo treatment they received through post-study manipulation checks. If a manipulation check was included, and study outcomes were discussed in light of this, the study was considered as having a low risk of bias.

Mean and standard deviations of main performance measure were extracted from each study. In instances where data were not explicitly reported, the corresponding authors were contacted and asked to provide means and standard deviations. We were unable to obtain the data from two studies (Clark, Hopkins, Hawley, & Burke, 2000; Tallis, Muhammad, Islam, & Duncan, 2016). For Tallis et al. (2016), means and standard errors (SE) were reported in a figure and we estimated the standard

deviations by multiplying the SE by the square root of the sample size (Burda, O'Connor, Webber, Redmond, & Perdue, 2017). For Clark et al. (2000), means and 95% confidence intervals were reported in a figure as percent change from baseline. We were therefore unable to extract means and standard deviations of the performance measure and the study was excluded from analysis.

The mean difference between placebo and controls was standardised by calculating Cohen's *d (d)*; the difference between means of both treatments divided by the pooled standard deviation, using an online statistical spreadsheet (Thalheimer & Cook, 2002). For studies that did not include a no-treatment control, baseline condition was used. Differences between 0.2 and <0.5 were interpreted as a small effect, between 0.5 and <0.8 as moderate, and ≥0.8 as large (Cohen, 1992). In addition, aggregated effect sizes of studies were calculated to determine potential moderators of the placebo effect:

1. Type of ergogenic aid (i.e. nutritional and mechanical)
2. Type of placebo (e.g. caffeine, cold-water immersion or kinesiology tape)
3. Mechanism (i.e. positive expectation, negative expectation or classical conditioning)

Where possible, data are reported as means, standard deviations and 95% CI confidence intervals.

Results

Description of studies

The database search identified 4,026 potential studies, with a further 8 identified through other resources (e.g. reference lists of articles). On closer inspection, 3,914 did not specifically measure the placebo effect, 74 were duplicate publications and 4 sampled non-healthy participants (e.g. participants with ankle instability or Parkinson's disease). Thus, 42 studies were assessed for eligibility. After reading each study in full, three were excluded as they did not meet the inclusion criteria regarding control condition (Corsi & Colloca, 2017; Higgins & Shabir, 2016; Reeser et al., 2005), two used non-healthy participants (Benedetti et al., 2003; Sawkins, Refshauge, Kilbreath, & Raymond, 2007), two used placebos that were distinguishable from the experimental treatment (Janes et al., 2016; Sabino-Carvalho et al., 2017), two did not report the results of the performance outcome (Benedetti, Durando, Giudetti, Pampallona, & Vighetti, 2015; Broelz et al., 2019) and one failed to deceive participants (Saunders et al., 2010). The final analyses therefore included 32 studies (table 1). Figure 1 provides a flow chart of study selection.

Of these studies, 20 used a between participant design and 12 used a within participant design. Six studies used a within-balanced placebo design.[1] Twenty investigated placebo effects of nutritional ergogenic aids and twelve mechanical (table 2). Five studies aimed to investigate the nocebo effect on performance (Andani, Tinazzi, Corsi, & Fiorio, 2015; Beedie, Coleman, & Foad, 2007; Bottoms, Buscombe, & Nicholettos, 2014; Hurst et al., 2017; Pollo, Carlino, Vase, & Benedetti, 2012), four studies administered a placebo and told participants it was a placebo (Bellinger & Minahan, 2016; Duncan, 2010; Duncan, Lyons, & Hankey, 2009; Foad, Beedie, & Coleman, 2008) and five studies used a preconditioning procedure in which a placebo is administered after surreptitiously augmenting the feedback of a previous performance (Andani et al., 2015; Fiorio, Emadi Andani, Marotta, Classen, & Tinazzi, 2014; Pollo, Carlino, & Benedetti, 2008; Pollo et al., 2012).

Studies were published between 1972 and 2018. Healthy participants were typically included (n = 13), with sub-elite athletes (n = 12), university students (n = 6) and elite athletes (n = 1) also sampled.

[1] The balanced placebo design uses four conditions; inform no-treatment/receive no-treatment; inform treatment/receive no-treatment; inform no-treatment/receive treatment; and inform treatment/receive treatment.

