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# Influence of pacing on reliability of middle-distance cycling performance

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## Abstract

The purpose of the present study was to examine the reliability of middle distance cycling time trials using fast-, even-, and slow-starts**.** Eighteen endurance-trained male cyclists [mean ± standard deviation; $VO_{2peak}$ 63.1 ± 6.1 $mL \cdot kg^{-1} \cdot min^{-1}$] performed nine cycling time trials where the total external work (96.5 ± 11.2 kJ) was identical to the better of two, 5-minute habituation time trials. Power output during the first quarter of the time-trials (24.1 ± 2.8 kJ) was fixed to induce fast-, even- or slow-starting strategies (60, 75 and 90 s, respectively). In consecutive sessions, participants performed three trials of each pacing condition although the order of these pacing conditions was counterbalanced. Average power output and performance time were unaffected by trial number in the fast- (*P* = 0.60), even- (*P* = 0.18) and slow-start (*P* = 0.53) trials. In all three pacing conditions, average power output was highly reliable and similar between trial 1 to 2 and trial 2 to 3 in fast- (standard error of measurement; SEM=8.3 and 8.2W), even (coefficient of variation; CV=2.8 and 2.4%) and slow-start (CV=2.4 and 1.5%) trials. In conclusion, the reproducibility of 5-min cycling time trials is unaffected by starting strategy and is acceptable following two self-paced habituation trials. Research examining the influence of pacing strategies may therefore be conducted without the need for familiarisation trials using each individual pacing condition.

**Keywords:** familiarisation, pacing strategy, starting strategy, repeatability, reproducibility

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## Introduction

It is well accepted that the distribution of work throughout an exercise task, known as pacing, can have a noticeable influence on an athlete's overall performance (Abbiss and Laursen 2008). Despite this, the majority of studies that have examined various pacing profiles (fast-, even-, slow-starts; see (Abbiss and Laursen 2008) for review) during high-intensity exercise have been unable to detect a significant effect of pacing on performance (Aisbett et al. 2003; Ariyoshi et al. 1979; Foster et al. 1993; Katch et al. 1976). These findings (Aisbett et al. 2003; Ariyoshi et al. 1979; Foster et al. 1993; Katch et al. 1976) may highlight the relative insensitivity of the statistical analyses used in these studies, thereby masking the identification of small but meaningful differences in performance when adopting different pacing strategies (Foster et al. 1994).

To date several studies have examined the reliability of various constant pace time to exhaustion tests, self-paced time trials (Hopkins et al. 2001; Jeukendrup et al. 1996; Laursen et al. 2007) as well as individual athletes pacing profiles (Skorski et al. 2013). However, we are unaware of any studies that have examined the variability and reproducibility of repeated, high-intensity performances whereby pacing has been manipulated by using fast-, slow- or even-starts. This is important since the majority of research studies examining the influence of various pacing strategies on overall performance typically conduct only a single trial per pacing condition (Aisbett et al. 2003; Ariyoshi et al. 1979; Bowles and Sigerseth 1968; Foster et al. 1993; Katch et al. 1976). Given that the variation in performance during constant pace time to exhaustion tests is typically greater than self-paced time trials (Jeukendrup et al. 1996; Laursen et al. 2007), it seems reasonable to suggest that controlling exercise intensity for part of a trial may have an influence on the repeatability of performance. However, since participants are not exercising to exhaustion during the constant pace portion of these trials it is possible that such pacing manipulation has little influence on the reliability of performance when compared with traditional self-paced time trials. This may be particularly true when the intensity is automatically

controlled by an ergometer (Abbiss et al. 2009; Aisbett et al. 2009a), rather than the participant based on visual or auditory feedback (Skorski et al. 2014; Thompson et al. 2002), both of which have been used in pacing manipulation research.

