

ENDPOINT CREDIBILITY AND TERMINAL ACCELERATION IN ENDURANCE CYCLING

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ABSTRACT

Distance feedback can change the final increase in cycling power without producing a corresponding change in completion time. Accurate information, source trust and uncertainty about the finish therefore require separate treatment. To determine the mathematical conditions under which credibility-weighted distance beliefs make a higher output acceptable until completion. Remaining distance is represented by a subjective probability distribution. A linear pool combines the pre-cue belief with the cue-implied distribution; perceived sustainable distance is calculated separately from expected speed and tolerable duration. Acceleration is acceptable when the upper distance quantile at a specified acceptance probability lies within that sustainable distance. For a pre-cue belief spanning 1.8–2.6 km, a cue spanning 1.5–1.7 km and an acceptance probability of 0.90, cue weights of 0.20 and 0.80 yield upper quantiles of 2.50 and 2.20 km. At a sustainable distance of 2.30 km, the criterion holds only for the larger weight. These values are analytical inputs, not participant observations. Disagreement between cues contributes directly to belief variance, so a nearer mean estimate need not remove the distant upper tail. Five predictions separate credibility, bias direction, interval width, cue-restoration timing and redundant position information. A prospective six-condition cycling experiment and a distance-based acceleration analysis specify tests of the decision criterion. The article contains theoretical derivations and cited endurance evidence; it makes no claim of a newly measured performance effect.

Keywords: endurance pacing; endpoint perception; cue credibility; distance uncertainty; terminal acceleration; critical power

INTRODUCTION

An endurance athlete who can increase output must also judge whether that increase is sustainable until the finish. Decision-making accounts describe pacing as the regulation of muscular work against anticipated demands and the consequences of premature expenditure (Renfree et al., 2014). Perception–action accounts similarly connect available information to the actions an athlete considers feasible (Smits et al., 2014). Experimental manipulations of pace and feedback show that these decisions are adjustable, but their consequences depend on task conditions (Skorski and Abbiss, 2017). In competitive running, the distribution of effort also depends on whether the objective is the fastest time or the best placing (Casado et al., 2021). These observations are compatible with pacing emerging from interacting physiological, perceptual and environmental constraints (Renfree and Casado, 2018).

The narrower problem considered here is the timing of commitment to terminal effort in a fixed-distance task. The finish is objectively unchanged; uncertainty concerns the athlete's estimate of remaining demand. Knowing the scheduled race distance does not ensure equally precise awareness of current position. A digital cue may be absent, inaccurate or mistrusted, while elapsed time, remembered splits and landmarks remain informative. Consequently, the behavioural effect of a display cannot be inferred from its presence alone.

The analysis distinguishes the distance implied by a cue, the athlete's reliance on that cue and the upper range of the remaining-distance belief. It then compares that upper range with the distance sustainable at a specified higher power.

This comparison identifies when identical numerical feedback can permit acceleration for one credibility weight while leaving the criterion unmet for another. The equations concern a specific decision near the finish, with separate tests for cue uptake and power response.

PHYSIOLOGICAL CONSTRAINTS AND THE SCOPE OF THE ARGUMENT

The power–duration relationship describes a physiological limit on sustained output, particularly in cycling (Burnley and Jones, 2018). Above critical power, the finite work capacity conventionally denoted W' is expended; modelling its reconstitution during recovery can help characterize intermittent effort (Skiba et al., 2012). Critical power also separates patterns of physiological disturbance, although neither W' nor a two-parameter fit represents every source of fatigue (Poole et al., 2016). An informational account must therefore distinguish willingness to accelerate from the capacity to execute that decision.

Premature and delayed commitment are temporally asymmetric. An early increase can incur fatigue before the actual finish, whereas a late increase may leave insufficient distance to exploit an otherwise sustainable action. Neither consequence establishes a universal ranking of errors. Their costs depend on the required output, course, event duration and athlete. An absent end spurt does not demonstrate unused physiological capacity, and an observed end spurt does not demonstrate that earlier conservation was optimal.

The primary scope is familiarized, self-paced cycling on a fixed course with a time-minimization objective. Running, swimming and tactical racing provide relevant boundary cases, but their mechanics and objectives require separate tests. The model describes a belief-dependent action constraint; it does not assign the entire pacing process to a single neural controller or imply that athletes consciously perform its calculations.