Sample sizes ranged from 6 to 712 (mean ± SD: 47 ± 123), with the majority of participants being male (68.5%). One study included over 100 (n = 712) participants (Hurst et al., 2017), four reported between 50 and 100 (range n = 51 to 70) participants (de la Vega, Alberti, Ruiz-Barquin, Soos, & Szabo, 2017; Fiorio et al., 2014; Pollo et al., 2012; Villa-Sánchez, Emadi Andani, & Fiorio, 2018) and 29 studies were conducted with 50 or fewer (range n = 6 to 42) participants. Overall, there were 1,513 participants.

Risk of bias in included studies

The methodological quality of the studies included in the review was generally poor (figure 2). Four of the 32 (12%) trials fulfilled all criteria and were judged as presenting a low risk of bias (Beedie, Stuart, Coleman, & Foad, 2006; Duncan, 2010; Saunders et al., 2017; Urroz, Colagiuri, Smith, Yeung, & Cheema, 2016). Randomisation to treatment was clearly used in 21 studies (66%) and allocation concealment was clear in 8 studies (25%). Eighteen studies (56%) used a manipulation check to confirm that the deception of the placebo treatment was successful. Dropouts in included studies were rare, with 28 studies reporting no dropouts (87%) and 31 of the 32 studies were free of selective reporting (97%). Other types of biases were present in 6 studies (20%). It should be noted that five studies (16%) included were conducted by the authors of this review (Beedie et al., 2007; Beedie et al., 2006; de la Vega et al., 2017; Foad et al., 2008; Hurst et al., 2017).

Preliminary and descriptive results

Nutritional ergogenic aids

In eight studies the placebo effect of caffeine on performance was investigated (Beedie et al., 2006; Duncan, 2010; Duncan et al., 2009; Pires et al., 2018; Pollo et al., 2008; Saunders et al., 2017; Tallis et al., 2016). Beedie et al. (2006) reported that the belief that 4.5mg·kg^{-1} and 9.0mg·kg^{-1} of caffeine had been received improved cycling power output by 1.3% (*d* = 0.08) and 3.1% (*d* = 0.21), respectively, whereas the belief that a placebo was received impaired performance by -1.4% (*d* = 0.8). Duncan et al. (2009) found that the total weight lifted during a leg extension task was improved by 23.8% (*d* = 0.93) when participants received a placebo believed to be caffeine. Pires et al. (2018) reported that after the ingestion of a placebo believed to be caffeine, participants' cycling peak power output improved by 10.4% (*d* = 0.88). Saunders et al. (2017) reported that placebo caffeine improved cycling power output by on average 1.0% (*d* = 0.07). Authors also found that participants' belief about which treatment they received (i.e. "caffeine," "do not know," "placebo") moderated the placebo effect. Those that believed they had received a placebo showed impairments in performance of -1.4% (*d* = 0.08), whereas those that did not know what they received and those believing they received caffeine improved by 2.4% (*d* = 0.14) and 3.5% (*d* = 0.27), respectively. Pollo et al. (2008) reported that placebo caffeine improved the amount of weight lifted during a leg extension task by 11.4% (*d* = 0.29). In the same study, the authors used a deceptive preconditioning procedure, whereby the administration of placebo caffeine was coupled with surreptitiously reducing the amount of weight lifted, giving participants the impression that the task had become easier with the administration of the treatment. In a follow up trial, and with the weight returned to normal, authors reported that placebo caffeine improved the total weight lifted by 25.9% (*d* = 0.82).

Three studies used a balanced-placebo design to investigate the placebo effect of caffeine on performance. Compared to baseline, Foad et al. (2008) reported improvements in power output during 40-km cycling time-trials, during the overt (2.3%, *d* = 0.28) and hidden (2.9%, *d* = 0.34) administration of caffeine. Authors reported no changes in performance when participants received placebo believed to be caffeine (0.1%, *d* = 0.05) but decreases in performance when participants received placebo believed to be placebo (-1.9%, *d* = 0.13). Duncan (2010) reported improvements in mean power output during a 30-second Wingate test for the overt (24.5%, *d* = 1.55) and hidden (3.2%, *d* = 0.20) administration of caffeine. Improvements were also reported when participants

received placebo believed to be caffeine (6.5%, *d* = 0.41). Similarly, Tallis et al. (2016) found that the overt administration of caffeine and deceptively hidden administration of placebo (i.e. administering a placebo but informing the participant it is caffeine) improved maximal voluntary concentric contractile force of the knee extensor muscles by 15.8% (*d* = 0.60) and 11.2% (*d* = 0.40), respectively. Nevertheless, no significant improvements were reported during the hidden administration of caffeine (6.2% *d* = 0.25).

