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Hurst, P., Schiphof-Godart, I., Hettinga, F., Roelands, B. and Beedie, C. Improved 1000-m running performance and pacing strategy with caffeine and placebo effect: a balanced placebo design study. *International Journal of Sports Physiology and Performance*. ISSN 1555-0265. (In Press)

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1 **Improved 1000-m running performance and pacing strategy with caffeine and placebo**
2 **effect: a balanced placebo design study**

3 **Original Investigation**

4

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17 **Running head:** Caffeine, placebo effects and pacing

18

19 **Word count:** 3532

20

21 **Number of figures and tables:** 3

22 **Abstract**

23 **Purpose:** To investigate the placebo effect of caffeine on pacing strategy and performance
24 over 1000-m running time-trials using a balanced placebo design. **Methods:** Eleven well-
25 trained male middle-distance athletes performed seven 1000-m time-trials (one
26 familiarisation, two baseline and four experimental). Experimental trials consisted of the
27 administration of four treatments: informed caffeine/received caffeine (CC), informed
28 caffeine/received placebo (CP), informed placebo/received caffeine (PC), and informed
29 placebo/received placebo (PP). Treatments were randomized. Split times were recorded at
30 200-, 400-, 600-, 800- and 1000-m and peak heart rate (HR_{peak}) and rating of perceived
31 exertion (RPE) were recorded at the completion of the trial. **Results:** Relative to baseline,
32 participants ran faster during CC ($d = 0.42$) and CP ($d = 0.43$). These changes were
33 associated with an increased pace during the first half of the trial. No differences were shown
34 in pacing or performance between baseline and the PC ($d = 0.21$) and open administration of
35 placebo ($d = 0.10$). No differences were reported between treatments for HR_{peak} ($\eta^2 = 0.084$)
36 and RPE ($\eta^2 = 0.009$). **Conclusions:** Our results indicate that the effect of believing to have
37 ingested caffeine improved performance to the same magnitude as actually receiving
38 caffeine. These improvements were associated with an increase in pace during the first half of
39 the time-trial.

40 **Key words:** belief, deception, ergogenic aids, nutrition, sport supplements

41 **Introduction**

42 The placebo effect is a desirable outcome resulting from a person's belief and/or learned
43 response to a treatment or situation.¹ Although there is considerable evidence for the effect
44 placebos can have on sports performance,² empirical evidence within sport and exercise
45 science has remained largely static in regards to the degree to which placebo effects interact
46 with the verum components of a treatment. Attempts to quantify the placebo effect in sport
47 and exercise science often rely exclusively on randomized control trials in which participants'
48 belief about the treatment they have been administered is held constant by blinding. Using
49 this type of design nevertheless does not provide sufficient information about whether there
50 are any interactions between a treatment and the belief that the treatment will influence
51 performance.³ Authors in placebo effect research^{4,5} have therefore advocated the use of the
52 four-treatment, balanced placebo design,⁶ which allows an assessment of each possible
53 combination of what the participant believes they have taken and what they have actually
54 taken.

55 To our knowledge, seven studies have used the balanced-placebo design to examine the
56 placebo effect on sport performance.^{4,5,7-11} While most studies using this design have reported
57 significant placebo effects on time-trial performance,^{5,7,9} few studies have investigated the
58 potential mechanisms related to its response. Since the mid-1990s, there has been an
59 exponential increase in the number of studies investigating the placebo effect and the
60 neurobiological pathways underlying this phenomenon.² Qualitative data suggest that placebo
61 effects may be associated with a reduction in pain sensation, arousal regulation and increases
62 in motivation,¹² which may be mediated and moderated by various neurobiological pathways,
63 such as the endogenous opioid and neurotransmitter pathways.¹³ However, while there is
64 mounting evidence of the mechanisms underpinning this phenomenon, it is unclear how
65 placebo effects affect sport performance during the actual measure itself. It reasonable to
66 suggest that after ingesting caffeine, for example, athletes may anticipate an offset in fatigue
67 and alter their exercise behaviour. Thus, athletes' pacing strategy may depend on their belief
68 regarding the effect of a substance and their subsequent decisions during performance.