DISTANCE FEEDBACK, EFFORT EXPECTATIONS AND TERMINAL POWER

EVIDENCE SELECTION AND EXPERIMENTAL CONTRASTS

The evidence comprises feedback omission, inaccurate distance cues and disguised exercise intensity in research from 2011–2023. Experiments are selected for contrasts relevant to endpoint judgment rather than to estimate an average feedback effect. Differences in task distance, participant training and information channels preclude treating these comparisons as interchangeable. The literature selection is purposive and does not constitute a systematic review.

In Faulkner et al., 13 men completed 6-km treadmill trials with accurate, premature, delayed or absent distance feedback. Completion was slower without feedback ($p < 0.001$); the accurate and inaccurate conditions did not significantly differ in completion time (Faulkner et al., 2011). In Smits et al., 20 trained male cyclists/triathletes completed 20-km trials. Mean completion times were 28.86 ± 3.68 min with full feedback and 30.95 ± 2.77 min without feedback; the between-group difference was not statistically significant. Terminal power increased in the full-feedback group but not in the group without feedback (Smits et al., 2016). Wingfield et al. examined nine trained male cyclists over 30 km, finding faster completion and higher power during the final 2 km when performance and distance information were available (Wingfield et al., 2018).

Distance deception can alter pacing and perceived exertion, but a display's numerical error alone does not determine its behavioural effect (Wingfield et al., 2019). Stone et al. tested ten trained male cyclists against a virtual pacer set at 102% or 105% of each cyclist's earlier power, with both pacers presented as that earlier performance. Completion in the 102% condition was 1.03% faster than in the initial timed trial; the 105% condition differed significantly from neither comparison (Stone et al., 2017). The absence of a significant 102%-versus-105% difference prevents identifying a superior deception magnitude from those comparisons. Taylor and Smith obtained running times of 1348 ± 140 , 1333 ± 129 and 1350 ± 135 s after the initial, deceptive higher-power and honestly disclosed higher-power cycling conditions, respectively (Taylor and Smith, 2017). Such intensity-expectation effects provide an alternative to a change in perceived remaining distance.

ATTENTION, COMPETITION AND EXISTING PACING EXPLANATIONS

Interpersonal competition can alter pacing even when the endpoint is unchanged (Konings and Hettinga, 2018). Cognitive self-regulation and attentional focus can also modify how athletes use task and body information (Brick et al., 2016). Performance feedback has mitigated mental-fatigue effects in a handgrip task (Dallaway et al., 2022); this demonstrates a possible nonspatial feedback pathway rather than direct support for a cycling endpoint mechanism. Research on prefrontal hemodynamics presents further measurement and interpretation difficulties (Hyland-Monks et al., 2022). Cycling studies have documented concurrent changes in affect, exertion, muscular activity and confidence (Renfree et al., 2012), while attentional research cautions against assuming that more information necessarily improves regulation (Brick et al., 2014).

Existing pacing explanations connect desired outcomes with tolerable physiological disturbance and psychophysiological interpretation (Foster et al., 2023). Smits et al. identified endpoint uncertainty as a possible constraint on terminal effort and suggested introducing endpoint information late in exercise (Smits et al., 2016). The distributional treatment developed here specifies the influence assigned to a cue, the disagreement retained between cues and the upper quantile used to judge completion. Table 1 identifies observations that can distinguish those terms from effects of a learned pacing pattern, attention or competition.

TABLE 1. EXISTING PACING EXPLANATIONS AND OBSERVATIONS THAT DISCRIMINATE THE ENDPOINT CRITERION FROM OTHER INFLUENCES. THESE MECHANISMS CAN OPERATE CONCURRENTLY.

Existing explanation	Shared premise	Added testable distinction
Decision making and perception-action (Renfree et al., 2014; Smits et al., 2014)	Actions reflect expected demand, body information and goals.	Equal cue content should yield different distance judgments when subjective source influence differs.
Endpoint knowledge and anticipation (Smits et al., 2016)	Uncertain completion may postpone terminal effort.	A mean endpoint estimate can shift toward the finish while its upper tail remains too distant for commitment.
Learned pacing template and integrated psychophysiology (Foster et al., 2023)	Experience and expected strain shape effort distribution.	A cue-dependent change in distance judgment should add predictive information beyond a pacing pattern estimated before cue exposure.
Competition and attentional regulation (Konings and Hettinga, 2018; Brick et al., 2016, 2014)	Visual feedback can change comparison, attention or motivation.	Matched visual stimulation without endpoint content should not reproduce the full endpoint-specific sequence.