We are aware of only a couple of studies that have to date conducted more than one trial for each pacing protocol (Aisbett et al. 2009b; Bishop et al. 2002). In both of these studies the authors did not report the variation in performance from the first to the second trial of the manipulated pacing conditions (Aisbett et al. 2009b; Bishop et al. 2002). Instead, the authors used the highest total work from each participant's best trial for each pacing protocol to compare performance between conditions (Aisbett et al. 2009b; Bishop et al. 2002). The trial in which the participants produced their best performance was not reported (Aisbett et al. 2009b; Bishop et al. 2002), and as a result it is not known if a learning effect was evident. As such, the variability in performance associated with consecutive high intensity exercise trials using the same pacing condition is unknown. Further, it is unclear whether participant's performance and/or the variability between performances changes over multiple pacing trials. It is possible that the self-paced habituation trials that usually precede manipulated or forced pacing trials (Abbiss et al. 2009; Aisbett et al. 2003; Mattern et al. 2001; Thompson et al. 2003) is sufficient to promote a high degree of reliability in overall performance during time-trials using various pacing strategies. Therefore, the purposes of this study were to: i) examine the reliability of performance during high intensity middle distance cycling time trials using fast-, even-, and slow-starts, and ii) determine if participants are required to be familiarised with each individual pacing profile or if a high degree of reliability (coefficient of variation < 3.5% (Abbiss et al. 2008; Hopkins et al. 2001)) during middle-distance cycling time trials using fast, even- and slow-start pacing can be obtained following two self-paced habituation trials of a similar duration.

## Materials and methods

### Participants

Eighteen endurance-trained male cyclists and triathletes [mean ± standard deviation (SD); age 28 ± 6 years, height 1.76 ± 0.06 m, mass 71.6 ± 6.7 kg and, $VO_{2peak}$ 63.1 ± 6.1 $mL \cdot kg^{-1} \cdot min^{-1}$] volunteered to participate in this study. Prior to participating in the study, all participants provided written informed consent in accordance with the appropriate Human Research Ethics Committee. The study conforms to the ethical standards of the Journal of Science and Cycling (Harriss and Atkinson 2011). Participants were instructed to attend the laboratory as if each session was an important race or training session. In the 24 h prior to the first session the participants' diet, training and sleep times were recorded using diaries. Participants were then asked to replicate these records as closely as possible during the subsequent trials. Participants were asked to refrain from consuming caffeine and alcohol in the 24 h prior to each trial. Participants were also instructed to maintain regular training commitments over the duration of the study.

### Study design

On separate days, participants in this study performed a total of eleven 5-min cycling time trials on an Excalibur Sport cycle ergometer (Lode, Excalibur Sport, Groninger, Netherlands). The first two trials performed were self-paced habituation trials and were conducted as previously described (Aisbett et al. 2009a) in order to determine the starting power output and total work during subsequent trials. The remaining trials were experimental trials whereby pacing was manipulated in order to induce fast-, even- and slow-starts (described below). Prior to all time-trials, participants performed a standardised 10-min warm-up where they cycled at a fixed power output (100 W) using a self-selected pedal-rate. At the start of the fifth, sixth, seventh, and eighth minute of this warm-up the participants were instructed to sprint for ~5 s (MacIntosh and MacEachern 1997).

### Habituation trials

Habituation trials were separated by at least two days (average: 3.4 ± 1.5 days). During these trials each participant performed a 5-min cycling test under the instructions to accumulate as much work as possible in the 5-min duration. A 5-min cycling test was chosen to approximate the duration of 4000-m cycling time-trials for trained, but not elite, athletes (Craig et al. 1993). Throughout the trials the participants were able to see their accumulated work, instantaneous power output, and the elapsed time. Participants were not given any specific pacing instructions before or during each 5-min cycling test and were not provided with verbal encouragement throughout the test.

