

The Placebo Effect in Sports Performance

A Brief Review

Christopher J. Beedie and Abigail J. Foad

Canterbury Christ Church University, Canterbury, UK

Contents

Abstract	313
1. The Placebo Effect in Sport	314
2. Findings of Intervention Studies	314
2.1 Ariel and Saville (1972)	314
2.2 Maganaris et al. (2000)	318
2.3 Clark et al. (2000)	319
2.4 Foster et al. (2004)	320
2.5 Porcari et al. (2006)	320
2.6 Beedie et al. (2006)	321
2.7 Beedie et al. (2007)	321
2.8 McClung and Collins (2007)	322
2.9 Kalasountas et al. (2007)	323
2.10 Benedetti et al. (2007)	324
2.11 Pollo et al. (2008)	325
2.12 Foad et al. (2008)	325
3. Conclusion	327

Abstract

The placebo effect, with its central role in clinical trials, is acknowledged as a factor in sports medicine, although until recently little has been known about the likely magnitude and extent of the effect in any specific research setting. Even less is known about the prevalence of the effect in competitive sport. The present paper reviews 12 intervention studies in sports performance. All examine placebo effects associated with the administration of an inert substance believed by subjects to be an ergogenic aid. Placebo effects of varying magnitudes are reported in studies addressing sports from weightlifting to endurance cycling. Findings suggest that psychological variables such as motivation, expectancy and conditioning, and the interaction of these variables with physiological variables, might be significant factors in driving both positive and negative outcomes. Programmatic research involving the triangulation of data, and investigation of contextual and personality factors in the mediation of placebo responses may help to advance knowledge in this area.

The interaction of the mind and body has intrigued philosophers and scientists for centuries. Alternative models have prevailed at different times,

but current emphasis is on the unity of the two.^[1-3] With the development in medicine of interdisciplinary fields such as psychoneuroimmunology,^[4]

research is demonstrating that the effects of an individual's beliefs may in fact have some scientific basis.^[5,6] One such belief is the placebo effect, a positive outcome resulting from the belief that a beneficial treatment has been received.^[7]

The placebo effect has an interesting history, one that exemplifies McGuire's^[8] model of the life of an artefact; first it is ignored, then its presumed contaminating effects are controlled for, finally it is studied in its own right. In medicine, the placebo effect has long been acknowledged, and has been controlled for in clinical trials for over 50 years. More recently, a substantial body of research has also studied the effect directly. For both a comprehensive review of this research and an interesting theory of the effect see, respectively, Price and co-workers^[9] and Evans.^[10]

1. The Placebo Effect in Sport

Arguably mirroring the situation several decades ago in medical research, there is more speculation than hard evidence relating to the placebo effect in sport. The placebo effect has been implicated in the use of nutritional ergogenic aids,^[11] anabolic steroids,^[12] creatine-monohydrate,^[13] vitamin E,^[14] mandibular orthopaedic devices,^[15] pre-competition anatomical manipulation,^[16] sports hypnotism^[17] and pre-competition fasting,^[18] as well as in phenomena such as the runner's high.^[19] Accounts of deliberate or inadvertent use of the placebo effect by coaches or athletes have been published in autobiographical texts,^[20] training manuals^[21] and newspaper articles.^[22] Anecdotal accounts of many examples of what might legitimately be described as placebo effects in sport are presented in a comprehensive overview of extraordinary human performance,^[23] and a survey of athletes' experiences.^[24] The authors suggest that placebo effects accounted for observed effects in studies of, for example, carbohydrate feeding,^[25] respiratory training devices,^[26] cooling protocols,^[27] fructose and glucose supplementation,^[28] ice water immersion,^[29] magnetic therapy,^[30] knee surgery^[31] and super-oxygenated water.^[32]

Such data support the idea that the placebo effect impacts on sports performance, although the

empirical evidence required to move beyond speculation was in fact lacking until recently, with only one published study prior to 2000.^[33] Since then, a further 11 experimental studies have been published.^[7,32,34-42] This review focuses on the methods and findings of these studies. Data are reported as originally presented, whether in terms of statistical significance or magnitude-based inferences.^[43] An indication of the relative magnitude of effects in terms of percentage change relative to baseline or control conditions is presented in table I.

2. Findings of Intervention Studies

2.1 Ariel and Saville (1972)

In 1972, and preceding further research by almost 30 years, Ariel and Saville^[33] investigated the placebo effect of anabolic steroids. Fifteen experienced weightlifters (≈ 5 sessions/week for ≈ 2 years) were recruited to a study of the effects of the oral anabolic steroid methandrostenedione. Baseline maximal strength data were collected for four tasks: bench press, military press, seated press and squat. Subjects were informed that they would receive methandrostenedione 10 mg/day for the duration of the study, and the likely positive effects of the drug on performance were described (a feature of placebo effect studies in sport and elsewhere is the catalysing or reinforcement of an expectation of the intervention via such a 'belief intervention'). Six subjects received an inert placebo throughout the study. Strength data for these six subjects were collected for two 4-week periods, the 'pre-placebo period' in which no intervention was administered, and the 'placebo period' in which a placebo capsule was administered. Subjects exhibited strength gains over baseline in the pre-placebo period (3.4%, 0.8%, 2.7% and 2.0% for bench press, military press, seated press and squat, respectively), and again in the placebo period (9.6%, 8.5%, 6.2% and 13.8%, respectively). Change scores between pre-placebo and placebo periods reached statistical significance at $p < 0.05$ in all but the seated press. The authors summarized by stating that significantly greater strength gains were exhibited when subjects believed that they were ingesting