DISTANCE BELIEFS AND THE SUSTAINABLE-COMPLETION CRITERION

DEFINITIONS AND CUE WEIGHTING

Let $D_t \geq 0$ be the true distance remaining at decision time t . The athlete's belief about D_t is represented by a cumulative distribution function $B_t(d)$ on nonnegative distance. Its mean μ_t describes the estimated endpoint and its upper quantiles describe uncertainty relevant to premature commitment. These are subjective states, not sampling distributions of an investigator's estimator.

For a new cue, $C_t(d)$ represents the distribution the athlete would attribute to its numerical content if relying on that cue alone. Its centre c_t need not equal D_t or the printed display value r_t . The signed objective cue bias is $b_t = r_t - D_t$: negative values display a nearer finish and positive values a farther finish. The simplest interpretation assumes $c_t = r_t$, but comprehension checks must assess that assumption. Cue precision concerns the spread of C_t , whereas credibility concerns the weight w_t assigned to it relative to the pre-cue belief. Neither property is inferred from observed acceleration.

A minimal descriptive rule is the linear distributional pool

$$B_{t+}(d) = (1 - w_t)B_{t-}(d) + w_t C_t(d), \quad 0 \leq w_t \leq 1. \quad (1)$$

The pre-cue belief and the cue-implied distribution must refer to the same position. Between observations, perceived travel and its error determine how the distance belief moves toward zero. A belief elicited at an earlier position therefore requires propagation before pooling. Equation 1 specifies a linear combination of distributions; multiplicative cue integration and nonlinear rejection of discrepant cues are alternative rules to test against it.

If the pre-cue belief has mean μ_- and variance σ_-^2 , and the cue distribution has mean c and variance σ_C^2 , then

$$\mu_+ = (1 - w)\mu_- + wc, \quad (2)$$

$$\sigma_+^2 = (1 - w)\sigma_-^2 + w\sigma_C^2 + w(1 - w)(\mu_- - c)^2. \quad (3)$$

The final term retains disagreement between sources. Thus, increasing reliance on a nearer cue can move the mean toward the finish without immediately eliminating a distant upper tail. Credibility and uncertainty are consequently related but not interchangeable. The resulting distribution can be bimodal; its mean and variance alone do not determine its upper quantile.

PERCEIVED SUSTAINABLE EFFORT AND THE DECISION CRITERION

For candidate output P^* above the current output, let $\hat{H}_t(P^*)$ be the duration the athlete believes sustainable before unacceptable pace deterioration. On a fixed flat ergometer course, let $\hat{v}_t(P^*)$ be perceived speed at that output. The corresponding tolerable distance is

$$L_t(P^*) = \hat{v}_t(P^*)\hat{H}_t(P^*). \quad (4)$$

Speed and duration must use compatible units. On a varying course, the relevant distance is the integral of predicted speed over the tolerable duration; a single constant-speed conversion is insufficient. Perceived exertion and muscular sensation may inform $\hat{H}_t$, but neither is equated to it. Independently estimated critical power and W' constrain physiological interpretation rather than defining the athlete's belief.

Define the subjective upper quantile

$$q_\alpha(B_t) = \inf\{d : B_t(d) \geq \alpha\}, \quad 0 < \alpha < 1. \quad (5)$$

For fixed L_t , the credibility–capacity condition is

$$B_t\{L_t(P^*)\} \geq \alpha \iff q_\alpha(B_t) \leq L_t(P^*). \quad (6)$$

Here α is the minimum subjective probability of sustainable completion the athlete requires for that candidate action. It is not a frequentist confidence level, and a population-wide value is not assumed. The criterion indicates when acceleration is acceptable under the represented demand; motivation, tactical objectives and actual execution capacity still determine whether it occurs.

Capacity uncertainty can be represented separately by a joint subjective distribution J_t over remaining distance and sustainable distance. The corresponding condition is $\Pr_{J_t}\{D_t^{\text{belief}} \leq L_t^{\text{belief}}(P^*)\} \geq \alpha$. Dependence must be retained when effort sensations inform both estimates. The fixed- L_t form is the initial testable restriction. Investigators should not fit unrestricted distance and capacity distributions, a freely changing w_t , and a freely changing α to the same pacing curve: that would make the explanation unidentifiable.