The mean total work produced in the first and second trial was 94.0 ± 11.5 kJ and 96.2 ± 11.2 kJ, respectively. The residual errors associated with total work were normally distributed (Kolmogorov-Smirnov test = 0.10, $P = 0.20$) and not correlated ($r = 0.06$, $P = 0.73$) with the predicted scores for performance. As such, the variability associated with total work was expressed in the absolute units of measurement (i.e. kJ; (Atkinson and Nevill 1998)). The mean standard error of measurement (SEM) for the total work produced in the two trials was 2.3 kJ [95% CI: 1.7 – 3.5 kJ]. Expressed as 95% limits of agreement (LOA), total work in consecutive 5-min cycling tests was not expected, in 95% of cases, to differ by more than 6.4 kJ (95% CI: 3.1 – 9.7 kJ). The highest work output produced by each participant during one of their two habituation trials was then used as their time-trial work target in the subsequent experimental trials. The mean work output from the best habituation trial for each participant was 96.5 ± 11.2 kJ.

### Experimental trials

To assess the reproducibility of multiple time-trials using different pacing conditions, each participant performed three experimental time-trials for each separate pacing profile (fast-, even- and slow-starts).

The three experimental trials for each starting strategy were performed in successive order however; the order in which each 'block' of starting conditions were administered was counterbalanced across the cohort. Experimental trials were completed at least two days apart (average: 4.0 ± 1.7 days) and were performed at approximately the same time of day for each participant to minimise any influence of diurnal variations (Reilly 1998). The fast-, even- and slow-start time-trials used in this study have been previously described (Aisbett et al. 2009a). Briefly, power output during the first quarter of work (24.1 ± 2.8 kJ) for each trial was manipulated so that the first 25% of work for each time trial would be conducted in 60 s (fast-), 75 s (even-) and 90 s (slow-start). During the first quarter of the trial, the cycle ergometer was set in pedal-rate independent mode and power output was calculated as previously described (Aisbett et al. 2009b). Following the first quarter of the trial the ergometer was switched to pedal-rate dependent mode and participants were instructed to complete the final 75% of the trial (72.3 ± 8.4 kJ) in the shortest possible time. The fast-start pacing strategy adopted in this study has previously been shown to result in greater oxygen uptake and improved performance compared with slow- and even-start pacing strategies (Aisbett et al. 2009a).

**Statistical analysis**

The reproducibility statistics of consecutive time-trial and habituation trial performances were derived from the mean square error (MSE) term from a two-way analysis of variance (ANOVA), where the participant's identity was a random effect and trial number was a fixed, repeated measures effect (Nevill and Atkinson 1999; Schabort et al. 1999). Homoscedastic measurement error, where the ANOVA residual errors were not significantly correlated with the predicted scores for performance, were expressed in the actual units of measurement as SEM and 95% limits of agreement (LOA; (Atkinson and Nevill 1998). Heteroscedastic measurement error, where the ANOVA residual errors were significantly correlated with the predicted performance scores, required that the natural logarithm of time-trial or habituation trial performance become the dependent variable in the aforementioned ANOVA (Atkinson et al. 1999; Bland and Altman 1986). Thereafter, heteroscedastic measurement error was presented as the coefficient of variation (CV) and the 95% ratio LOA (rLOA; (Atkinson et al. 1999; Bland and Altman 1986) (Schabort et al. 1999). For descriptive purposes only, homoscedastic measurement error was converted into CV or rLoA using the mean performance, to allow readers to informally compare the measurement error between pacing conditions. To further aid this descriptive comparison, a CV lower than 3.5% was regarded as high test re-test reliability, based upon previous research examining the reliability of trained cyclists using the same or similar electromagnetically braked cycle ergometer (Abbiss et al. 2008; Hopkins et al. 2001; Jeukendrup et al. 1996). However, as homosedastic and heteroscedastic measurement error cannot be formally compared, the reliability of performance under different starting conditions was compared using a one-way ANOVA of the participant's SD of performance across the three trials. The individual finishing time SD for the fast-, even-, and slow-start trials were square root transformed as the residual errors were heteroscedastic even following logarithmic transformation.