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

Authors (y)	Sample size	Sample characteristics ^a	Design	Performance measure	Intervention		% change
					informed	received	
Ariel and Saville ^[33] (1972)	6	Sub-elite weightlifters	Within-subjects design	Strength (bench press, military press, seated press, squat)	Anabolic steroid	Placebo	9.5
Maganaris et al. ^[39] (2000)	11	Sub-elite weightlifters	Between-subjects design	Strength (bench press, dead lift, squat)	Anabolic steroid	Placebo	3.8
					Anabolic steroid then placebo ^b	Placebo	1.7
Clark et al. ^[7] (2000)	43	Sub-elite endurance cyclists	Between-subjects Latin-square design (6-cell)	Endurance (40 km cycling power)	Carbohydrate	Placebo (50% of subjects), carbohydrate (50% of subjects)	4.3
					Placebo	Placebo (50% of subjects) carbohydrate (50% of subjects)	0.5
					50/50 chance of receiving carbohydrate or placebo	Placebo (50% of subjects), carbohydrate (50% of subjects)	-1.1
						Overall placebo effect	3.8
Foster et al. ^[37] (2004)	16	Sub-elite runners	Within-subjects design	Endurance (5 km running time)	New ergogenic aid	Placebo	1.1
Porcari et al. ^[32] (2006)	32	Sub-elite runners	Between-subjects design	Endurance (5 km running time)	Super-oxygenated water	Placebo	8.0

Continued next page

Table I. Contd

Authors (y)	Sample size	Sample characteristics ^a	Design	Performance measure	Intervention		% change
					informed	received	
Beedie et al. ^[34] (2006)	6	Sub-elite cyclists	Within-subjects design	Endurance (10 km cycling power)	0 mg/kg caffeine	Placebo	-1.4
					4.5 mg/kg caffeine	Placebo	1.3
					9.0 mg/kg caffeine	Placebo	3.1
						Overall placebo effect	2.2
McClung and Collins ^[40] (2007)	16	Sub-elite endurance athletes	Within-subjects Latin square/ balanced placebo design (4-cell)	Endurance (1000 m running time)	Sodium bicarbonate	Sodium bicarbonate	1.7
					Sodium bicarbonate	Placebo	1.5
					No treatment	Sodium bicarbonate	-0.3
					No treatment	No treatment	0.0
						Overall placebo effect	1.8
Beedie et al. ^[35] (2007)	43	Sub-elite athletes	Between-subjects design	Anaerobic (30 m running speed)	Positive ergogenic aid	Placebo	0.0
					Negative ergogenic aid	Placebo	-1.6
Kalasountas et al. ^[38] (2007)	42	Untrained students	Between-subjects design	Strength (bench press, seated leg press)	Amino acids	Placebo	19.6
				Strength (bench press, seated leg press)	Amino acids then placebo ^b	Placebo	6.3

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Table I. Contd

Authors (y)	Sample size	Sample characteristics ^a	Design	Performance measure	Intervention		% change
					informed	received	
Benedetti et al. ^[41] (2007)	40	Sub-elite athletes	Mixed design	Pain tolerance	No treatment	No treatment	7.5
					Morphine	Placebo	17.6
					Morphine (after conditioning procedure)	Placebo	50.7
					Morphine (after conditioning procedure)	Naloxone	6.2
Pollo et al. ^[42] (2008)	44	Sub-elite athletes	Mixed design	Strength (leg extension)	Caffeine	Placebo	11.8
					Caffeine (after conditioning procedure)	Placebo	22.1
				Perceived fatigue	Caffeine	Placebo	−0.3
					Caffeine (after conditioning procedure)	Placebo	−7.8
Foad et al. ^[36] (2008)	14	Sub-elite cyclists	Within-subjects Latin-square/balanced placebo design (4 cell)	Endurance (40 km cycling power)	Caffeine	Caffeine	2.3
					Caffeine	Placebo	0.1
					No treatment	Caffeine	2.9
					No treatment	No treatment	−1.9
						Overall placebo effect	0.7

a Subject classifications are derived from their descriptions in the original papers: untrained=no regular training; sub-elite=regularly training but not above national status; elite=international status.

b Subjects were initially informed that they were receiving the drug. Halfway through the trials, subjects were correctly informed that they were receiving a placebo.

methandrostenolone than when they believed they were not. No inferences regarding the mechanisms underlying the increases in performance during the placebo period – e.g. increased effort or motivation – were offered. They concluded that future investigators must be cautious when assessing the effects of ergogenic aids on performance because the assumption that the dependent measure has been isolated may well be erroneous. This may be the case, although Ariel and Saville^[33] did not test this assumption on subjects blind to treatment – the design most commonly employed in ergogenics research.

The effects reported by Ariel and Saville^[33] were substantial and, in all but one comparison, statistically significant. These data are surprising given the relatively small improvement that would be expected from subjects reported to be highly trained and experienced weightlifters. Expectation of the likely effects of methandrostenolone might have been high, and these might have resulted in increased motivation and perhaps a greater than usual vigilance to training and recovery. Such responses, although behavioural, satisfy the definition of a placebo effect above. They might also have augmented more apparently physiological placebo responses of the type reported elsewhere,^[9,10,34] such as enhanced pain tolerance and fatigue resistance. It is possible that subjects did not perform to volitional maximum at baseline (an issue discussed below), and on this basis the degree to which the observed effects were driven by the experimental manipulation could be questioned. It is unfortunate that data for experimental subjects who *did* receive methandrostenolone were not reported, as this would have facilitated direct comparison of the magnitude of placebo and drug effects. Furthermore, given the improvements observed in the pre-placebo period, a control group for the placebo period was warranted.