Using a within-participant balanced-placebo design, Bellinger and Minahan (2016) reported improvements in 1-km cycling power output compared to baseline following consumption of β-alanine, irrespective of whether it was described as a performance enhancing supplement (2.4%, *d* = 0.14) or placebo (1.8%, *d* = 0.10). Minimal differences in performance were reported when participants received a placebo described as β-alanine (0.6%, *d* = 0.06) or placebo (-1.0%, *d* = 0.04). McClung and Collins (2007) also used a balanced-placebo design to investigate the placebo and true effect of sodium bicarbonate on 1000-m running time-trial performance. Authors reported that the overt administration of sodium bicarbonate and the expectation of receiving sodium bicarbonate improved performance by 1.7% (*d* = 0.14) and 1.5% (*d* = 0.13), respectively. However, when participants received sodium bicarbonate but expected a placebo, performance decreased by -0.3% (*d* = 0.03).

In a first study to explicitly investigate the nocebo effect on sports performance, Beedie et al. (2007) reported significant decreases in 30-m running speed after the administration of a fictitious supplement described as beneficial to endurance but detrimental to speed (-1.7%, *d* = 0.41). Authors also reported significant placebo effects in running speed following the administration of an inert capsule described as beneficial. While no change in mean speed was reported compared to baseline (0.0%, *d* = 0.0), the authors suggested that the maintenance of speed over six consecutive trials was indicative of a placebo effect when compared to the negative belief group. Similarly, Bottoms et al. (2014) investigated both the placebo and nocebo effect on peak minute power during incremental arm crank ergometry and found a significant placebo effect on performance (6.3%, d = 0.37), but no nocebo effect compared to baseline (-0.8%, *d* = 0.06). Hurst et al. (2017) investigated the placebo and nocebo effect of a fictitious sport supplement on repeat sprint performance with the inclusion of a no-treatment control group. Authors found that compared to no treatment controls, performance decreased when participants received a placebo that was purported to be harmful to performance (-0.9%, *d* = 0.32), but performance did not change when participants received a placebo that was purported to be beneficial (-0.1%, *d* = 0.02).

In another study investigating placebo effects on sprint performance, de la Vega et al. (2017) reported the influence of a fictitious supplement on 200-m sprint performance and randomised participants into three groups. Group 1 participants were told the supplement would improve performance, Group 2 were told it may or may not improve performance and Group 3 were told it would not affect performance. Compared to baseline, sprint times in Group 1 were significantly faster (5.9%, *d* = 0.28), whereas in Group 2 and Group 3, sprint times were similar (Group 2 = 2.2% *d* = 0.09 and Group 3 = 1.9%, *d* = 0.11). Tolusso, Laurent, Fullenkamp, and Tobar (2015) also reported significant improvements in participants' ability to recover from sprinting compared to baseline when runners believed they had received a new performance-enhancing substance (5.2%, *d* = 0.41).

Kalasountas, Reed, and Fitzpatrick (2007) reported significant improvements in weightlifting performance following consumption of a placebo described as amino acids (11.0%, *d* = 0.36). Hulston and Jeukendrup (2009) found no placebo effect during 60-min cycling time-trial after ingestion of carbohydrate (0.5%, *d* = 0.05). Ariel and Saville (1972) and Maganaris, Collins, and Sharp (2000) reported significant improvements in weightlifting performance when participants believed they had ingested anabolic steroids (9.6%, *d* = 2.15 and 4.6%, *d* = 0.72, respectively) and Ross, Gill, Cronin, and Malcata (2015) reported significant improvements in 3000-m running time when participants self-

injected saline, which they believed was a substance similar to recombinant erythropoietin (1.5%, *d* = 0.81).

Mechanical ergogenic aids

Four studies investigated the placebo effect of transcutaneous electrical nerve stimulation (TENS) on performance (Andani et al., 2015; Fiorio et al., 2014; Pollo et al., 2002; Villa-Sánchez et al., 2018). Fiorio et al. (2014) investigated the placebo effect of TENS on peak abduction force produced by the right index finger. Performance improved by 14.4% (*d* = 1.05) when participants believed they had received TENS. Similar performance improvements were shown after a preconditioning procedure in which the force produced was surreptitiously increased (13.8%, *d* = 1.11). In a follow up study, and using a similar experimental design, Andani et al. (2015) investigated the nocebo effect on force production of the right index finger. Compared to no-treatment controls, performance decreased by 12.9% (*d* = 0.96) following a preconditioning procedure in which the force produced was surreptitiously decreased. Pollo et al. (2012) investigated the nocebo effect of TENS on leg extension in two separate experiments. In the first experiment, performance reduced by 11.2% (*d* = 0.67) when participants believed they had received a harmful treatment. In a second experiment, authors reported a reduction in performance of 8.5% (*d* = 0.52) after a preconditioning nocebo-inducing procedure. Villa-Sánchez et al. (2018) also investigated the placebo effect of TENS in two separate experiments. In the first experiment, maximal force production of the right index finger improved by 2.4% (*d* = 1.22) after participants were administered TENS and informed that it would benefit performance. In the second experiment, maximal force improved by 4.3% (*d* = 0.93) compared to baseline after a surreptitious preconditioning procedure.