69 Pacing strategies are set according to an athlete's expectation of the task they are required to
70 perform, based on previous experiences that were used to form a performance template.¹⁴
71 Numerous studies have manipulated pacing strategies through deception about timing, the
72 presence of a competitor and inaccurate feedback.¹⁵ Konings and colleagues¹⁶ reported that
73 when riding against a virtual opponent, time to complete 4-km cycling times trials improved
74 compared to no opponent due to a faster pace at the start of the time-trial. It has been
75 suggested that this change in pacing behaviour is influenced through neurotransmitters, such
76 as dopamine, which are affected by motivation, drive and perception of effort.¹⁷ Based on
77 this, if an athlete receives a treatment they believe to be performance enhancing, that athlete
78 may be more likely to change their pacing strategy, thereby impacting on performance.
79 However, to the authors' knowledge, no study has investigated the effects of a placebo
80 treatment on pacing strategy.

81 In this study, we used a balanced placebo design to examine the placebo effects of caffeine
82 on pacing strategy and performance over 1000-m running time-trials. By using a balanced
83 placebo design, we specifically aimed to: 1) determine the influence both placebo and
84 caffeine have on performance and 2) analyse participants' pacing strategies after
85 administration of deceptive and open treatments of caffeine and placebo. We also aimed to

86 establish whether any changes in performance were associated with changes in peak heart
87 rate and whether this was made possible by participants' propensity to knowingly exert more
88 effort.

89 **Method**

90 *Participants and statistical power*

91 Eight participants were estimated to provide an *a priori* statistical power of 0.80. This
92 estimation was based on a study design using repeated measures ANOVA, an *a-value* of 0.05
93 and an explained effect of $1.4 \pm 1.6\%$.¹⁸ In case of drop out, fifteen participants were initially
94 recruited. Four withdrew (two due to injury and two because of a conflicting timetable),
95 leaving eleven well-trained male middle-distance athletes (mean \pm SD: age = 25.2 ± 5.6 yrs;
96 height = 176.3 ± 8.1 cm; body mass = 66.8 ± 6.1 kg; daily caffeine consumption; 269 ± 43
97 mg·d⁻¹). Eligibility criteria stipulated that participants must be nationally ranked in the United
98 Kingdom for 800-, 1500-, 3000- or 5000-m, aged between 18 and 35 and have trained
99 minimally five days per week for at least 3 months prior to the start of the study. Only light-
100 moderate caffeine (200-350 mg·day⁻¹) users were included in the study to control for
101 individual differences and familiarity of the effects of caffeine.¹⁹ The study was anticipated to
102 last approximately four weeks. For this reason, only males were recruited to avoid
103 confounding performance variation in the mid-luteal phase of the menstrual cycle.²⁰
104 Institutional ethics approval was granted, in agreement with the Declaration of Helsinki.
105 Participants were informed that participation was voluntary and they had the right to
106 withdraw at any time during the course of the study. Participants provided written informed
107 consent after reading the study information sheet.

108 *Design*

109 We used a quasi-randomised, repeated measures, balanced placebo design to determine the
110 effects of caffeine and placebo on 1000-m running time-trial performance. Participants
111 performed seven trials: familiarisation, two baseline and four as part of the balanced placebo
112 design. The four balanced placebo design trials were as follows:

- 113 1. Informed caffeine and given caffeine (CC) – participants were informed they received
114 caffeine and did
- 115 2. Informed caffeine and given placebo (CP) – participants were informed they received
116 caffeine but received placebo
- 117 3. Informed placebo and given caffeine (PC) – participants were informed they received
118 placebo but received caffeine
- 119 4. Informed placebo and given placebo (PP) – participants were informed they received
120 placebo and did

121 The balanced-placebo 1000-m trials were randomised using a computer generated
122 programme (www.randomization.com) and participants were deceived about the treatment
123 they received in CP and PC. Participants ran 1000-m and split times were recorded at 200-,
124 400-, 600-, 800- and 1000-m. Peak heart rate (HR_{peak}) and ratings of perceived exertion
125 (RPE) were recorded immediately after the trial.

