

HEAT ACCLIMATION, SESSION WORK AND THERMAL STRAIN: THREE COMPARISONS OF ENDURANCE ADAPTATION

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ABSTRACT

Heat acclimation alters sweating, cardiovascular function and body-temperature regulation during exercise. These adaptations also change the thermal strain generated by subsequent sessions, so a constant exercise prescription need not deliver constant internal exposure. McDonald and colleagues' Bayesian analysis estimated an end-exercise heart-rate reduction of 17 beats/min (90% credible interval: 14–19), a core-temperature reduction of 0.43°C (0.36–0.48) and a time-trial improvement of 3.1% (1.8–4.5). Individual experiments delimit these average responses: isothermic exercise reduced exercise requirements in a 24-man comparison, whereas hot-water immersion improved a 5-km trial in heat without a corresponding temperate-condition improvement. Three causal comparisons are defined: assignment to a complete acclimation programme, hot versus thermoneutral immersion after a common exercise prescription, and exercise versus passive heating under temperature-targeting rules. Mechanical work and the core-temperature time integral describe separate exposure dimensions; physiological adaptation and task performance remain separate outcomes. A temporal diagram identifies how adaptation and fatigue connect successive sessions and why adjustment for achieved work or temperature can alter an assignment-based comparison. Small samples, incomplete exposures and dependence between articles from the same cohort constrain inference.

Keywords: heat acclimation; physiological adaptation; transformation processes; external work; thermal strain; causal estimand.

INTRODUCTION

Transformation processes in kinesiology include changes in sweating, cardiovascular regulation and fluid balance that support exercise in heat. These responses need not develop at the same rate or produce the same improvement in endurance performance (Périard et al., 2015). Here, *physiological transformation* denotes a change after repeated heat exposure in a prespecified response measured under standardized conditions. Persistence requires a later assessment. A resting-temperature reduction, altered sweating response and faster time trial consequently describe different aspects of acclimation.

McDonald et al. analysed 211 articles using Bayesian multilevel models. Posterior mean changes were –17 beats/min for end-exercise heart rate (90% credible interval, CrI: –19 to –14), –0.43°C for end-exercise core temperature (90% CrI: –0.48 to –0.36), 5.6% for plasma-volume expansion (90% CrI: 3.8–7.0%) and 3.1% for time-trial improvement (90% CrI: 1.8–4.5%) (McDonald et al., 2025). These aggregate estimates quantify average responses across the included literature. They cannot identify which individual athlete will improve or establish the superiority of a particular heating method.

Exercise produces metabolic heat. Ambient temperature, humidity, airflow, clothing, hydration and the athlete's heat-loss responses determine the resulting thermal strain (Périard et al., 2021). As sweating and temperature regulation change,

a fixed exercise prescription can generate a different core-temperature trajectory. Maintaining a specified trajectory can instead require changes in exercise power, rest or passive exposure. Acclimation therefore affects both the athlete's response and the exposure generated by later sessions.

Gibson et al. compared fixed-intensity and isothermic exercise (Gibson et al., 2015); Zurawlew et al. quantified post-exercise immersion using core-temperature area above 38.5°C (Zurawlew et al., 2016); and Kissling et al. compared exercise, immersion and sauna using temperature-targeting procedures (Kissling et al., 2022). Each comparison answers a different question. A complete programme contrast includes its exercise, heating, recovery and tolerability consequences. A common exercise prescription isolates the assignment to added heating more closely. Temperature-targeting strategies compare routes to an intended internal exposure, while retaining each route's exercise and tolerability consequences. Keeping these contrasts distinct prevents one result from being used to support all three interpretations.