In three experiments, Cheung and colleagues investigated the placebo effect of Kinesiology tape on peak force of quadriceps (Poon et al., 2015), maximal vertical jump height (Cheung et al., 2016) and maximal handgrip strength (Cai, Au, An, & Cheung, 2016). All studies used a similar within-participant design and compared Kinesiology tape to placebo and no-treatment control. All studies reported no changes in performance for Kinesiology tape compared to controls, (range = 1.46 to 4.68%, *d* range = 0.04 to 0.08) or placebo (range = 0.6 to 2.55%, *d* range = 0.02 to 0.05).

In two studies, the placebo effect of ischemic preconditioning on swimming performance was investigated (Ferreira et al., 2016; Marocolo, da Mota, Pelegrini, & Appell Coriolano, 2015). Marocolo et al. (2015) reported improvements in performance after ischemic preconditioning compared to baseline (1.5%, *d* = 0.24) and placebo (0.9%, *d* = 0.16), whereas Ferreira et al. (2016) reported improvements in performance after ischemic preconditioning (1.2%, *d* = 0.54) but not for placebo (0.01%, *d* = 0.04).

Guillot, Genevois, Desliens, Saieb, and Rogowski (2012) investigated the placebo effect on tennis serve velocity. After baseline trials of fastest tennis serve, participants were given a racket that was suggested to enhance serve performance. Compared to baseline, participants' tennis serve velocity improved by 5.65% (*d* = 0.36). Broatch, Petersen, and Bishop (2014) examined the placebo effect of cold-water immersion on maximal voluntary contraction following high-intensity exercise. Compared to controls, authors reported a significant improvement in performance when participants received cold-water immersion (10.6%, *d* = 0.39) and placebo (7.2%, *d* = 0.11).

Using a within participant balanced-placebo design, Brazier, Sinclair, and Bottoms (2014) investigated the placebo effect of magnetic wristbands on 5-m sprint performance. Compared to a control treatment in which no wristband was worn, authors reported no differences in any of the three treatments: told wristband/given wristband (-0.4%, *d* = 0.02), told wristband/given placebo (-2.7%, *d* = 0.11), and told placebo/given wristband (-7.2%, *d* = 0.21).

Primary results

Thirty-two studies with 1,513 participants investigated placebo effects on sport performance. Pooled effect sizes revealed a small to moderate effect size across all studies (d = 0.37 ± 0.42, 95% CI = 0.25 to 0.49).

Moderators of placebo effects

Type of placebo

Small to moderate placebo effects were found for nutritional (n = 1099, d = 0.35 ± 0.44, 95% CI = 0.20 to 0.51), and mechanical (n = 414, d = 0.47 ± 0.42, 95% CI = 0.25 to 0.68) ergogenic aids. Placebo effects of purported anabolic steroids had the largest effect on performance (n = 17, d = 1.44 ± 1.01, 95% CI = 0.03 to 2.84). Placebo effects elicited by an erythropoietin like substance (n = 15, d = 0.81) were also found to have a large effect on performance. Moderate to large effect sizes were reported for the placebo effect of TENS (n = 113, d = 0.86 ± 0.22, 95% CI = 0.70 to 1.02), while small to moderate effect sizes were reported for amino acids (n = 42, d = 0.36), caffeine (n = 149, d = 0.40 ± 0.28, 95% CI = 0.23 to 0.57) and placebo tennis rackets (n = 22, d = 0.36). The placebo effect of a fictitious sport supplement was found to have a small effect on performance (n = 836, d = 0.21 ± 0.17, 95% CI = 0.10 to 0.31), while null effects were found for cold-water immersion (n = 30, d = 0.18), sodium bicarbonate (n = 16, d = 0.13), ischemic preconditioning (n = 38, d = 0.10 ± 0.08, 95 CI = 0.00 to 0.22), carbohydrate (n = 10; d = 0.05), β-alanine (n = 8, d = 0.06), kinesiology tape (n = 93, d = 0.04 ± 0.02, 95% CI = 0.02 to 0.05) and magnetic wristbands (n = 18, d = 0.11).