126 *Performance measure and equipment*

127 All trials were run on a 400-m, tartan track, in accordance with the International Association
128 of Athletics Federation's standards (polymer synthetic tartan track, with a depth of three
129 centimetres). Participants ran two and a half laps (1000-m) around the track as fast as
130 possible, with no assistance (e.g. pacemakers or external feedback). Times and splits were
131 measured using an automated, single-beam photocell, light gate system (Smartspeed Pro™,
132 Fusion Sport Inc., Australia) and were mounted in lane 1 of the 200- and 400-m start/finish
133 line. Single-beam light gate systems are the most common method for measuring running
134 performance and have shown to have good reliability.²¹ Weather measurements for wind
135 speed (m/s), temperature (°C), relative humidity (%) and wind chill (°C) were recorded using
136 the Pasco weather sensor (PS-2174, Pasco, Roseville CA, USA) attached to the Xplorer GLX
137 graphing data-logger (PS-2002, Pasco, Roseville CA, USA). Minimal differences were
138 reported for all time-trials (wind speed = 0.5 ± 0.2 m/s; temperature = 18.5 ± 1.9 °C; relative
139 humidity = 53.5 ± 0.9 %). *Caffeine and placebo treatments*

140 Based on previous research in the deceptive administration of caffeine,⁴ in the CC and CP
141 treatments, participants ingested 200-mL of chilled saline with 3.0 mg·kg⁻¹ of anhydrous
142 caffeine (Myprotein; Norwich, England). The dosage of 3.0 mg·kg⁻¹ caffeine was chosen as it
143 has been suggested to be optimal for improving performance lasting ~3-minutes.²² Given that
144 peak plasma caffeine typically occurs 45-minutes post-ingestion,²³ participants were asked to
145 consume the treatments 1-hour prior to the start of the time-trial. In the CP and PP treatments,
146 participants consumed 200-mL of chilled saline only. In placebo effect research, the validity
147 of the balanced-placebo design relies on the credibility of the deception in the CP and PC
148 treatments. Extensive pilot testing was therefore conducted to ensure that no taste or
149 palpability differences could be identified between placebo and caffeine treatments.

150 *Belief manipulation*

151 Before any data collection, participants attended a short presentation on the benefits of
152 caffeine on middle-distance running performance delivered by the first author. Participants
153 were provided with literature reviewing the findings of published research on caffeine and
154 middle-distance running and were informed that caffeine was previously a banned
155 performance enhancing substance. To further augment the belief that caffeine is performance
156 enhancing, and in line with current recommendations for reporting fine details of participant
157 contact and communication,² anecdotal evidence relating to the first authors' experience in
158 the use of caffeine was explained. At the time of data collection, the first author competed as
159 an international level athlete against notable Olympians and participants were informed that
160 caffeine acted as potent ergogenic aid during competition. The efficacy of this manipulation
161 of beliefs was supported by data collected in post-study interviews.

162 *Procedure*

163 Participants performed seven 1000-m running time-trials. All trials were performed on
164 Monday and Friday evening at the same location. The time between trials allowed an
165 adequate wash out period for caffeine supplementation²⁴ and is sufficient for middle-distance
166 trained athletes to fully recover.²⁵

167 For all trials, participants were instructed to arrive in 'race-shape' condition. High intensity
168 exercise 48 hours preceding the trials was not permitted, as well as the consumption of
169 alcohol or sport supplements. Participants were asked to adhere to their regular pre-race diet,

170 rest and warm-up routines. Participants began all trials at the same time of day to minimise
171 circadian variation in performance²⁶ and each trial was started by a green LED, which would
172 flash up on the photocell. To limit the potential for participants to employ pacing strategies
173 based on knowledge of previous trials and performance during trials, they did not to wear a
174 watch and were given no encouragement. No information about split times was given and the
175 results of the trials were given after all data had been collected. HR_{peak} was recorded using a
176 Polar stopwatch (Heart Monitors, Polar Ltd, Finland) and RPE from 0 (nothing at all) to 10
177 (maximal) was measured using the Borg Category Ratio²⁷ immediately after participants
178 completed the trial.

179 For familiarisation trials, participants were informed: “Today you are performing a
180 familiarisation trial” and for baseline trials 1 and 2, participants were informed “Today you
181 are performing a baseline trial”. For balanced placebo design trials, participants were further
182 reminded about which treatment they had received. For CC and CP treatments, participants
183 were informed: “Today you will be performing the trial with caffeine” and for PC and PP
184 treatments, participants were told: “Today you will be performing the trial with no caffeine.”
185 Upon completion of all data collection, participants were debriefed about the true nature of
186 the study.