MECHANICAL WORK AND CORE-TEMPERATURE EXPOSURE

EXTERNAL MECHANICAL WORK

For cycling, let u denote elapsed time in seconds and τ_s the duration of session s in seconds. Recorded external mechanical work is

$$W_s = \int_0^{\tau_s} P_{\text{ext},s}(u) du. \quad (1)$$

Power is expressed in watts and W_s in joules. The integral describes recorded external work, not the complete training stimulus: the intensity distribution, recruitment, recovery and internal metabolic cost remain relevant. In running, distance does not provide a mechanically equivalent substitute. Modality-specific records should accompany any claim of preserved exercise exposure.

Body mass and surface area affect thermoregulatory comparisons. Cramer and Jay demonstrated why matching only relative aerobic intensity can distort comparisons between groups of different body size (Cramer and Jay, 2014). Work records should therefore be accompanied by the exercise modality, metabolic heat production and body-size description. A single normalization cannot account for all differences in heat production and heat exchange.

TEMPERATURE-TIME INTEGRAL

Using the same seconds-based time variable, a temperature-area descriptor in °C·min can be defined as

$$D_{\theta,s}(T_{\text{ref}}) = \frac{1}{60} \int_0^{\tau_s} [T_{c,s}(u) - T_{\text{ref}}]_+ du, \quad [a]_+ = \max(a, 0). \quad (2)$$

The factor 1/60 converts seconds to minutes. $T_{c,s}$ is measured core temperature, and T_{ref} is an analytical reference fixed before outcome analysis. The integral describes accumulated temperature elevation above that reference. It is a thermal-strain descriptor; its threshold does not define a physiological transition or a stopping rule.

An absolute reference compares exposure above the same temperature in every session. A reference set relative to the pre-exposure temperature instead compares the magnitude of temperature elevation. These definitions answer different questions and should remain identifiable throughout the analysis. Sensitivity analysis at adjacent prespecified reference temperatures can establish whether exposure rankings depend on the chosen threshold. Selecting the reference to maximize its association with adaptation would bias that association.

Equal integrals can arise from different peaks, durations, starting temperatures and rates of temperature increase. A smaller integral can also reflect a lower initial temperature without a smaller exercise-induced rise. Interpretation therefore requires the measurement site, sampling interval, sensor limitations, processing rules and the complete available temperature trace. Missing intervals must remain distinguishable from zero thermal exposure; measurement coverage and any interpolation require explicit documentation. Skin temperature, hydration and heat production provide additional information that Equation (2) does not contain. The integral has no established role here as an individual prescription or safety limit.

A fixed-work prescription constrains W_s while allowing the thermal trace to evolve. A temperature-targeting prescription constrains selected properties of that trace while permitting work or passive exposure to change. These are protocol-dependent tendencies, not universal directions. Starting temperature, fitness, airflow and accumulated fatigue can alter the mapping. The relevant object is the session sequence $(W_s, D_{\theta,s}, T_{c,s}(u))$, accompanied by the recorded conditions that generated it.

ADAPTATION AND PERFORMANCE ACROSS HEATING METHODS

EXPOSURE CONTROL AND PHYSIOLOGICAL ADAPTATION

In 24 men, Gibson et al. compared fixed intensity with continuous and progressive isothermic acclimation over five and ten sessions. Isothermic conditions required shorter exercise duration and lower average intensity, with no detected superiority of the principal measured adaptations (Gibson et al., 2015). This demonstrates different exercise requirements under alternative constraints; an absence of detected differences does not by itself demonstrate statistical equivalence.

McIntyre et al. randomized 27 men, nine per group, to six days of post-exercise immersion, exercise in heat or work-matched temperate exercise. Immersion and exercise in heat produced similar core-temperature areas, while immersion elicited larger thermal adaptations (McIntyre et al., 2021). In their 12-day comparison, the final analysis comprised seven male completers per condition. Immersion produced a broader thermal adaptation profile, while endurance capacity did not differ detectably between interventions (McIntyre et al., 2022). Completion-based analysis restricts the programme interpretation, and the capacity result does not establish equivalence.