2.2 Maganaris et al. (2000)

The work of Ariel and Saville^[33] is frequently cited in the strength and conditioning, weightlifting and body-building media. While studies were subsequently published in exercise^[44] and

sports-related surgery,^[31] it was over 25 years before another empirical study of the placebo effect on sports performance was published. Maganaris et al.^[39] investigated the deceptive administration of a placebo anabolic steroid among 11 national-level power lifters. The authors used a more complex design than had Ariel and Saville^[33] and proposed two hypotheses: firstly, that subjects would show substantial increases in performance, and secondly, that when the deception was revealed, performance would return to baseline. Baseline data were collected in competitive conditions for the bench press, dead lift and squat. One week later, and prior to the first experimental trial, subjects were administered the placebo but informed that they were receiving a fast-acting anabolic steroid. Mean percentage improvements in maximal weight lifted over baseline were 3.5%, 4.2% and 5.2% for the bench press, dead lift and squat, respectively ($p < 0.01$). Subjects were given another dose to take during the following week. One week later, and prior to a second experimental trial, all subjects reported improved training performance since taking the tablets. At this stage, however, six subjects were correctly informed of the experimental deception. In the second experimental trial, while improvements over baseline were maintained in the group who believed they had ingested steroids (3.2%, 4.0% and 4.4%, respectively; $p < 0.01$), performances of the six subjects correctly informed of the deception were reduced significantly (1.7%, -0.4% and 0.4%, respectively relative to baseline). In fact the authors described the performance of the second group as having returned to baseline.

Maganaris et al.^[39] discussed how subjects had “picked up on the street reputation of anabolic steroids” (p. 277) and that the expectancy driven by this reputation generated substantial improvements in performance that were reversed once this expectancy was removed. The authors suggested that evidence indicative of a substantial placebo effect associated with a treatment widely considered to be ergogenic could be used in anti-doping initiatives. They added that their investigation might have been more convincing had they used a Latin-square design in which steroids

had been administered alongside the placebo. The Latin square design, also referred to as the balanced placebo design,^[45] commonly comprises four conditions: (i) inform drug/receive drug; (ii) inform drug/receive placebo; (iii) inform no-treatment/receive drug; and (iv) inform placebo/receive placebo. This design facilitates assessment of the independent effects of placebo and pharmacology, and their interactions, and would have enabled Maganaris et al.^[39] to better assess both the placebo and pharmacological effects of steroid supplementation.

While the investigations of Ariel and Saville^[33] and Maganaris et al.^[39] were arguably limited in terms of sample size – and in the former, the lack of a suitable control group with which to differentiate ‘pure’ placebo effects from other factors – the effects were substantial for weightlifters. In fact Maganaris et al. stated that all but one subject would have gained international status as the result of the intervention. The authors of both studies suggested that subjects in strength-based sports expect certain drugs to enhance performance, and thus a component of such enhancements may be placebo driven.

2.3 Clark et al. (2000)

In the same year as Maganaris et al.^[39] published their findings, Clark et al.^[7] published an investigation of the placebo and real effects of carbohydrate supplementation on 40 km cycling performance among 41 male and two female competitive cyclists. The authors used a somewhat more complex balanced repeated-measures design than had previous studies. Also unlike previous studies, the authors administered an active substance alongside the placebo. To encourage positive beliefs about the intervention, subjects were advised that those who received carbohydrate would probably show an improvement in performance compared with those who received the placebo. Baseline data were collected, and then 1 week later subjects performed an experimental time trial during which they consumed a drink containing non-caloric sweetener either with or without carbohydrate. Subjects were randomized to one of three treatment

groups: (i) informed carbohydrate; (ii) informed non-caloric sweetener; and (iii) informed 50/50 chance of receiving carbohydrate. Without their knowledge, half of the subjects in each group were further randomized to receive carbohydrate, while the others received placebo. The experimenters were thus able to analyse the effects of six different combinations of placebo and nutritional supplementation.

Informed carbohydrate subjects showed an improvement in power over baseline ($4.3 \pm 4.8\%$); this improvement was, surprisingly, greater for those who received the placebo than for those who received the carbohydrate. Informed 50/50 subjects showed little change in performance ($-1.1 \pm 8.5\%$) compared with informed placebo subjects ($0.5 \pm 5.8\%$), irrespective of the actual substance administered. The authors estimated that the placebo effect, calculated as the change for the informed carbohydrate group minus the change for the informed placebo group, was 3.8% (7.9 to -0.2% ; $p=0.06$), and that the real effect of carbohydrate, calculated as change in power for carbohydrate minus the change in power for placebo, was a slight reduction in power of 0.3% (4.4 to -3.8% ; $p=0.87$). The coefficient of variation for the informed 50/50 group was 1.6 times larger than the combined coefficients of variation of the other two groups. The authors speculated that uncertainty about the treatment caused some subjects to make a greater effort than at baseline, whereas others resigned themselves to poorer performance.

Clark et al.^[7] argued that the existence of a placebo effect with a sham treatment, and the existence of individual differences with a blind treatment, both imply that at least some subjects do not perform at volitional maximum in performance research. They speculated further that, if in competition these athletes perform at a higher percentage of volitional maximum, the effect of a treatment in a laboratory test might be substantially different from the effect of the same treatment in competition. They suggested that a treatment might operate in the zone between submaximal and maximal effort in a performance test, a margin that may be substantially reduced or even absent in competition. The authors, as

had Ariel and Saville^[33] before them, advised caution in extrapolating enhancements observed in the laboratory to the real world.

Clark et al.^[7] made several methodological recommendations in relation to placebo effect research in sport, for example the use of Latin-square designs and assessment of personality towards better understanding placebo mechanisms. While the first of these recommendations has been heeded by several investigators,^[36,40] the role of personality in placebo responses has only just begun to be explored,^[46] and remains a promising avenue for future research.