187 *Data analysis*

188 Times to complete the 1000-m time-trials for baseline 1 and baseline 2 and each split (200-,
189 400-, 600-, 800- and 1000-m) were inputted into an online reliability spreadsheet.²⁸ Data
190 were log transformed to reduce nonuniform errors and the intraclass correlation (ICC) and
191 Pearson correlation (r) provided estimates of reliability. The precision of ICC was interpreted
192 as extremely high (0.99); very high (0.90), high (0.75) moderate (0.50) and low (0.20).²⁸ r
193 was interpreted as trivial (<0.1), small (0.3), moderate (0.5), large (0.5), very large (0.7),
194 nearly perfect (0.9) and perfect (1.0). In addition, paired samples t -tests were conducted to
195 determine any systematic difference in performance between baseline 1 and baseline 2.

196 Data were entered into SPSS version 24.0 (IBM, Armonk, NY) and tested for homogeneity of
197 variance, normal distribution and anomalies. Repeated measures ANOVA identified
198 differences in time to complete 1000-m time-trials between each treatment (i.e. baseline, CC,
199 CP, PC and PP) and split (i.e. 200-, 400-, 600-, 800- and 1000-m). Differences in HR_{peak},
200 RPE and mean time to complete the 1000-m trials between each treatment were also
201 established using repeated measures ANOVA. Greenhouse-Geisser epsilon was reported
202 when sphericity was violated and post-hoc LSD tests were used. Cohen’s d was calculated to
203 determine the effect size (d) of the mean differences. Differences between 0.2 and <0.5 were
204 interpreted as a small effect, between 0.5 and <0.8 as moderate, and >0.8 as large.²⁹ Data are
205 presented as mean \pm standard error of the mean with statistical significance set at $p < 0.05$

206 **Results**

207 Times were similar between baseline 1 and baseline 2 at 200- (mean differences = $-0.48 \pm$
208 0.34 s, $P = .290$, $r = 0.897$, ICC = 0.90), 400- (0.04 ± 0.40 s, $p = 0.936$, $r = 0.776$, ICC =
209 0.77), 600- (-0.56 ± 0.30 s, $p = 0.217$, $r = 0.885$, ICC = 0.85), 800- (-0.13 ± 0.53 s, $p = 0.149$,
210 $r = 0.584$, ICC = 0.61) and 1000-m (0.60 ± 0.61 s, $p = 0.189$, $r = 0.614$, ICC = 0.67). The
211 average of these two time-trials was thus used to measure baseline. Mean times to complete
212 1000-m trials in all treatments are shown in table 1.

213

214 *Main analyses*

215

216 Repeated measures ANOVA (treatment \times split) reported differences between treatment ($F_{(4, 160)} = 6.162$, $p = 0.006$; $\eta^2 = 0.381$) and split ($F_{(4, 160)} = 9.288$, $p < 0.001$; $\eta^2 = 0.482$). No
217 difference in time was shown for treatment \times split ($F_{(4, 160)} = 1.055$, $p = 0.266$; $\eta^2 = 0.108$).
218

219 *Differences in time between treatments*

220 Compared to baseline, participants ran faster in CC (mean differences = 0.64 ± 0.11 s, p
221 < 0.001 , $d = 0.42$) and CP (0.66 ± 0.18 s, $p = 0.004$, $d = 0.43$) treatments. Compared to PP,
222 participants ran faster in CC (0.80 ± 0.18 s, $p = 0.001$, $d = 0.47$) and CP (0.83 ± 0.21 s, $p =$
223 0.002 , $d = 0.48$) treatments. All differences between mean times to complete the trials and
224 treatments are shown in figure 1.