A WORKED MATHEMATICAL EXAMPLE

The calculation uses a uniform pre-cue distance belief on $[1.8, 2.6]$ km and a uniform cue-implied belief on $[1.5, 1.7]$ km. These intervals are analytical inputs rather than athlete measurements. With perceived sustainable distance $L = 2.3$ km and acceptance probability $\alpha = 0.90$, a cue weight of $w = 0.20$ gives a post-cue mean of 2.08 km and an upper 0.90 quantile of 2.50 km. A weight of $w = 0.80$ gives 1.72 km and 2.20 km, respectively. The smaller weight leaves the upper quantile beyond sustainable distance; the larger weight places it within sustainable distance. Cue content and capacity are identical in the two calculations.

In both cases the 0.90 quantile lies in the pre-cue interval. Because the cue component has already reached cumulative probability one by 1.8 km, the quantile is

$$q_{0.90} = 1.8 + 0.8 \frac{0.90 - w}{1 - w} \text{ km}, \quad w < 0.90. \quad (7)$$

At $L = 2.3$ km, the pre-cue cumulative probability is 0.625 and the cue-implied cumulative probability is one. Equation 1 therefore gives

$$\begin{cases} B_+(2.3) = 0.625 + 0.375w, \\ B_+(2.3) \geq 0.90 \iff w \geq \frac{11}{15} \approx 0.7333. \end{cases} \quad (8)$$

The completion probabilities are 0.700 at $w = 0.20$ and 0.925 at $w = 0.80$. Thus, the acceptance condition is determined by a specific weight threshold for these intervals and this sustainable distance. The threshold is an algebraic consequence of the stated inputs, not an estimate of an athlete's minimum trust.

Figure 1 also identifies the precision boundary. A uniform belief with fixed mean μ and half-width h has $q_{0.90} = \mu + 0.8h$. With $\mu = 2.2$ km and $L = 2.3$ km, the criterion holds for $h \leq 0.125$ km. These boundaries apply to the specified uniform distributions; a different distribution requires its own upper-quantile calculation.

CONDITIONAL PREDICTIONS AND CONDITIONS FOR REJECTION

Table 2 maps each prediction to the contrast needed to test it. The predicted effects apply when the manipulation changes the intended belief property, when the criterion is near enough to its boundary to permit a response, and when