Formal evaluation within each pacing condition was also not possible when comparing the reliability of performance between trials one and two and two and three, as the data for trial two is common to each statistic. In this instance, the reliability of performance between trials was compared for descriptive purposes only, by calculating the ratio of each statistic used (i.e. CV and SEM) (Hopkins 2002). A ratio of 1.20 (or 0.83) was deemed to be the smallest worthwhile difference in the statistic used (Abbiss et al. 2008). The statistical analyses detailed above were performed using SPSS v11.0 (Champaign, Illinois), unless otherwise stated.

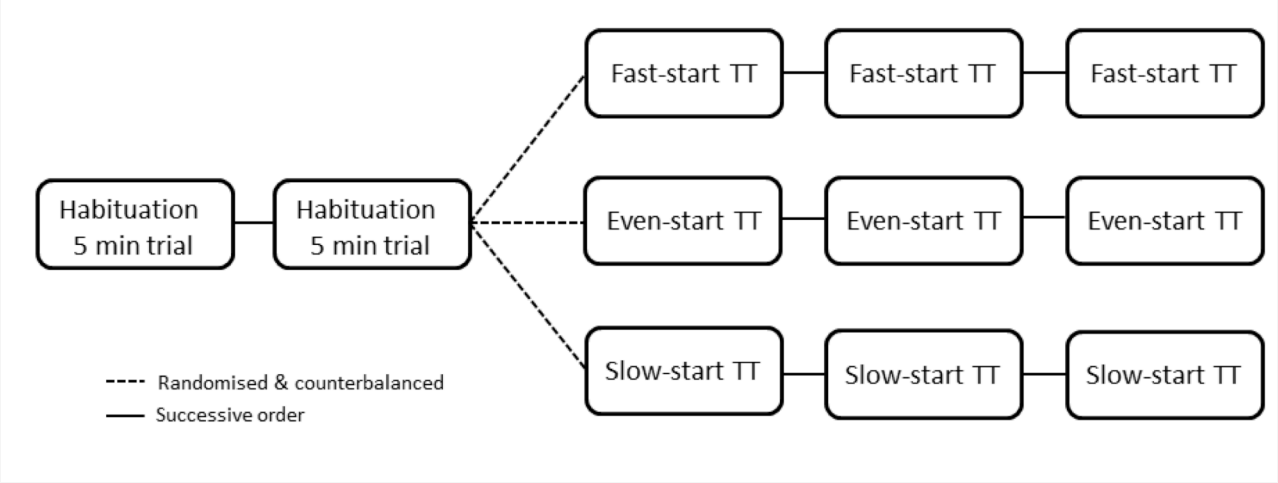


**Figure 1.** Schematic representation of the study design. Trials were separated by at least two days and participants performed all trials.

## Results

Average power output and overall performance times during the fast-, even and slow-start time-trials are presented in Table 1. There was no significant main effects for trial number (time; $P = 0.20$, power; $P = 0.16$) or significant interaction between starting condition and trial number (time; $P = 0.50$, power; $P = 0.48$) for the finishing times and mean power produced in the fast-, even-, and slow-start time-trials (Table 1). When the three trials for each condition were analysed separately, there were also no significant main effects for trial number on finishing time in the fast- ($P = 0.60$), even- ($P = 0.18$), or slow-start ($P = 0.19$) trials (Table 1). The main effects for trial number on fast- ($P = 0.60$), even- ($P = 0.18$), or slow-start ($P = 0.53$) mean power outputs were also not significant (Table 1).

Measures of reliability for average power output and overall performance times during time-trials using the fast- even-, and slow-start conditions are presented in Tables 1 and 2, respectively. The measurement error for the fast-start conditions was homoscedastic and therefore is presented in absolute units of time (s) and power (W). Therefore, the CV and 95% rLoA statistics are not applicable to the fast-start condition and are depicted as n/a in Tables 1 and 2. In contrast the measurement error for the even- and slow-start conditions was heteroscedastic and is presented in percentage and ratios. Here, the SEM and 95%LoA statistics cannot be applied to the even- and slow-start condition results, and therefore these cells are also depicted as n/a in Tables 1 and 2. The inclusion of both SEM and 95%LoA (and their heteroscedastic equivalents) was an attempt to provide a breadth of statistics so that readers could refer to statistic they are most comfortable using.