225 *Differences in treatment between splits*

226 At the 200-m split and compared to PP, participants ran faster in CC (mean differences =
227 0.94 ± 0.29 s, $p = 0.009$, $d = 0.42$) and CP (1.21 ± 0.38 s, $p = 0.010$, $d = 0.57$). At the 400-m
228 split, participants ran faster in CC compared to baseline (-0.87 ± 0.25 , $p = 0.006$, $d = 0.55$),
229 PC (-0.91 ± 0.28 s, $p = 0.009$, $d = 0.54$) and PP (-1.69 ± 0.28 s, $p = 0.001$, $d = 0.84$).
230 Similarly, participants ran faster at 400-m in CP compared to baseline (-0.68 ± 0.27 s, $p =$
231 0.031 , $d = 0.41$), PC (-0.72 ± 0.31 s, $p = 0.044$, $d = 0.41$) and PP (-1.40 ± 0.28 s, $p = 0.001$, d
232 $= 0.72$). At the 600-m split, participants ran faster in CP compared to baseline (-0.94 ± 0.27 s,
233 $p = 0.005$, $d = 0.64$) and PP (-0.81 ± 0.33 s, $p = 0.043$, $d = 0.47$). Participants also ran faster
234 at 600-m in PC compared to baseline (-1.01 ± 0.31 s, $p = 0.008$, $d = 0.60$), CC (-0.61 ± 0.23 s,
235 $p = 0.024$, $d = 0.36$) and PP (-0.88 ± 0.33 s, $p = 0.023$, $d = 0.46$). No differences were shown
236 between any treatments at the 800-m split ($p > 0.05$), but participants ran faster at 1000-m in
237 CC compared to baseline (-1.08 ± 0.43 s, $p = 0.030$, $d = 0.52$) and PP (-0.98 ± 0.40 s, $p =$
238 0.035 , $d = 0.45$). All differences between each treatment and split are shown in figure 2.

239 *Differences in peak heart rate and RPE between treatments*

240 Repeated measures ANOVA revealed no differences between treatments for HR_{peak} ($F_{(4, 40)} =$
241 1.198 , $p = 0.327$, $\eta^2 = 0.084$) and RPE ($F_{(4, 40)} = 0.892$, $p = 0.641$, $\eta^2 = 0.009$). Across all
242 treatments, mean HR_{peak} and RPE average scores ranged from 180 to 184 bpm (183.5 ± 2.3
243 bpm) and 9 to 10 (9.6 ± 0.4), respectively.

244 **Discussion**

245 We used a balanced placebo design to investigate the effect of a placebo and caffeine on
246 pacing strategy during 1000-m running time-trials. Collectively, our results indicate that the
247 belief of receipt of caffeine improved performance, which was associated with a significant
248 increase in speed during the first 400-m of the time-trial. In contrast, the hidden and open
249 administration of caffeine and placebo, respectively, did not improve performance compared
250 to baseline. Participants ran faster between 400- and 600-m during the hidden administration
251 of caffeine, but time to complete the trial overall was similar to baseline.

252 In our study, the effect of believing to have ingested caffeine improved performance to the
253 same magnitude as actually receiving caffeine. These findings complement previous findings
254 in this area, in which participants were able to significantly improve their performance after
255 being falsely informed they had received caffeine.^{30,31} However, in addition to previous
256 studies investigating the placebo effect of caffeine,^{4,30-32} we also examined participants'
257 pacing strategy during the trial, in order to establish if a change in pacing might help explain
258 the performance improvements. Given that we informed participants that they had received
259 caffeine in the CC and CP treatments, this information appears to have influenced their belief
260 of how fast they could perform, influencing the goal-directed process of decision-making
261 regarding how to distribute the available energy resources.³³ Results indicated that
262 participants were significantly faster at 400-m than baseline and also faster at 200- and 400-m
263 than when they were given a placebo and informed it was a placebo. This highlights that the
264 belief of receipt of caffeine, influences the pacing strategy at the start of a 1000-m running
265 time-trial, impacting on performance.

266 While both belief and actual receipt of caffeine improved performance at the start of the time-
267 trial, only the actual receipt of caffeine improved performance in the latter stages. At 1000-m,
268 participants ran significantly faster than baseline during the open administration of caffeine.
269 This suggests that caffeine may offset fatigue during the final stages of a 1000-m time-trial. It
270 has been reported that caffeine directly affects neuromuscular output,³⁴ which increases
271 muscular endurance and subsequently offsets fatigue.³⁵ However, no improvements in
272 performance at 1000-m were shown during the hidden administration of caffeine. Therefore,
273 the belief of receipt of caffeine was primarily responsible for the ergogenic effect of caffeine.
274 These results are similar to Atlas and colleagues,³⁶ who reported that the benefits of an
275 opioid drug were augmented after open administration compared to hidden and to a placebo
276 described as the drug. In the same study, follow up fMRI data revealed that drug and placebo
277 effects activate different neurobiological pathways, suggesting that the benefits from the drug
278 and placebo are additive. From the results reported in the present study, it could be suggested
279 that caffeine and placebo use different neurobiological pathways that affect performance.
280 Thus, when caffeine is administered openly, the verum and placebo components of caffeine
281 may combine to provide a greater improvement in performance. However, while these data
282 show additive effects for caffeine in the latter stages of the trial, it does not exclude the
283 possibility that other treatments may show interactive effects (i.e. use the same mechanisms).
284 A paucity of evidence in sport and exercise science is available in this area and future
285 research needs to design studies that examines the additive or interactive effects of treatments
286 and placebos.