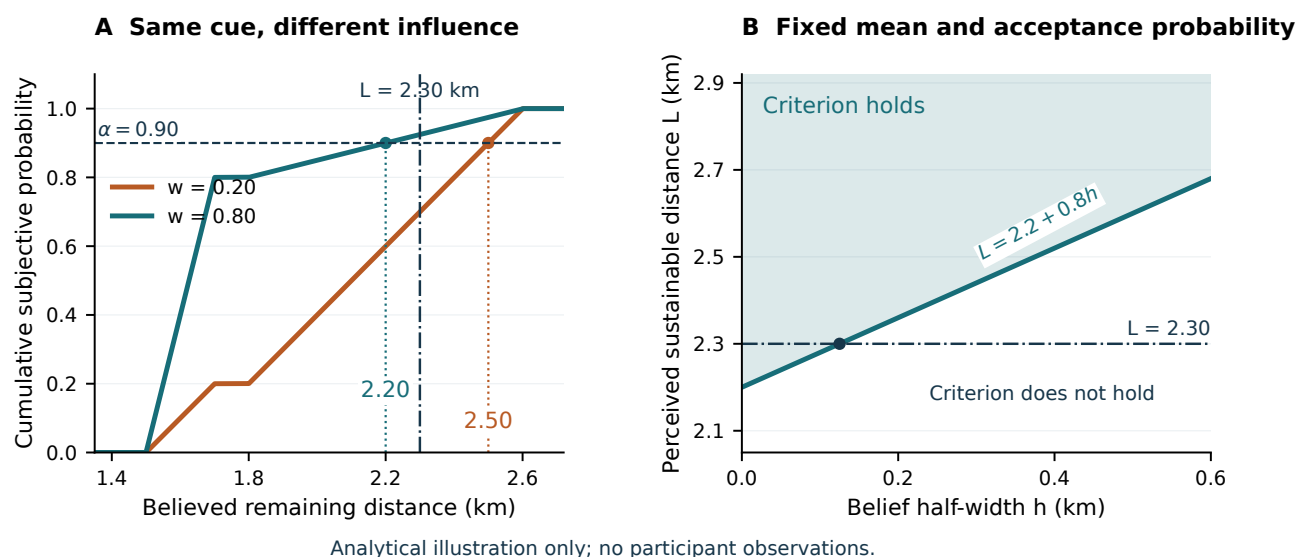


FIGURE 1. CALCULATED DISTANCE BELIEFS AND SUSTAINABLE-COMPLETION BOUNDARY. (A) POST-CUE CUMULATIVE DISTRIBUTIONS FOR $w = 0.20$ AND $w = 0.80$, USING IDENTICAL PRE-CUE AND CUE DISTRIBUTIONS, $L = 2.3$ KM AND $\alpha = 0.90$. THE UPPER QUANTILES ARE 2.50 AND 2.20 KM. (B) THE BOUNDARY $L = 2.2 + 0.8h$ FOR A UNIFORM BELIEF CENTRED AT 2.2 KM. CANDIDATE OUTPUT, MEAN ENDPOINT, ACCEPTANCE PROBABILITY AND COURSE MAPPING REMAIN FIXED; BELIEF HALF-WIDTH AND PERCEIVED SUSTAINABLE DISTANCE VARY. SHADING IDENTIFIES SATISFACTION OF EQUATION 6. BOTH PANELS ARE ANALYTICAL CALCULATIONS WITHOUT PARTICIPANT OBSERVATIONS.

TABLE 2. PREDICTIONS AND THEIR DESIGNATED EXPERIMENTS. EARLIER ONSET MEANS A SMALLER FRACTION OF TRUE DISTANCE COMPLETED.

Test	Controlled contrast	Predicted pattern	Design module
P1	Same mean endpoint and credibility; narrower versus wider believed distance interval.	A reduced upper quantile permits earlier commitment when perceived sustainable distance lies between the two quantiles.	Precision experiment.
P2	Equal signed bias under high versus low source credibility.	Greater cue uptake produces a larger belief displacement; onset shifts in the corresponding direction near the boundary.	Core 2×3 experiment.
P3	Equal-magnitude nearer versus farther finish bias.	Nearer belief favours early commitment; farther belief favours delay. Subsequent slowing is predicted only when early commitment exceeds actual tolerance.	Core experiment; full-trial surge analysis.
P4	Absent information restored at earlier versus later true positions.	A change in distance judgment should precede a response where the candidate action remains feasible.	Cue-restoration experiment.
P5	Endpoint cue omission with versus without an independent accurate position cue.	Removing one cue has less effect when another preserves the relevant upper endpoint quantile.	Redundancy experiment.

actual capacity allows acceleration. These conditions must be checked prospectively. They cannot be invoked after any null result without measurement.

An unchanged onset distribution after verified belief shifts across a prospectively informative capacity range would weaken the central link. A null behavioural effect without belief uptake tests the manipulation rather than the theory. A change in pace reproduced by visual stimulation without a change in endpoint belief would favour attention or arousal. Failure of Equation 1 to predict elicited distributions would reject that pooling rule, even if a broader association between endpoint judgment and pacing persisted. A priori practical effect bounds and uncertainty intervals are needed to distinguish imprecision from evidence against an informative effect.

The model's discriminating value depends on parameter restrictions. Credibility weights should be estimated from judgment tasks independently of the primary pacing outcome; acceptance probability should be fixed in prespecified sensitivity analyses or measured in a separate decision task. Perceived capacity should be measured before the relevant

cue when used as a moderator. No post hoc change in those quantities should be used to rescue an otherwise contradictory prediction.

A STAGED EXPERIMENTAL PROGRAMME

CONSTRUCT VALIDATION BEFORE PERFORMANCE TESTING

A preliminary measurement study should establish that participants can report a point estimate and a subjective interval for remaining distance during familiarized cycling. The interval instructions should identify an explicit probability, such as an 80% central interval, with practice judgments and comprehension checks. A separate question should ask how long a specified higher power could be maintained; substituting general confidence in race performance would conflate endpoint and capacity beliefs.

Source credibility can be manipulated by prior experience of source consistency in a short verification task. Display identity, colour and screen position should be counterbalanced. The numerical centre, displayed uncertainty interval and visual motion in the performance trial should remain identical across source conditions. Such training can nevertheless alter perceived numerical precision as well as source trust. The pilot must therefore separately measure both constructs. A claim of isolated credibility requires interval-width equivalence within a prespecified meaningful margin; a non-significant difference is insufficient. If equivalence is not supported, the intervention should be interpreted as a source-reliability package and its effects decomposed cautiously rather than labelled a pure credibility effect.