2.4 Foster et al. (2004)

In 2004, Foster et al.^[37] investigated the effects of a placebo treatment on 5 km running performance. The study was presented at a conference and it has not been published in full text in a peer-reviewed journal. Sixteen well trained and task-habituated recreational runners ($\dot{V}O_{2\text{peak}} = 58 \pm 8 \text{ mL/kg/min}$) were recruited to a study of a 'new' ergogenic aid. The authors showed subjects a video designed to promote the value of the substance in endurance performance. Subjects performed random-ordered 5 km time trials after consuming either normal water or water falsely purporting to contain the ergogenic aid. Measures included total time, lap times, ratings of perceived exertion (RPE), heart rate and blood lactate. Competitively meaningful differences were observed in 5-km time trial performance between the control and placebo water conditions (21:54 vs 21:40; $p=0.11$), with 12 of the 16 subjects running faster when they believed they had ingested the ergogenic aid. The authors also noted a competitively meaningful mean 2.5-second improvement in performance over the final 400 m when subjects believed they had ingested the ergogenic aid. No significant differences were observed between conditions in RPE (8.2 ± 1.0 vs 8.4 ± 1.2), peak heart rate (177 ± 5 vs 177 ± 6 beats/min) or blood lactate (12.2 ± 3.2 vs $11.4 \pm 2.2 \text{ mmol/L}$). The authors concluded that while they were not statistically significant, the pattern of observed effects was clear enough to warrant further research.

2.5 Porcari et al. (2006)

In a follow-up conference abstract, Porcari and co-workers^[32] reported the findings of an investigation of the placebo effect in 5 km running performance. This study expanded on a previous paper^[47] that reported no differences between 'super-oxygenated' water and placebo in several variables (e.g. heart rate, blood lactate, ratings of perceived exertion) associated with performance. Thirty-two experienced runners ranging from recreational to competitive completed an exercise test to measure fitness level ($\dot{V}O_{2\text{max}} = 60.8 \pm 8.2 \text{ mL/kg}$). Subjects then watched a video suggesting the ergogenic qualities of super-oxygenated water. Each subject subsequently ran three 5 km time trials on an indoor 200 m track (1 × habituation and 2 × counterbalanced experimental). Runs were completed at least 3 days apart. Prior to experimental trials, subjects drank either 475 mL of bottled water that was correctly identified as such, or water that they were told was super-oxygenated. Measures were total time, heart rate, RPE and blood lactate. Significant differences between control (water) and experimental (placebo) trials for total time ($21:04 \pm 3:34$ vs $19:41 \pm 2:32$) were reported. No significant differences in heart rate (data not presented), RPE (7.7 ± 1.4 vs 7.7 ± 1.2) or lactate (9.8 ± 3.9 vs $10.2 \pm 3.7 \text{ mmol/L}$) were reported. Twenty-seven of 32 subjects (84%) ran faster when they believed they had received the super-oxygenated water.

The authors reported that the observed improvement over baseline in the experimental conditions was largely attributable to the performances of less accomplished runners (2:22 minutes as opposed to 0:28 minutes for the more accomplished runners). In describing the study on the American Council for Exercise website,^[48] one of the authors, Otto, reported that several of the less accomplished subjects claimed that they 'felt lighter on their feet' and wanted to know where they could buy the product, while more experienced runners asserted that they didn't feel any different after the run and 'didn't think that stuff works'. This hint of a relationship between status and placebo responsiveness has been alluded to elsewhere.^[7,34]

2.6 Beedie et al. (2006)

In 2006, Beedie et al.^[34] examined the possibility of a dose-response relationship to placebos presented as 'zero-', 'low-' and 'high'-dose caffeine among seven well-trained competitive cyclists. Measures in the first phase of the study were power, heart rate, oxygen uptake and blood lactate. Subsequently, qualitative data were derived through interview.

Subjects were provided with literature reviewing research findings in caffeine and cycling performance and detailing anecdotal evidence of the use of caffeine amongst elite cyclists. Following habituation and baseline trials, subjects were informed that, over three experimental trials, they would receive a placebo, caffeine 4.5 and 9.0 mg/kg double-blind and randomly assigned. However, a placebo was administered in all experimental conditions. Post-experimental baseline trials were also conducted. One subject failed to complete one trial and was excluded from further statistical analysis.

The authors reported their findings in terms of magnitude-based inference.^[43] A likely trivial increase in mean power of 1.0% (−1.4% to 3.6%) over baseline was associated with experimental trials, rising to a likely beneficial 2.2% (−0.8% to 5.4%) increase in power associated with experimental trials in which subjects believed they had ingested caffeine. A dose-response relationship was evident in experimental trials, with subjects producing 1.4% (−4.6% to 1.9%) less power than at baseline when they believed they had ingested a placebo, 1.3% (−1.4% to 4.1%) more power than at baseline when they believed they had ingested caffeine 4.5 mg/kg, and 3.1% (0.4–6.7%) more power than at baseline when they believed they had ingested caffeine 9.0 mg/kg. The authors concluded that when subjects were administered a placebo capsule believing it to be caffeine, their performance was substantially enhanced. They noted that the effects were similar in magnitude to those associated with the administration of caffeine reported elsewhere. Of further interest was the fact that no substantial differences in any measured physiological variables between baseline and experimental conditions were observed,

suggesting that the mechanism underlying the observed effects was not a substantial change in effort.