Kissling et al. used a randomized crossover design in 13 participants, including five women. Each condition comprised five days with 60-min planned exposures. Heated conditions targeted a 1.5°C rectal-temperature rise using 40°C water, a 55°C sauna or exercise at 40°C ; thermoneutral immersion was the control. Mean sauna duration was 42 ± 10 min (SD), demonstrating a delivery difference despite the intended duration (Kissling et al., 2022). Temperature targets, tolerability and achieved exposure consequently require separate interpretation.

Neal et al. compared euhydration and restricted fluid intake in eight men under matched isothermal strain. Permissive dehydration did not notably increase acquisition or alter decay of acclimation (Neal et al., 2016). Schleh et al. likewise found no additional adaptation from promoted dehydration during their short-term intervention (Schleh et al., 2018). These comparisons address hydration under the tested thermal conditions; they do not establish a hydration-independent response across all acclimation methods.

PERFORMANCE TRANSFER AND UNCERTAINTY

James et al. compared five-day acclimation with a work- and power-matched control in 17 runners. Mean hot-condition 5-km time changes were -6.2% and -0.6% , respectively; the between-method contrast had $P = 0.029$. Several conventional physiological determinants changed similarly in the two conditions (James et al., 2017). The performance difference identifies an effect in that task, while its physiological mediator remains unresolved.

After six days of hot immersion, Zurawlew et al. found a 4.9% improvement in a 5-km trial at 33°C , without a corresponding improvement at 18°C (Zurawlew et al., 2016). A treatment-by-environment interaction is required to establish environment dependence; significance in one condition and nonsignificance in another is insufficient. Karlsen et al. found no additional cool-condition cycling benefit after outdoor heat acclimatization (Karlsen et al., 2015). In Keiser et al.'s seven-man crossover experiment, benefits occurred in heat without temperate transfer, and acute plasma-volume expansion did not produce the same performance benefit (Keiser et al., 2015).

McCleave et al. examined 26 runners and identified delayed temperate performance benefits in small intervention groups (McCleave et al., 2017). Assessment timing must therefore be specified before comparing immediate and delayed responses. The article used magnitude-based inference, whose probability labels cannot be interpreted as conventional posterior probabilities. Welsh and Knight's critique identifies statistical reasons for caution when interpreting those labels (Welsh and Knight, 2015).

Oberholzer et al. measured haemoglobin-mass changes of $+34 \pm 36$ g in the heat group and $+2 \pm 33$ g in control (means $\pm$ SD); the group-by-time interaction had $P = 0.061$ (Oberholzer et al., 2019). The dispersion and interaction indicate uncertainty about a heat-specific increase. In the same 21-cyclist cohort, Mikkelsen et al. found cool-condition time-trial mean power increasing from 298 ± 6 to 315 ± 6 W with heat training and from 294 ± 11 to 311 ± 10 W in control (means $\pm$ standard errors). Each group improved by 17 W, without additional benefit from heat training (Mikkelsen et al., 2019). The two articles constitute complementary outcomes from one cohort.

THREE TARGETS OF INFERENCE

Let $\mathcal{P}$ denote the prespecified population of eligible athletes, e the assessment environment and f the follow-up interval from the final planned exposure. $Y(e, f)$ is the task outcome, such as time to complete a fixed distance or mean power during a fixed-duration trial. Its scoring direction is declared before analysis. Physiological responses, denoted R_k , are separate endpoints expressed in their respective units. Expectations refer to $\mathcal{P}$, with common assessment procedures across randomized groups.

ASSIGNMENT TO A PRACTICAL PROGRAMME

For assignment $Z \in \{H, C\}$ to a specified heat programme or comparator, the programme estimand is

$$\psi_{\text{programme}}(\mathcal{P}, e, f) = \mathbb{E}_{\mathcal{P}}[Y^{Z=H}(e, f) - Y^{Z=C}(e, f)]. \quad (3)$$

The superscript denotes programme assignment, including its specified adjustment and stopping rules. This is an intention-to-treat target: missed sessions, early termination and accompanying changes in recovery remain consequences of assignment. Follow-up should still be sought after discontinuation. Random allocation permits estimation of the assignment contrast under well-defined interventions, no relevant interference and adequate handling of missing outcomes. Randomization does not remove bias from informative loss to follow-up. Adherence effects ask an additional question and require additional assumptions (Hernán and Robins, 2017).