On judgment-only probe trials, obtain beliefs before and after cues with enough disagreement to estimate w from Equation 2. When c is almost equal to μ_- , that ratio is unstable and should not be used. Collect additional subjective quantiles to test the pooled distribution itself: point estimates plus one interval cannot identify an unrestricted B_t . Measurement error in elicited quantiles should be retained, and parameter estimation should use the probe trials rather than back-calculation from end-spurt timing.

CORE CREDIBILITY-BY-SIGNED-BIAS EXPERIMENT

The core test should use familiarized adults able to complete repeated laboratory 20-km cycling time trials on a calibrated ergometer. Recruitment should cover the population to which inference is intended; training history and sex should be recorded, and subgroup conclusions should depend on adequate representation. A separate familiarization session establishes equipment use, a pacing reference and judgment procedures. Eligibility and health screening should be specified before recruitment.

The repeated-measures design has six conditions: high or low source credibility crossed with accurate, nearer-finish or farther-finish feedback. All trials cover the same true distance and virtual course. Conditions should be assigned in a balanced sequence, with standardized time of day, preparation, environment and recovery of at least 48 hours, lengthened when recovery criteria are not met. Source training should precede each session; belief checks must detect whether earlier sessions have degraded the intended contrast. A parallel-group replication is appropriate if carryover prevents a credible crossover comparison.

Until 75% of true distance is completed, all conditions receive the same accurate distance feedback. The assigned source and numerical intervention then begin without changing the other display elements. Let D be true remaining distance and $\delta > 0$ a pilot-selected, ethically approved proportional bias. The three displayed centres are D , $(1 - \delta)D$ and $(1 + \delta)D$. This produces equal-magnitude signed errors at each actual position, preserves a monotone countdown and avoids falsely displaying completion before the real finish. The source change occurs in every condition, including accurate controls. The finish signal is identical and occurs only at true completion.

The pilot must select δ using belief uptake, detection and safety rather than the size of a desirable performance effect. No numeric bias magnitude is asserted to be universally safe or effective. A design that scales distance continuously also changes the apparent countdown rate; the intervention should therefore be described as biased distance feedback, not as an isolated static endpoint displacement. Clock, speed and route-landmark feedback should either be hidden throughout all conditions or held identical and explicitly treated as redundant information. Neutral instructions and the absence of virtual opponents reduce comparison effects. Analysts who identify acceleration events should be masked to condition.

MEASUREMENTS AND ACCELERATION ADJUDICATION

Record calibrated power, cadence, actual distance and time with synchronized timestamps at 1 Hz or faster. Table 3 separates informational measurements from behavioural and physiological observations. Instrument error, transmission

loss and the conversion from ergometer power to virtual distance must be documented.

TABLE 3. OPERATIONAL SEPARATION OF CONSTRUCTS AND OUTCOMES. ALL MEASUREMENT CHOICES ARE PROSPECTIVE.

Quantity	Operational measurement	Main interpretive restriction
Believed remaining distance	Point estimate and specified subjective quantiles at sparse probes.	Posttrial recall alone cannot establish the timing of a belief change.
Cue credibility	Independent source-reliance judgments and pre/post cue uptake during validation.	An early acceleration is not a credibility measurement.
Cue precision	Perceived interval width at fixed cue centre and source.	Displayed precision may differ from perceived precision.
Perceived sustainable distance	Expected sustainable duration at specified P^* , converted using the fixed course mapping.	Distinct from a physiological capacity estimate.
Acceleration onset	First qualifying power increase in the prespecified terminal risk window.	Does not prove intention or successful maintenance to the finish.
Physiological sensitivity measures	Separately calibrated critical power, W' and model-derived W' balance.	Model-dependent; post-cue balance is not a pre-intervention confounder.

For the core trial, define the terminal risk window as $s \in [0.75, 1)$, where s is the fraction of actual distance completed. Set each session's reference power to the median over $s \in [0.50, 0.70]$, before the assigned intervention. A candidate operational definition uses a trailing 5-s median power trace, an increase of at least 5% above that reference, and maintenance above the threshold for 10 s. These values specify a testable starting rule, not validated universal cut-offs. Signal-only pilot recordings must confirm detectability relative to measurement error and normal variability; any changes must be locked before condition-labelled performance data are examined.