Procedures and participants' belief

Preconditioning procedures, whereby a placebo is administered after surreptitiously augmenting the perceived performance on a previous performance, was found to have a large effect on performance (n = 257, d = 0.82 ± 0.18, 95% CI = 0.66 to 0.97). Small to moderate effect sizes were found for positive (n = 985, d = 0.36 ± 0.44, 95% CI = 0.21 to 0.50) and negative (n = 265, d = 0.37 ± 0.25, 95% CI = 0.12 to 0.61) expectations and null effects were found when participants received a placebo and were told they had received a placebo (n = 48, d = 0.08 ± 0.04, 95% CI = 0.04 to 0.12).

Discussion

We found a small to moderate placebo effect on sport performance when combining the results of 32 studies with a total of 1,513 participants. Equally small to moderate effect sizes were found for nutritional (n = 1099, d = 0.35) and mechanical (n = 414, d = 0.47) ergogenic aids. Larger placebo effects were found when participants were led to believe they were given banned performance enhancing ergogenic aids (anabolic steroids; n = 17, d = 1.44, EPO; n = 15, d = 0.87) and Transcutaneous Nerve Stimulation (TENS; n = 113, d = 0.86). Moderate effects were found in studies investigating placebo effects of caffeine (n = 149, d = 0.40), amino acids (n = 42, d = 0.36) and modified tennis rackets (n = 22, d = 0.36).

The magnitude of the placebo effect varies depending on the type of placebo and on how the effects are induced. Our review illustrates that using pre-conditioning procedures leads to larger placebo effects (n = 257, d = 0.82) than studies inducing positive (n = 985, d = 0.36 ± 0.44) and negative (n = 265, d = 0.37) expectations, a finding consistent with Pollo et al. (2008) who directly compared both approaches in a muscle fatigue task. This is consistent with meta-analyses in other research domains in which placebo effects induced by expectation have been reported as smaller compared to pre-conditioning situations (Forsberg, Martinussen, & Flaten, 2017; Petersen et al., 2014; Vase, Riley, & Price, 2002).

The mean effect size reported during overt administration of placebo indicated no effect on performance (n = 54, d = 0.08). However, results were mixed between studies, with some reporting

significant placebo effects (Duncan, 2010; Duncan et al., 2009) and others reporting significant nocebo effects (Foad et al., 2008). Research in other fields has shown that open-label placebos (i.e. overt administration of placebo) can significantly improve various conditions such as irritable bowel syndrome (Kaptchuk et al., 2010), lower back pain (Carvalho et al., 2016), allergic rhinitis (Schaefer, Harke, & Denke, 2016), depression (Kelley, Kaptchuk, Cusin, Lipkin, & Fava, 2012) and cancer related fatigue (Hoenemeyer, Kaptchuk, Mehta, & Fontaine, 2018). However, these studies were specifically designed to investigate the effects of open-label placebos, whereas studies included in this review often use the open-label placebo as a comparator to experimental treatments. Further studies directly investigating the effect of open-label placebos on sport performance are needed.

Strengths and weaknesses of the review

Two main strengths of our review are the large number of studies examined and the inclusion criteria for studies with objective dependent measures (e.g. cycling time-trial performance). This enabled a comprehensive assessment of the magnitude of placebo effects on sport performance, provided a solid foundation for analyses of the moderators of placebo effects (e.g. type of ergogenic aid), and of the risk of bias observed in sport studies.

Placebo effects are ethically and methodologically problematic to study, and authors are not always able to meet standards of methodological rigor than non-deceptive research. For these reasons, the methodological quality of studies included in this review was low by stated criteria. Of the 32 studies, four (12%) studies scored sufficient for all risk of bias criteria. The most common type of bias was allocation concealment, with 69% of studies reported at high risk. Participants in these studies may therefore have realised when they received the placebo and subsequently benefited from the experimental treatment. While it is appreciated that allocation concealment is practically impossible for some treatments (e.g. cold-water immersion and ischemic preconditioning), it is important for placebo researchers to ensure manipulation of perceived treatment was effective. However, 41% of studies did not satisfy this criterion. Given the poor methodological quality of the studies included in this review, the effect size estimates should thus be interpreted with caution.