To aid a simple, descriptive comparison of the measurement error across the three starting conditions, the SEM and 95% LOA (plus mean performance) for all three of the fast-start time-trials becomes 2.4% and 1.07, respectively, when expressed relative to mean fast-start time-trial performance. These values are comparable to CV and 95% rLOA accompanying repeated even-start time-trial performances (Table 1). The SD for repeated fast- and even-start time-trial performances appear to be more variable than the SD for consecutive slow-start time-trials (Table 1)

**Table 1.** Overall finishing time, mean power output and the reliability of overall finishing time, mean power output for the three-trials performed using the fast-, even-, and slow-start conditions.

| | Trial One | Trial Two | Trial Three | Mean | SEM | CV (%) (95%CI) | 95%LOA (95%CI) | 95% rLOA (95%CI) |
|---|---|---|---|---|---|---|---|---|
| *Fast-start* | | | | | | | | |
| Time (min:s) | 4:52 ± 0:12 | 4:51 ± 0:08 | 4:49 ± 0:11 | 4:50 ± 0:09*# | 0:07 (0:06 to 0:10) | 2.4 ‡ (2.1 to 3.4) | 0:20 (0:18 to 0:23) | n/a |
| Mean power (W) | 330 ± 37 | 331 ± 34 | 333 ± 35 | 331 ± 35*# | 8.0 (6.5 to 10.0) | 2.4 ‡ (2.0 to 3.0) | 22.2 (14.0 to 30.4) | n/a |
| *Even-start* | | | | | | | | |
| Time (min:s) | 5:01 ± 0:11 | 4:57 ± 0:11 | 4:58 ± 0:13 | 4:59 ± 0:13* | n/a | 2.6 Δ (2.1 to 3.5) | n/a | 1.07 Δ (1.06 to 1.08) |
| Mean power (W) | 320 ± 37 | 324 ± 35 | 324 ± 35 | 323 ± 38* | n/a | 2.6 Δ (2.1 to 3.5) | n/a | 1.07 Δ (1.06 to 1.08) |
| *Slow-start* | | | | | | | | |
| Time (min:s) | 5:07 ± 0:12 | 5:05 ± 0:14 | 5:07 ± 0:12 | 5:07 ± 0:13# | n/a | 2.0 Δ (1.6 to 2.7) | n/a | 1.06 Δ (1.05 to 1.07) |
| Mean power (W) | 314 ± 36 | 316 ± 37 | 315 ± 40 | 315 ± 42# | n/a | 2.0 Δ (1.6 to 2.7) | n/a | 1.06 Δ (1.05 to 1.07) |

Performance values are means ± SD ($n$ = 18); SEM, standard error of measurement; CV, coefficient of variation; 95%CI, 95% confidence intervals; 95% LOA, 95% limits of agreement; 95% rLOA, 95% ratio limits of agreement; n/a – not applicable based upon statistical analysis performed (see methods); Δ residuals not normally distributed and/or heterscedastic- reliability statistic calculated from natural logarithm; ‡converted into a CV based on mean performance for descriptive purposes only; *P<0.05, than slow-start ; #P<0.05, than even-start.