Define onset as the first threshold crossing that subsequently meets the duration requirement. Retain the crossing time rather than the end of the confirmation interval. The last eligible crossing is 10 s before finish; shorter final bursts constitute a separate outcome. A trial already above threshold at 75% completion has acceleration present at risk-window entry and contributes an onset at 0.75 to the primary restricted-position outcome. This retains athletes whose power increase precedes entry into the terminal window.

Identify all qualifying increases, not only the first. Record whether each is followed by a decline below the reference before completion and whether elevated output is maintained to the finish. The primary outcome is onset of a qualifying terminal-window acceleration; successful maintenance and later slowing are separate secondary outcomes. A full-trial surge analysis captures unusually early commitment outside the terminal window. Nearer-finish feedback can therefore shift onset without requiring every early increase to be followed by slowing.

Repeat event detection using trailing 3- and 10-s windows, 3% and 7% thresholds, and 5- and 15-s duration requirements as declared sensitivity analyses. Treat the prespecified rule as primary and report all alternatives without selecting the most favourable result. A condition-by-distance analysis of the complete power trace tests whether the conclusion depends on a threshold definition.

SEPARATING BEHAVIOURAL AND MECHANISTIC INFERENCE

The main performance sessions should minimize questioning. A separate randomized substudy should compare sparse belief probes with a matched motor-response control and a no-probe condition to quantify measurement reactivity. Identical questions across treatment conditions do not by themselves show that questioning has no effect.

For a temporal mechanism test, use a controlled, submaximal lead-in followed by one source cue, a prompt distance judgment and then a freely chosen power segment. Collect power continuously and timestamp the cue and response. Compare the belief-probe trials with matched trials requiring an equally brief nonspatial response. If power changes before the distance response is complete, temporal precedence of the measured belief has not been demonstrated. A change in distance judgment followed by acceleration is consistent with the decision criterion, but response latency and concurrent physiological or attentional changes prevent treating that sequence alone as proof of mediation.

Separate validation trials establish whether the intervention changes distance judgments; they cannot establish within-trial mediation in performance sessions without probes. Posttrial interviews document cue detection and perceived trust. Participants who detect bias remain in the assigned-condition analysis, with detection treated as an intervention consequence rather than a reason for exclusion.

PRIMARY ESTIMAND AND STATISTICAL ANALYSIS

Let U be actual-distance fraction at qualifying onset, assigning $U = 1$ when no onset occurs before the finish. The primary summary is the restricted mean onset position $E(U)$ together with the cumulative probability of onset by each true position. A smaller $E(U)$ indicates earlier acceleration or a greater probability of acceleration within the risk window; the analysis must distinguish those components. Trials without onset remain informative and contribute the finish value to this restricted summary.

The primary randomized contrast is the difference in nearer-versus-farther onset displacement between the two assigned credibility conditions,

$$\Delta = \{E(U_{\text{high,near}}) - E(U_{\text{high,far}})\} - \{E(U_{\text{low,near}}) - E(U_{\text{low,far}})\}. \quad (9)$$

The expected sign near an informative decision boundary is negative. Comparisons with accurate feedback and the two component contrasts are prespecified secondary analyses. This is the total effect of assignment to source training and feedback; it is not a causal effect of an inferred w .

For event analysis, fit a discrete-distance hazard model using fixed bins of actual position, with source assignment, the three-level bias factor, their interaction, trial period and a flexible function of actual-distance position. Participant effects account for repeated sessions; random treatment slopes require sufficient observations for stable estimation. Derive marginal cumulative-onset curves and restricted means, with participant-level resampling for uncertainty. Absolute onset probabilities and distance displacement quantify the effect beyond its hazard coefficient. Finish provides administrative right censoring for the event analysis; it is an observed boundary rather than a missing onset estimate.

Analyse permanent trial withdrawal separately, with reasons and sensitivity analyses for informative withdrawal. Transient slowing is not a competing event because subsequent acceleration remains possible. Missing power segments capable of concealing onset should not be coded as no acceleration; bound their effect by plausible earliest and latest onset positions. Prespecify treatment of technical failure, repeated sessions and withdrawal without inspecting treatment effects.

The total-effect model should not adjust for power expenditure or W' balance after the cue. Earlier feedback-induced pacing can change those quantities, making them potential mediators rather than pre-intervention confounders. Pre-cue power history and pre-cue capacity measurements can support precision adjustment or prespecified moderation. Analyses of post-cue belief and capacity pathways are secondary and require explicit causal assumptions; they cannot be interpreted as randomized mediator effects.