The effect sizes presented here could also be underestimated because of the considerable inter-individual variability in response to the placebo treatment. Given that the proportion of participants responding to a placebo treatment varies considerably from study to study, administration of a placebo will not always elicit a placebo effect. Placebo effects can be triggered by various factors such as the interaction between participant and researcher, the environment in which the treatment is delivered, the type of placebo and participants' previous experiences. Thus, the standard deviation of the effect size can be larger than the mean effect. The results from this review, combined with previous research from other fields (Benedetti & Amanzio, 2011), underscore the inter-individual nature of the placebo effect and highlight the importance of further investigation into potential mediators and moderators of its effect on sport performance. Future research should aim to identify placebo responders and non-responders in data analyses and further explore the difference between groups on treatment outcome.

Large standard deviations can also be the result of lack of familiarisation to the performance task (Hurst & Board, 2017). Generally, performance measures require greater familiarisation before reliable data can be obtained (Hopker, Coleman, Wiles, & Galbraith, 2009). The results of this review may be limited by the fact that some studies did not familiarise participants to what they were required to do and future research should aim to familiarise participants with the performance task prior to experimental trials in order to ensure robust and reliable data is collected.

Implications for research

This review has potentially important implications for research in sport science. Findings of studies investigating the impact of nutritional and mechanical ergogenic aids on sport performance can be susceptible to placebo effects. Researchers should therefore ensure that they are adequately controlling for the placebo effect during efficacy trials, in which the performance of a treatment is examined under controlled conditions (e.g. randomised controlled trials). To achieve this, researchers should use placebos that are indistinguishable from the actual treatment, and control for participants' knowledge or correct guessing of the received treatment.

Our results also highlight that participants' prior experiences with a treatment can significantly influence the efficacy of that treatment. Participants recruited for a study may have heightened expectations and conditioned responses to a treatment, which could influence outcomes. Researchers should therefore aim to measure, analyse and report participants' experiences and expectations about a treatment during the experiment to help facilitate an accurate estimation of its efficacy.

Implications for practice

We found that placebo effects can have a significant impact on sport performance. Given that treatment effect sizes larger than $d = 0.2$ are suggested to be beneficial for athletes (Hopkins, Hawley, & Burke, 1999), the average effect size of $d = 0.38$ reported here suggests that placebo effects can be of value for an athlete's performance. While it has been advocated before that athlete support personnel (i.e. coaches, doctors, physiotherapists) should not explicitly use placebos to improve performance (Beedie et al., 2017), these results do highlight that if an athlete does not fully believe in the effectiveness of a 'real' treatment, that athlete may not fully benefit from it. On this basis, it is reasonable to suggest that athlete support personnel should endeavour to maximise the placebo effect of a legitimate treatment by engendering a positive belief in its effectiveness (c.f. Beedie, Foad, & Hurst, 2015)

Placebo effects were larger for placebos purporting to be ergogenic aids that are banned by regulatory bodies in sport (i.e. doping substances). While to date there is no data to support this it could be speculated that athletes believing that doping substances are banned for a reason (i.e. they have a significant ergogenic effect) this may reinforce the belief that they are effective. It could therefore be suggested that banning a substance in sport may have an unintended consequence and increase its performance enhancing effectiveness. Given this, if athletes are made aware of the fact that a large proportion of the benefit of a performance enhancing drug could be attributed to their belief in it rather than the actual pharmacological effect, they may be less likely to consider using it (Hurst, Kavussanu, Boardley, & Ring, 2019). National governing bodies and policy organisations aiming to reduce drug use should therefore consider educating athletes about the impact the placebo effect has on sport performance in anti-doping prevention programmes.

Conclusion

The studies reviewed here suggest that placebo effects have a small to moderate effect on sport performance. Given that in sport a small effect could influence the outcome on an event, even an athletes' sporting career, the small to moderate sizes reported here could be meaningful. This review confirms that various forms of placebos can influence sport performance and that nutritional and mechanical ergogenic aids are susceptible to the placebo effect. Larger placebo effects are reported for banned performance enhancing substances, such as anabolic steroids and EPO, and when using pre-conditioning procedures. Future research is needed that augments our understanding of the mediators and moderators of the placebo effect on sport performance.