Wishing to investigate the potential mechanisms of the observed effects, Beedie et al.^[34] conducted follow-up interviews with each subject. Interviews were conducted in two parts, the first before revealing the experimental deception to the subject, the second afterwards. Interview data were reported for all seven original subjects. Data were consistent with the performance of some subjects but less so with others: five subjects believed that they had experienced a placebo effect in one or more of the three experimental trials, and proposed mechanisms such as pain reduction, fatigue resistance, changes in strategy and reduced arousal. One subject reported experiencing a substantial negative effect on performance that he attributed to the high dose of caffeine. The two subjects who reported the least confidence in having experienced a placebo effect produced the highest mean power overall, and the subject who produced the lowest mean power overall reported arguably the largest and least ambiguous placebo effect. The authors suggested that the findings supported the previously proposed relationship between training status and placebo responsiveness.^[7,32]

2.7 Beedie et al. (2007)

Having observed potentially negative placebo (nocebo) effects associated with the administration of caffeine,^[34] Beedie et al.^[35] designed a study to investigate whether, following ingestion of a placebo ergogenic aid, subjects who possessed positive beliefs about the substance would perform to a higher level than subjects who had negative beliefs about the substance. Forty-two team sports athletes were randomly allocated to one of two groups, positive belief and negative belief. Both groups were informed that they would be completing a 30 m repeat-sprint protocol in two conditions, baseline and experimental. They were further informed that prior to the experimental condition they would be administered a new ergogenic aid. To minimize the potential

for experimenter effects, a highly experienced researcher not associated with the original research project was responsible for delivering the intervention and managing the data collection process. The two groups were also isolated from one another to eliminate the possibility of any spillover.

Subjects performed 3×30 m sprints with 2 minutes recovery between each. They were then administered a placebo capsule. Each group was provided with a different description of the placebo: the positive belief group that the substance had been found to enhance both repeat sprint and endurance performance in team sport players, the negative belief group that the substance had been found to enhance endurance performance while having a *negative* impact on repeat sprint performance. Twenty minutes subsequent to the intervention, subjects undertook experimental trials in an identical manner to the baseline trials.

The speed of both groups diminished progressively in successive baseline trials. In experimental trials, however, while the trend towards reduced speed in consecutive trials continued for the negative belief group, mean speed per trial for the positive belief group increased. Although no change in mean speed from baseline to experimental trials was evident for the positive belief group ($p=0.96$), a significant linear trend of greater speed with each successive experimental trial suggested that positive belief exerted a positive impact on performance ($p<0.01$). Data for the negative belief group indicated that they ran on average 0.08 seconds (1.7%) slower than baseline in experimental trials ($p=0.01$). Furthermore, mean delta score (baseline to experimental) for the positive belief group was 0.00 (standard deviation [SD]=0.09), while that for the negative belief group it was 0.09 (SD=0.10) and differed significantly ($p=0.01$). The authors concluded that both positive and negative beliefs were associated with placebo effects of opposite polarity that significantly affected performance.

Beedie et al.^[35] also investigated inter-individual variability in the placebo response. In the positive belief group, 26% of individual performances fell outside individual 95% limits of agreement, and 50% of these were faster than the

upper limit. In the negative belief group, 33% of performances fell outside the limits of agreement, and all of these were slower than the lower limit. The authors concluded that both subjects' beliefs about whether or not they have ingested a substance and subjects' beliefs about the potential efficacy of that substance influence placebo responding. They also speculated that, if a negative belief about a placebo treatment exerts a negative impact on performance, negative beliefs about a legitimate treatment could offset some percentage of the beneficial pharmacological or physiological effects of that treatment.

2.8 McClung and Collins (2007)

In a further study of running performance, McClung and Collins^[40] used a Latin-square design to evaluate the physiological and psychological effects of sodium bicarbonate among 16 track athletes (12 men and four women). The authors reported extensive piloting prior to the study. Subjects ran 5×1000 m time trials, one habituation and one trial per counterbalanced condition of (i) informed drug/received drug, (ii) informed drug/received placebo, (iii) informed no-treatment/received drug, and (iv) informed no-treatment/received no-treatment. Measures were time, RPE, blood lactate and heart rate (no heart rate data or analyses were reported). The authors hypothesized that not only would subjects who received sodium bicarbonate run faster and report a lower RPE than in conditions in which they did not receive the drug, but that the expectation of receipt of the drug in the informed drug/received placebo condition would result in improved performance and lower RPE than in the informed no-treatment/received no-treatment condition.

McClung and Collins^[40] informed subjects that the study was to examine the effects of sodium bicarbonate and a new additive that would reduce gastric discomfort associated with sodium bicarbonate. The authors used this information to explain why, before the informed no-treatment/received sodium bicarbonate condition, subjects had to ingest a lemon-flavoured drink. That is, subjects were informed that the strong,

lemon-tasting drink was the additive, and that the trial in question was a test of the additive alone. Sodium bicarbonate was in fact deceptively administered in this solution.

Results of a 2×2 (drug $\times$ belief) ANOVA with final time as the dependent variable indicated a statistically significant main effect of belief ($p < 0.001$). No statistically significant main effect for drug, or interaction between drug and belief, was observed. Similar effects were observed with RPE as the dependent variable. In relation to lactate, although the authors reported that pre-trial lactate was significantly lower in the two experimental conditions in which sodium bicarbonate was administered than in the two in which it was not – a factor that they interpret as suggestive of the efficacy of sodium bicarbonate as a lactate buffer – they did not discuss the post-trial lactate data presented in their table, which are somewhat less easy to interpret.