**Table 2.** Variability of performance between trials one to two, and trials two to three of the fast-, even- and slow-start conditions.

| | Trials One to Two | | | | Trials Two to Three | | | |
|---|---|---|---|---|---|---|---|---|
| | SEM | CV (%) | 95%LOA | 95% rLOA | SEM | CV (%) | 95%LOA | 95% rLOA |
| *Fast-start* | | | | | | | | |
| Time (min) | 7.7 (5.8 to 11.5) | 2.6 ‡ (2.0 to 3.9) | 21.3 (18.1 to 24.4) | n/a | 7.2 (5.4 to 10.8) | 2.5 ‡ (1.9 to 3.7) | 19.9 (16.9 to 22.9) | n/a |
| Mean power (W) | 8.3 (6.2 to 12.4) | 2.5 ‡ (1.9 to 3.7) | 22.9 (12.8 to 33.1) | n/a | 8.2 (6.2 to 12.3) | 2.5 ‡ (1.9 to 3.7) | 22.7 (12.8 to 32.6) | n/a |
| *Even-start* | | | | | | | | |
| Time (min) | n/a | 2.8 Δ (2.1 to 4.2) | n/a | 1.08 Δ (1.06 to 1.10) | n/a | 2.4 Δ (1.8 to 3.6) | n/a | 1.07 Δ (1.06 to 1.08) |
| Mean power (W) | 7.1 (5.2 to 11.0) | 2.3 ‡ (1.7 to 3.5) | 19.7 (9.4 to 30.1) | n/a | n/a | 2.4 Δ (1.8 to 3.6) | n/a | 1.07 Δ (1.06 to 1.08) |
| *Slow-start* | | | | | | | | |
| Time (min) | n/a | 2.4 Δ (1.8 to 3.6) | n/a | 1.07 Δ (1.05 to 1.09) | 4.8 (3.6 to 7.3) | 1.5 ‡ (1.1 to 2.3) | 12.2 (8.4 to 16.0) | n/a |
| Mean power (W) | n/a | 2.4 Δ (1.8 to 3.6) | n/a | 1.07 Δ (1.02 to 1.12) | n/a | 1.5 Δ (1.1 to 2.2) | n/a | 1.04 Δ (0.99 to 1.10) |

Reliability statistics are means (95% confidence intervals) ($n$ = 18); SEM, standard error of measurement; CV, coefficient of variation; 95%CI, 95% confidence intervals; 95% LOA, 95% limits of agreement; 95% rLOA, 95% ratio limits of agreement; n/a – not applicable based upon statistical analysis performed (see methods); Δ residuals not normally distributed and/or heterscedastic- reliability statistic calculated from natural logarithm; ‡ converted into a CV based on mean performance for descriptive purposes only.

however, this was not significant for finishing time ($P$ = 0.11) or mean power output ($P$ = 0.07).

The variability in performance during fast- and even-start trials was comparable between the first two (Trial One and Two) and the last two trials (Trial Two and Three; Table 2). The SEM in mean power output between fast-start trials one and two, and between two and three were 8.3 and 8.2W, respectively (ratio = 0.99; 95% CI = 0.60 to 1.62). The CV in finishing time between even-start trials one and two, and between trials two and three was 2.8 and 2.4% (ratio = 0.86; 95% CI = 0.52 to 1.40). During the slow-start trials, the CV in power output between trials one and two appeared greater than that of trials two and three (2.4 and 1.5%, respectively; ratio = 0.63; 95% CI = 0.38 to 1.02). However, a formal evaluation of the measurement error accompanying each pair of time-trials under a specific starting condition was not undertaken since data from trial two was common to each statistic.

## Discussion

The present study examined the reliability of repeated high-intensity cycling time trials using fast-, even-, and slow-start pacing strategies. The results from this study indicate that the test-retest reliability of a middle distance (5-min) cycling time trial is not influenced by pacing strategy manipulations. Further, power output and overall time during consecutive fast-, even-, and slow-start time-trials was highly reliable (CV < 3.5% (2, 19)) and not significantly different between trials one to three following two self-paced habituation trials.