287 Table 1 shows large variability between each treatment, which indicates that some
288 participants may be more likely to respond to a placebo than others. It is recognised that a
289 participant responding to a placebo can vary from study to study^{1,2} and even those who do
290 respond, may not do so consistently.³⁷ Researchers often focus on single-factor casual
291 mechanisms such as expectation theory^{4,31} or classical conditioning.^{38,39} However, placebo
292 effects are a manifestation of several factors, such as the context in which the treatment is
293 administered, the person administering it, and the psychology of the athlete (e.g. personality,
294 beliefs, and intentions). Beedie et al.³⁷ suggest that variability of the placebo effect can be a
295 function of 1) an athlete's response to the verum component of a treatment (e.g. caffeine); 2)
296 an athletes response to the placebo component only; and 3) an athletes response to both the
297 verum and placebo component. To increase knowledge and understanding of the placebo

298 effect, research is needed that helps identify the mechanisms underlying the variation in
299 placebo responsiveness.

300 Similar to previous research,^{4,31,40} no differences in peak heart rate or perceived exertion was
301 found between treatments. Given that the aim of a pacing strategy is to ensure physiological
302 limits are not surpassed while performing at an optimal level,¹⁵ a limitation of this study was
303 that the growth curve of heart rate and perceived exertion during each trial was not measured.
304 Future research should measure the differences in slopes of heart rate and RPE at each split to
305 provide a better insight into the variability in intraindividual patterns of change over time
306 between treatments.

307

308 **Conclusion**

309 In conclusion, this is the first study to show that the belief of receipt of caffeine improves
310 1000-m running time-trial performance on competitive level athletes. That is, believing to
311 have ingested caffeine, improved performance to the same magnitude as actually receiving
312 caffeine. These improvements were associated with an increase in speed during the first-part
313 of the time-trial. While slight changes in pacing strategy were demonstrated during the mid-
314 part of the time-trial with the hidden ingestion of caffeine, overall no changes compared to
315 baseline were shown. Therefore, for practitioners aiming to maximise the benefits of caffeine
316 on an athlete's performance, they should couple the administration of caffeine with a positive
317 belief of its effectiveness to increase the likelihood of that athlete improving performance.

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416

417 **Figure captions**

418 **Figure 1.** Mean split time between each treatment. Note: Data are means \pm 95% CI. * = $p <$
419 0.01 vs. CC and CP. ** = $p < 0.01$ vs. CC and CP

420 **Figure 2.** Differences in times between treatment and splits. Note: *PP vs. CC and CP ($p <$
421 0.05). **CC and CP vs. baseline, PC and PP ($p < 0.05$). #PC vs. baseline, CC and PP($p <$
422 0.05) and CP vs. baseline and PP ($p < 0.05$). †CC vs. baseline and PP ($p < 0.05$)

Table 1. Mean times (s) to complete 1000-m time-trials in each treatment

<i>Participant</i>	<i>Baseline</i>	<i>CC</i>	<i>CP</i>	<i>PC</i>	<i>PP</i>
1	166.9	164.3	165.4	172.1	165.1
2	187.3	182.9	180.9	187.1	193.1
3	179.4	174.9	175.4	174.7	178.2
4	176.4	171.1	170.5	173.7	175.3
5	168.4	164.1	160.3	163.0	164.8
6	180.4	178.4	178.7	177.8	184.5
7	169.3	164.6	165.9	165.9	169.8
8	166.3	162.5	163.7	164.3	168.1
9	183.3	180.4	179.2	182.4	181.6
10	175.2	173.3	173.6	173.9	179.1
11	181.8	182.8	184.4	182.4	183.9
Mean \pm SEM	175.9 \pm 0.55	172.7 \pm 0.60	172.6 \pm 0.60	174.3 \pm 0.59	176.7 \pm 0.68

Note: CC = Told caffeine/given caffeine; CP = Told caffeine/given placebo; PC = Told placebo/given caffeine; PP = Told placebo/given placebo