McClung and Collins^[40] summarized by stating that not only does the overt administration of sodium bicarbonate improve performance by a competitively meaningful degree over no-treatment (1.7%), but that the expectation of receiving sodium bicarbonate improves performance in the absence of that substance by a not dissimilar amount (1.5%). These findings, they suggested, hint at the possibility that some of the well documented benefits of sodium bicarbonate may be gained through expectancy effects alone. The authors noted the lack of a performance effect when subjects had ingested sodium bicarbonate but believed that they had not, suggesting what they termed a biochemical ‘failure’. However, in this experimental condition, subjects believed that they had ingested the ‘new additive’ and might have suspected that this could have an effect on performance. A valid test of the biological effects of sodium bicarbonate uncontaminated by psychological factors would require that subjects believe they have received no treatment at all. Post-intervention manipulation checks suggested that four subjects were suspicious that the study was not what it appeared to be, although these subjects could not explain which aspect they were suspicious of. The authors concluded by reiterating the suggestion of Maganaris

and colleagues^[39] that evidence of a placebo effect of an ergogenic treatment provides a strong argument against the use of performance-enhancing drugs and might therefore contribute to educational anti-doping strategies.

2.9 Kalasountas et al. (2007)

Again in 2007, and citing the work of Maganaris et al.^[39] as a catalyst, Kalasountas et al.^[38] examined placebo effects on the weightlifting performance of college students. The authors tested the same hypotheses as had Maganaris et al.: (i) that subjects who received a placebo ergogenic aid would show greater increases in performance than controls; and (ii) that the performance of subjects informed that the substance is no longer effective will return to control levels. Forty-two subjects were randomly allocated to one of three groups of 14: two experimental (placebo/placebo and placebo/no-placebo) and control. Subjects in the placebo/placebo group received a placebo ergogenic aid during both experimental trials, while subjects in the placebo/no-placebo group received a placebo in the first experimental trial only. Controls did not receive the placebo in either trial. Subjects were requested not to engage in weight-training activity other than that required for the experiment and to cease use of dietary supplements for 10 days prior to and during the study.

Five trials were conducted. The first three were baseline, each trial separated by 48 hours. Subjects performed single lifts on the bench press and a seated leg press, performing one lift per minute and increasing the resistance until a repetition could not be completed with correct form. Resistance on the final completed attempt was recorded as the maximum. Experimental trials were carried out the following week and were also separated by 48 hours. Prior to the first trial, subjects in both experimental groups were given placebo tablets and informed that the substance was a combination of amino acids likely to produce immediate strength effects. Two more tablets were given 8–10 minutes after the trial. In the second experimental trial, the same process was followed for placebo/placebo subjects while

placebo/no-placebo subjects were provided with negative information about the substance and informed that no tablets would be administered. Subsequently, subjects in both experimental groups were interviewed about their beliefs regarding the effectiveness of the pill. All were then informed of the true nature of the study.

In the first experimental trial, both experimental groups improved significantly over controls on both measures ($p < 0.01$). In the second experimental trial, revealing the deception to the placebo/no-placebo group resulted in performance on both measures dropping to a level not significantly different from controls ($p > 0.05$). Performance of controls did not improve from baseline during the experimental period. The authors suggested on this basis that placebo-associated expectancy played a significant part in the observed performance. They speculated that, because participants were largely untrained, alterations in neurobiological factors, set in motion by expectancies, may offer some explanation of the underlying mechanisms.

Follow-up interviews revealed that 67% and 56% of subjects in the placebo/placebo and placebo/no-placebo groups, respectively, reported positive expectations resulting from consuming the pill, 75% and 56% reported increased vigour and energy levels after taking the pill, and 58% and 56% reported feeling better the day between experimental trials. No subjects reported feeling worse after taking the pill, although 67% of the placebo/no-placebo group reported feeling disappointed, less enthusiastic or that their performance suffered as a result of the negative news about the supplement. The authors suggested that their results generally supported those of Maganaris et al.^[39] In fact, they indicated that their use of a control group was an advance on the latter's method, although in both studies the subjects arguably acted as their own controls. Kalasountas et al.^[38] noted that their second hypothesis was only partially supported. That is, although force outputs declined on average in the placebo/no-placebo group in the second experimental trial, they did not reach baseline levels, suggesting that a placebo intervention subsequently revealed might still exert a positive

effect, or alternatively that the initial placebo intervention resulted in increased motivation in the first trial and a subsequent greater training effect carried through to the second.

Expressing a similar sentiment to Maganaris et al.^[39] and McClung and Collins,^[40] Kalasountas et al.^[38] concluded that, as corticosteroid use is on the rise among adolescents, their study could serve as a starting point for coaches and teachers in educating young persons about the risks of doping and using ineffective nutritional supplements, while encouraging them to concentrate on the psychological aspects of enhancing performance. In relation to performance interventions, the authors speculated that manipulations resulting in the positive performance outcomes in the placebo/placebo group could possibly serve as an appropriate intervention to assist amateur lifters and fitness enthusiasts though performance slumps, or as a method to demonstrate the importance of psychological factors in successful performance.

2.10 Benedetti et al. (2007)

The question of whether placebo responses could or should be used to enhance performance in training and competition was raised in two studies by Benedetti and colleagues.^[41,42] In the first of these studies, Benedetti et al.^[41] investigated the placebo analgesic effects of morphine on a pain endurance test designed to simulate sport competition. Morphine or placebo (saline or naloxone) was administered to 40 recreationally active males in a randomized, double-blind design. During pre-competition training, teams A and B received no pharmacological substance; teams C and D were trained with morphine. During competition, team A received no treatment while teams B and C were given placebo morphine 1 hour before competition. Team D also received a placebo and was told that it was morphine; however, they actually received naloxone, an opioid antagonist. Subjects had a tourniquet wrapped around their forearm and were required to repeatedly squeeze a hand spring exerciser until they could no longer continue. The time before stopping was recorded and team

averages calculated. The largest placebo effect was seen in team C who received the morphine preconditioning ($p < .001$). Naloxone negated the morphine preconditioning effects in team D indicating the activation of endogenous opioids after placebo administration. A correlation between morphine and placebo was, however, still present after naloxone treatment, suggesting the possible contribution of non-opioid mechanisms. The placebo analgesic responses were obtained after two morphine administrations that were separated as long as 1 week from each other. These long time intervals indicate that pharmacological conditioning procedures have long-lasting effects, with potentially interesting implications for the use of drugs in training and competition. That is, could the placebo response be used to enhance performance in competition, and if so, would it be ethically acceptable to do so?