Enrollment for the six-condition experiment is determined by prospective simulation using participant variability, non-acceleration frequency, carryover, withdrawal and the smallest informative interaction. The simulation specifies the data-generating process, estimand, estimator, interval coverage and detection probability (Morris et al., 2019). Several input sets are required to assess uncertainty in pilot estimates, with Monte Carlo error included in the precision calculation. Enrollment and statistical power remain prospective quantities because this article contains no recruited cohort.

EXPERIMENTS FOR THE REMAINING PREDICTIONS

P1 requires a separate accurate-feedback experiment with equal mean distance and source identity but narrow versus broad displayed uncertainty. Pilot checks must demonstrate a change in the relevant subjective upper quantile. Wider visual intervals should have matched legibility and animation; otherwise an attention effect can mimic an uncertainty response. The core bias experiment cannot substitute for this contrast.

For P4, restore an accurate cue at two independently randomized late-race positions, with an absent-cue control. The restoration times must precede the eligible onset horizon and be selected before testing. Onset should be analysed on a common actual-distance scale with appropriate time-varying exposure, rather than comparing only elapsed time after restoration. Pre-restoration power and capacity are measured before intervention. A matched display-onset control with no distance content separates alerting from a change in judged remaining distance.

For P5, cross digital-cue availability with an independent accurate position signal, such as an unambiguous lap count on a fixed virtual circuit. The second signal should convey position without adding an opponent or altering task difficulty. Test whether the availability contrast is attenuated when the other cue preserves endpoint judgment. A separate omission session is therefore informative about availability, but cannot isolate credibility without these controls.

ETHICAL SAFEGUARDS

The prospective cycling experiments require institutional approval and consent procedures appropriate to authorized incomplete disclosure. Participants receive advance notice that some display information may be altered; the exact

contrast remains concealed only when the approved design permits it. They retain the right to stop, with health screening, supervised exercise and predefined termination criteria. Bias magnitude and intensity limits require pilot evidence and ethics approval; the theory does not justify unrestricted exertion or covert intervention in real competition.

Debriefing should occur as soon as participation is complete, or earlier when required for safety or withdrawal. Any delay across crossover sessions must be specifically approved because immediate disclosure could invalidate later conditions. Explain the altered feedback, its purpose and the approved options concerning data withdrawal. No ethics approval is claimed for these unperformed experiments.

SPORT-SPECIFIC INTERPRETATION AND LIMITATIONS

REACH AND PRACTICAL MEANING

On a velodrome, lap counts may make a digital display redundant. In swimming, wall contacts provide discrete distance information but turns interrupt effort. Irregular terrain can make speed changes poor indicators of intentional acceleration, while in tactical racing an opponent may elicit an increase that is costly yet competitively necessary. Event objectives and interpersonal demands must therefore enter any field extension (Casado et al., 2021; Konings and Hettinga, 2018).

For coaching assessment, record the cues available near the finish and compare the athlete's judged position with accurately mapped course distance. A missing end spurt may reflect endpoint uncertainty, limited physiological tolerance or tactical choice. Position estimates and power traces distinguish those possibilities more directly than a general motivation judgment. The cycling evidence does not establish deception as a training intervention or guarantee faster completion from an additional display.

CONCLUSION

Endpoint credibility concerns the influence of a distance cue on the athlete's judgment that a higher power can be maintained until completion. Linear pooling specifies that influence through the cue weight and retains disagreement between cue-implied and pre-cue distributions. The acceptance condition compares an upper subjective distance quantile with perceived sustainable distance. In the analytical example, identical cue content gives quantiles of 2.50 and 2.20 km at weights of 0.20 and 0.80; a sustainable distance of 2.30 km satisfies the condition only at the larger weight.

Empirical evaluation requires separate manipulations of credibility, signed bias and uncertainty, distance judgments independent of the acceleration outcome, and inclusion of trials without a terminal power increase. Changes in cue-dependent judgments must add predictive information beyond learned pacing, attention and competition. The equations establish mathematical consequences of the stated beliefs; their application to athletes depends on those prospective tests and on the capacity to execute the higher output.

DECLARATIONS

CONFLICT OF INTEREST

The author declares that there are no conflicts of interest related to this study.

DATA AVAILABILITY

The data supporting the findings of this study are available from the corresponding author upon reasonable request.

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