While the reliability of various constant or self-paced testing protocols have been established (Jeukendrup et al. 1996; Laursen et al. 2007), this study is the first, to the authors' knowledge, to examine the influence of pacing strategies on the reproducibility of high-intensity cycling performance. In the present study, it was found that pacing strategy manipulation had no effect on the overall test-retest reliability of 5-min (96.5 ± 11.2 kJ) time trial performance, as evidenced by a lack of significant difference between participant's individual SD for repeated fast-, even-, and slow-start performances. However, it is worth acknowledging that the difference in individual SD for performance between the fast- (mean time SD: 6.2 s, mean power SD: 6.9 W), even- (time: 6.8 s, power: 7.1 W), and slow- (time: 4.6 s, power: 4.5 W) start time-trials was close to statistical significance (finishing time SD, $P$ = 0.11; mean power SD, $P$ = 0.07). The effect size for this possible difference was, however, small ($\eta^2$ = 0.14), despite 13 of the 18 participants recording their lowest SD using the slow-start protocol. The lack of significant difference in the reproducibility of multiple time-trial performances using different starting conditions indicates that varying the time taken to complete the first quarter of work does not significantly influence the reproducibility of performance during cycle ergometer trials lasting ~ 5 min. Somewhat contradictory to these findings, Thompson et al. (2002) found a lower degree of random error during even-paced, compared with fast- or slow-start 175-m breaststroke swimming. In that study (Thompson et al. 2002), pacing of nine national and club swimmers was deliberately controlled by audio signals so to induce positive, negative or even split times. It was suggested that lower degree of random error during the even-paced time trial was due to the participants' inability to precisely reproduce changes in pace associated with fast-, or slow-start strategies (Thompson et al. 2002). The work of Thompson et al. (2002) therefore highlighted the error associated with athletes' ability to swim at a given pace rather than the adoption of particular pacing profile itself. The low error and presumably reduced variability observed in the even-paced time trial of Thompson et al. (2002), was not observed in the current study since power output during the first 25% of each experimental cycling time trial was fixed using the cycle ergometer and in no way controlled by the participant as when using auditory signals. Future research examining the variability of performance when adopting various pacing strategies during field-based cycling, whereby pacing strategy has been manipulated using sensory feedback, such as heart rate, speed, auditory signals, power output, is warranted.

The second aim of the present paper was to determine if a high degree of reliability (CV < 3.5% (2, 19)) during middle-distance cycling time trials using fast, even- and slow-starts can be obtained following two self-paced habituation trials of a similar duration. These results are important since the majority of previous research examining the effects of pacing on time trial performance generally do not conduct specific familiarisation trials for each pacing condition. Instead, within this previous research (Abbiss et al. 2009; Aisbett et al. 2003; Mattern et al. 2001; Thompson et al. 2003), participants typically perform one or two self-paced habituation trials of a similar distance or duration prior to completing the manipulated pacing performance trials. Irrespective of the pacing strategy used in the present study, power output and overall time following two self-paced habituation trials, was highly reliable and not significantly different between trials one to three (Table 1). As a result, it appears that a high degree of reliability in measures of performance during middle distance cycling using various enforced pacing strategies (CV < 3.5%; Table 2) may be obtained if participants perform only two self-paced habituation trials of a similar distance/duration. Research examining the influence of different pacing strategies may therefore be conducted without the need for a familiarisation trial using each pacing condition. These results should however, be interpreted with caution since the coefficient of variation in performance during the slow-start time trials appeared greater for trials one and two, compared trials two and three (ratio = 0.63; 95%CI = 0.38 to 1.02). Therefore, though it is possible that study design may be improved if participants perform at least one practice trial using each pacing condition prior to experimental trials, formal evaluation of reliability in the current study does

not support this contention. These findings are fundamental for planning future pacing research, since the number of trials directly impacts various aspects of research including ethical considerations, subject recruitment and research costs.

In conclusion, the results of the present study indicate that the overall reproducibility of middle distance (5-min) cycling time trial performance is unaffected by starting strategy. Further, following two habituation trials a high degree of reliability in measures of performance (CV < 3.5%; (Abbiss et al. 2008; Hopkins et al. 2001)) may be observed, irrespective of starting strategy. Research examining the influence of different pacing strategies may therefore be conducted following two habituation trials and without the need for a familiarisation trial using each pacing condition.

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