2.11 Pollo et al. (2008)

In a subsequent study involving two of the previous three authors, Pollo et al.^[42] investigated the effects of an ergogenic placebo on quadriceps muscle performance and perceived fatigue. Forty-four recreationally active males were divided into four groups, two control and two placebo ($n = 11$). In the first experiment, a placebo was deceptively administered with the suggestion that it was a high dose of caffeine. This resulted in a significant increase in mean muscle work ($11.8 \pm 16.1\%$, $p < .01$) but no perceived decrease in muscle fatigue ($p > .05$). In the second experiment, placebo caffeine administration was accompanied by a conditioning procedure whereby the weight to be lifted was surreptitiously reduced. The load was then restored to the original weight and placebo caffeine administered again. Compared with the first experiment, the placebo effect was larger, with a significant increase in muscle work ($22.1 \pm 23.5\%$, $p < .01$) and a decrease in perceived muscle fatigue (-7.8 ± 10.1 , $p < .01$). These findings, the authors suggested, indicate a central mechanism of top-down modulation of the global performance of muscles by placebos, and underscore the role of learning in the placebo response.

The two studies by Benedetti and colleagues^[41,42] indicate that either pharmacological or non-pharmacological conditioning procedures can be effective in eliciting a placebo response. The authors suggested that these procedures could be employed in the field; for example, athletes could be pre-conditioned with a performance-boosting drug and then given a placebo prior to competition to avoid illegal drug administration on competition day. These studies therefore raise important and timely questions: are such procedures legally and ethically acceptable ways to enhance performance, or should they be considered as doping? As the authors noted, if such procedures were performed, many illegal drugs in sport would be neither discoverable nor would they violate the anti-doping rules.

2.12 Foad et al. (2008)

Using a design similar to the Latin squares design employed by McClung and Collins,^[40] Foad et al.^[36] used the balanced placebo design^[45] to examine the placebo and pharmacological effects of caffeine in cycling performance. Fourteen well trained competitive cyclists were informed that they were participating in a study examining the effects of caffeine on 40 km laboratory cycling performance. The authors reported piloting several aspects of the design before the experimental phase. Subjects performed two 40 km time trials in each of four experimental conditions: (i) informed caffeine/received caffeine; (ii) informed no-treatment/received caffeine; (iii) informed caffeine/received placebo; and (iv) informed no-treatment/received no-treatment. Trials were conducted once per week per subject. No feedback other than distance covered was provided to subjects during trials. Measures were power, oxygen uptake, blood lactate and heart rate. To avoid alerting subjects to potential changes in subjective symptoms during trials, the authors chose not to measure RPE, although they acknowledged this as a potential limitation. Caffeine was administered in a chilled saline solution that had been shown in a pilot study to mask the taste of caffeine. Subjects

were informed that the saline solution was administered to maintain hydration. In the two conditions in which caffeine was administered, it was administered in this solution. In the two conditions in which subjects were informed they were receiving caffeine, a placebo capsule was administered with the saline solution to maintain this belief.

The authors reported their findings in terms of magnitude-based inference.^[43] A very likely beneficial main effect on mean power of receiving caffeine ($3.5 \pm 2.0\%$), and a possibly beneficial main effect of being informed of caffeine ($0.7 \pm 1.4\%$), was observed. A substantial interaction between belief and pharmacology ($2.6 \pm 3.3\%$) indicated that caffeine exerted a greater effect on performance when subjects were informed that they had not ingested it (a similar finding to Clark et al.^[7] in relation to carbohydrate), while belief exerted a greater influence on performance in the absence of caffeine, a finding counter to the greater effect of belief in the presence of the active substance reported by McClung and Collins.^[40] A possibly harmful nocebo effect relative to baseline was present when subjects were correctly informed that they had ingested no caffeine ($-1.9 \pm 2.2\%$). No substantial changes relative to baseline were observed in mean heart rate, although clear and substantial increases in blood lactate were evident following the receipt of caffeine. Data for mean oxygen uptake were unclear.

The authors reported that the within-subject coefficient of variation (CV) for power in deceptive conditions at 2.8% was 1.7 times larger than the CV when subjects were truthfully informed that they were receiving caffeine, indicating the possibility of some disparity between internal sensations and instructions amongst some subjects. This finding adds to that of Clark et al.,^[7] who reported that the CV for their not-informed group was 1.6 times larger than for informed subjects. Both ratios suggest that either a lack of information or a disparity between information and experience might reduce the reliability of experimental trials.

In summarizing, Foad et al.^[36] suggested that their data supported the ergogenic efficacy of

caffeine and argued that such an improvement is highly likely to be worthwhile to a competitive cyclist. They suggested that, consistent with the findings of Beedie et al.,^[35] both positive and negative expectations likely impact on performance and that this effect might vary between individuals. In considering their findings in the context of previous research, the authors noted the failure to observe a clear placebo effect in the informed caffeine/received placebo condition. Certainly, as the authors argued, given that subjects produced greater power in that condition than in the informed no-treatment/received no-treatment condition, a substantial placebo effect could be inferred. Nonetheless, performance in the former condition was only marginally better than at baseline, suggesting that, in the absence of caffeine, the negative effect of negative belief on performance was somewhat more substantial than the positive effect of positive belief.