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Table 1. Characteristics and findings of placebo effect research on sports performance

#	Authors	Year	Design	Sample size	Type of participant	Performance measure	Treatment		*d*	% change
							Informed	Received		
Nutritional and pharmacological ergogenic aids										
1	Ariel et al.,	1972	Within-participant	6	University athletes	Maximal weight lifted	Anabolic steroids	Placebo	2.15	9.6
2	Maganaris et al.,	2000	Within-participant	11	Elite athletes	Maximal weight lifted	Anabolic steroids	Placebo	0.72	4.6
3	Beedie et al.,	2006	Within-participant	6	Sub-elite athletes	10-km TT	Placebo	Placebo	0.08	-1.4
							4.5mg caffeine	Placebo	0.08	1.3
							9.0mg caffeine	Placebo	0.21	3.1
4	Kalasountas et al.,	2007	Between-participant	42	University athletes	Maximal weight lifted	Amino acids	Placebo	0.36	11
5	McClung et al.,	2007	Within-participant balanced placebo design	16	Sub-elite athletes	1000-m TT	Sodium bicarbonate	Sodium bicarbonate	0.14	1.7
							Sodium bicarbonate	Placebo	0.13	1.5
							Placebo	Sodium bicarbonate	0.03	-0.3

Table 1 cont.

#	Authors	Year	Design	Sample size	Type of participant	Performance measure	Treatment		*d*	% change
							Informed	Received		
6	Beedie et al.,	2007	Between-participant	42	Sub-elite athletes	30-m repeated sprints	Positive supplement	Placebo	0.00	0.0
							Negative supplement	Placebo	0.41	-1.7
7	Foad et al.,	2008	Within-participant balanced placebo design	14	Sub-elite athletes	40-km TT	Caffeine	Caffeine	0.28	2.3
							Caffeine	Placebo	0.05	0.1
							Placebo	Caffeine	0.34	2.9
							Placebo	Placebo	0.13	-1.9
8	Pollo et al.,	2008	Between-participant	44	University students	Leg Extension	Caffeine	Placebo	0.29	11.4
							Caffeine	Placebo and conditioning	0.82	25.9
9	Duncan et al.,	2009	Within-participant	12	Healthy participants	Leg extension	Caffeine	Placebo	0.93	23.8
10	Hulston et al.,	2009	Within-participant	10	Sub-elite athletes	60-minute TT	50% change of carbohydrate or placebo	Caffeine	1.01	11.0
								Placebo	0.05	0.5
11	Duncan	2010	Within-participant balanced placebo design	14	Healthy participants	Wingate	Caffeine	Caffeine	1.55	24.5
							Caffeine	Placebo	0.41	6.5
							Placebo	Caffeine	0.20	3.2

Table 1 cont.

#	Authors	Year	Design	Sample size	Type of participant	Performance measure	Treatment		*d*	% change
							Informed	Received		
12	Bottoms et al.,	2014	Within-participant	12	Healthy participants	Arm crank ergometer	Positive supplement	Placebo	0.37	6.3
							Negative supplement	Placebo	0.06	-0.8
13	Ross et al.,	2015	Within-participant	15	Sub-elite athletes	3000-m TT	Oxy RBX	Placebo	0.81	1.5
14	Tolusso et al.,	2015	Within-participant	10	Healthy participants	Repeated anaerobic sprint test (RAST)	New sport supplement	Placebo	0.41	5.2
15	Bellinger et al.,	2016	Within-participant balanced placebo design	8	Sub-elite athletes	1kmTT	β-alanine	β-alanine	0.14	2.4
							β-alanine	Placebo	0.06	0.6
							Placebo	β-alanine	0.10	1.8
							Placebo	Placebo	0.04	-1.0
16	Saunders et al.,	2016	Within-participant	42	Sub-elite athletes	25-minute TT	Caffeine (guessed placebo)	Placebo	-0.08	-1.4
							Caffeine (didn't know)	Placebo	0.14	2.4
							Caffeine (guessed caffeine)	Placebo	0.27	3.5
							Caffeine (overall)	Placebo	0.07	1.0

Table 1 cont.