In light of their data and of previous findings, Foad et al.^[36] speculated that placebo/nocebo effects might operate somewhat differently in the presence of an active substance than in its absence. They added that, in some cases, the placebo and biological effects of a substance might share the same space, i.e. the potential to improve performance from sub-maximal to maximal, an argument made by Clark et al.^[7] and arguably supported by the effects reported by McClung and Collins.^[40] They concluded that, all other things being equal, the placebo effect observed in a study in which an active substance is administered and in which beliefs are also manipulated, might be somewhat different in magnitude to the placebo effect observed in a study in which only the beliefs are manipulated and no active substance is administered.

In discussing their findings, Foad et al.^[36] also addressed the issue of expectancy. They contrasted their design with previous research in which subjects have been unsure as to whether they would receive caffeine or a placebo. Although it has often been suggested in the literature that uncertainty about treatment allocation likely reduces the magnitude of observed effects,^[49] the authors cited recent research that suggests that a degree of uncertainty might in fact

be required to elicit a placebo effect. For example, Fiorillo and co-workers^[50] demonstrated that placebo-induced dopamine activation is maximal when the probability of experiencing a beneficial outcome is 0.5. The authors suggested that this somewhat counter-intuitive idea may lend support, in sports performance at least, to the idea suggested by Clark et al.^[7] that the placebo effect might be more of a factor in laboratory research than in the real world.

Arguably the main finding of Foad et al.^[36] was that caffeine exerted an ergogenic effect whether subjects believed it had been ingested or not. Still, the observed interactions between pharmacology and psychology are of interest. Furthermore, the finding that caffeine exerted a greater effect in trials in which subjects were falsely informed they had not ingested it than in trials in which they were correctly informed that they had is as counter-intuitive as that of Clark et al.,^[7] who reported that subjects produced more power in the informed carbohydrate/received placebo condition than in the informed carbohydrate/received carbohydrate condition. Although possibly anomalous, these findings warrant further investigation.

3. Conclusion

This review addresses 12 intervention studies from the sports literature. Of these, one was published in 1972, and the remaining 11 were published after 2000. It is evident that systematic research has been a feature of only the last few years. In six studies the dependent variable was endurance performance, in four strength performance, in one, anaerobic performance and in one, pain tolerance. Over and above performance data, several studies report physiological or psychological data. Both positive and negative placebo effects on performance were reported, with magnitudes varying from -1.9% to 50.7% of baseline/control performance, the majority falling between 1% and 5%. All but one study reported either a statistically or clinically significant effect.

Several authors have suggested potential applications of their findings. As is the case in

medicine, use of the placebo effect by practitioners might prove ethically problematical; issues of trust between practitioner and client, or between scientist and subject, should be paramount. Beyond these issues, given the evidence for nocebo responses above, the assumption that such practical application would always elicit positive results is questionable. Thus, the question as to whether placebo responsiveness, if indeed it is a generalized trait, represents a desirable or undesirable characteristic in terms of athletic personality is perhaps one of the key questions to be addressed by future research.

The placebo effect is still a little understood phenomenon. This statement is true of many sports psychological phenomena, for example flow states and emotion, although unlike such phenomena the placebo effect, given its central role in the estimation of effects in placebo-controlled studies, is fundamental to sports science research and evidence-based practice. The placebo effect also warrants further investigation as a mind-body phenomenon of interest in its own right. As is suggested both explicitly and implicitly by several authors above, the logical conclusion from any study in which an athlete performs to a higher level as the result of receiving a sham treatment is that there is untapped psychological potential in that athlete. Whatever the mechanisms underlying placebo effects in sport, it certainly seems incumbent on sports scientists to further investigate the potential for placebo effects to enhance performance. Over and above this, that some authors have reported placebo effects similar in magnitude to those reported for the drug the placebo purported to represent, suggests that there is potential for future placebo effect research to be targeted at informing anti-doping initiatives.

An opportunity to examine the placebo effect passes unused in many research environments. Incorporating a fully balanced placebo design may not always be feasible, or indeed appropriate, but by incorporating a baseline measure or non-placebo control group into a study, researchers might better elucidate both the biological and psychological effects of the intervention under examination. While this approach

is still more costly than the standard two-condition design, there is also an economy in the approach, i.e. findings might inform two domains. Given that the placebo effect has arguably transitioned from the role of artefact to that of legitimate area of study, it is possible to envisage a point in time at which stronger justification might be required for not incorporating a no-placebo condition than for incorporating one.

By comparison with placebo effect research in medicine, placebo effect research in sport is in its infancy. Potential mechanisms have only recently been addressed. Physiological data have provided few clues, and although qualitative data suggest that effects might be related to expectancy-driven changes in pain sensation, fatigue resistance and anxiety, such data are retrospective, and, even if they were collected in real time, might reflect faulty perceptual processing. There is, however, sufficient empirical evidence from sport to warrant more concerted and consistent research into the placebo effect from within the discipline. Programmatic research involving the triangulation of data to establish, clarify and elucidate the nature of placebo effects in sport could be instructive. Investigation of contextual and personality factors in the mediation of placebo responses, and a thorough exploration of designs and methodologies, may also help to advance knowledge in this area. By elucidating the nature of this effect in sports performance, research may contribute not only to an elaboration of placebo phenomena *per se*, but to a greater understanding of the mechanisms and methods for best unravelling experimental effects in sports research and understanding real world performance.

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Correspondence: Dr Christopher Beedie, Department of Sport Science, Tourism and Leisure, Canterbury Christ Church University, North Holmes Road, Canterbury, Kent, CT1 1QU, UK.

E-mail: christopher.beedie@canterbury.ac.uk