#	Authors	Year	Design	Sample size	Type of participant	Performance measure	Treatment		*d*	% change
							Informed	Received		
17	Tallis et al.,	2016	Within-participant balanced placebo design	14	Healthy participants	Leg extension	Caffeine	Caffeine	0.60	15.8
							Caffeine	Placebo	0.25	6.2
							Placebo	Caffeine	0.40	11.2
18	de la Vega et al.,	2017	Between-participant	60	Healthy participants	200-m sprint	Beneficial supplement	Placebo	0.28	5.9
							Partially beneficial supplement	Placebo	0.09	2.2
							Neutral supplement	Placebo	0.11	1.9
19	Hurst et al.,	2017	Between-participant	712	Sub-elite athletes	20-m repeated sprints	Positive sport supplement	Placebo	0.02	0.10
							Negative sport supplement	Placebo	0.32	-0.9
20	Pires et al.,	2018	Within-participant	9	Healthy participants	Cycling maximal incremental test	Caffeine	Placebo	0.88	10.4
Mechanical ergogenic aids										
21	Guillot et al.,	2012	Between-participant	22	Sub-elite athletes	Tennis Serve	Enhanced tennis racket	Placebo	0.36	5.6

Table 1 cont.

#	Authors	Year	Design	Sample size	Type of participant	Performance measure	Treatment		*d*	% change
							Informed	Received		
22	Pollo et al.,	2012	Between-participant	70	Healthy participants	Leg extension	Transcutaneous electrical nerve stimulation	Placebo	0.67	-11.2
							Transcutaneous electrical nerve stimulation	Placebo and conditioning	0.52	-8.5
23	Brazier et al.,	2014	Within-participant balanced placebo design	18	University students	5-m sprint	Magnetic wristband	Magnetic wristband	0.02	0.4
							Magnetic wristband	Placebo	0.11	2.7
							Placebo	Magnetic wristband	0.21	7.2
24	Broatch et al.,	2014	Between-participant	30	Healthy participants	Leg extension	Cold-water immersion	Cold-water immersion	0.39	10.6
							New developed recovery oil	Placebo	0.18	7.3
25	Fiorio et al.,	2014	Between-participant	60	University students	Abduction of the right index finger	Transcutaneous electrical nerve stimulation	Placebo	0.87	14.4
							Transcutaneous electrical nerve stimulation	Placebo and conditioning	0.85	13.8

Table 1 cont.

#	Authors	Year	Design	Sample size	Type of participant	Performance measure	Treatment		*d*	% change
							Informed	Received		
26	Andani et al.,	2015	Between-participant	32	Healthy participants	Abduction of the right index finger	Transcutaneous electrical nerve stimulation	Placebo and conditioning	0.96	-12.9
27	Marocolo et al.,	2015	Between-participant	15	Sub-elite athletes	100-m front crawl TT	Ischemic preconditioning	Ischemic preconditioning	0.24	1.5
							Placebo	Placebo	0.16	0.9
28	Poon et al.,	2015	Within-participant	30	Healthy participants	Leg extension	Kinesiology tape	Kinesiology tape	0.08	2.8
							Kinesiology tape	Placebo	0.02	0.6
29	Ferreira et al.,	2016	Between-participant	23	University athletes	6 x 50-m sprints	Ischemic preconditioning	Ischemic preconditioning	0.54	1.2
							Ischemic preconditioning	Placebo	0.04	0.08

Table 1 cont.

#	Authors	Year	Design	Sample size	Type of participant	Performance measure	Treatment		*d*	% change
							Informed	Received		
30	Cai et al.,	2016	Within-participant	33	Healthy participants	Handgrip	Kinesiology tape	Kinesiology tape	0.07	4.7
							Kinesiology tape	Placebo	0.04	2.6
31	Cheung et al.,	2016	Within-participant	30	Sub-elite athletes	Standing long jump	Kinesiology tape	Kinesiology tape	0.04	1.0
							Kinesiology tape	Placebo	0.05	1.5
32	Villa-Sanchez et al.	2018	Within-participant	51	Healthy participants	Abduction of the right index finger	Transcutaneous electrical nerve stimulation	Placebo	1.22	2.4
							Transcutaneous electrical nerve stimulation	Placebo and conditioning	0.93	4.3

Table 1 caption: *Note*: *d* = Cohens *d* effect statistic, TT = Time-trial

Table 2. Types of ergogenic aids investigated in placebo effect studies

Ergogenic aid	Type	n =
Nutritional	Caffeine	8
	Fictitious supplement	5
	Anabolic steroids	2
	Amino Acids	1
	B-alanine	1
	Carbohydrate	1
	Erythropoietin	1
	Sodium bicarbonate	1
Mechanical	Transcutaneous Nerve Stimulation	4
	Kinesiology tape	3
	Ischemic preconditioning	2
	Magnetic wristband	1
	Tennis racket	1
	Cold-water immersion	1

Figure captions

Figure 1. Study flow diagram

Figure 2. Assessment of bias of all studies. Authors' judgments about the risk of bias presented as percentages across all included studies