ADDED HEAT UNDER A COMMON EXERCISE PRESCRIPTION

Let w denote an exercise schedule with prespecified modality, duration and intensity distribution, common to both arms. Let $g_{\text{hot}}(w)$ and $g_{\text{neutral}}(w)$ denote assignment to that schedule followed by hot or thermoneutral immersion, including the respective temperature, depth, duration, hydration and termination rules. Define

$$\psi_{\text{added}}(\mathcal{P}, e, f; w) = \mathbb{E}_{\mathcal{P}}[Y^{g_{\text{hot}}(w)}(e, f) - Y^{g_{\text{neutral}}(w)}(e, f)]. \quad (4)$$

This estimand compares added-heat protocols under a shared *prescribed* exercise schedule. It does not condition on equal work achieved after allocation. If the added exposure changes the ability to complete later exercise, that difference belongs to the assigned protocol's effect and must be recorded; restricting analysis to participants who happened to preserve work would select on a post-randomization event.

Zurawlew et al. used a common 40-min treadmill run at 65% of maximal oxygen uptake in 18°C, followed by neck-level immersion for up to 40 min in either 40°C water ($n = 10$) or 34°C water ($n = 7$). Thermoneutral immersion controlled exercise-training and hydrostatic effects. Early exits shortened hot-immersion duration (Zurawlew et al., 2016). The comparison therefore identifies assignment to temperature-specific immersion, including its tolerability and duration consequences, rather than temperature isolated from every associated pathway.

ALTERNATIVE THERMAL-TARGET STRATEGIES

Let $g_A(\mathcal{H}_s)$ and $g_P(\mathcal{H}_s)$ denote exercise-based and passive-heating algorithms acting on the available session history $\mathcal{H}_s$. Each specifies the target trajectory, measurement site, adjustment frequency, permissible changes, maximum planned duration and independent safety rules. The strategy estimand is

$$\psi_{\text{strategy}}(\mathcal{P}, e, f) = \mathbb{E}_{\mathcal{P}}[Y^{g_A}(e, f) - Y^{g_P}(e, f)]. \quad (5)$$

In Kissling et al.'s controlled-hyperthermia sessions, cycling power or immersion depth was adjusted as rectal temperature approached the target (Kissling et al., 2022). These are different interventions even when they target the same temperature rise. The target, permitted adjustments, background training, hydration and assessment timing define the contrast. Exercise undertaken to attain the target remains part of the exercise-based intervention.

Comparable intended targets are intervention definitions; comparable achieved trajectories are manipulation checks. A protocol should prespecify acceptable differences in the trajectory, peak, duration and temperature-area descriptor using measurement reliability and a physiological rationale. Passing only an integral-based criterion is insufficient. If comparability fails, the randomized result still estimates the contrast between the assigned algorithms, but cannot support a claim about methods delivering comparable realized thermal strain. Excluding participants because they missed the target would introduce selection.

EXPOSURE FEEDBACK AND ADJUSTMENT

Figure 1 orders the feedback in time. Exposure in session s changes the state entering session $s + 1$; that state may influence both the next achieved exposure and the final outcome. A feedback process can therefore be represented without a directed cycle by distinguishing successive occasions.

Achieved work and thermal strain occur after programme assignment. Conditioning on either can remove part of the programme effect or introduce associations through shared causes of exposure and outcome. Fatigue can be affected by one session and subsequently influence both later exposure and performance, creating time-dependent confounding. Ordinary covariate adjustment is generally inadequate for this structure. Longitudinal g-computation, inverse-probability-weighted marginal structural models or g-estimation require consistency, sequential exchangeability, positivity, adequate

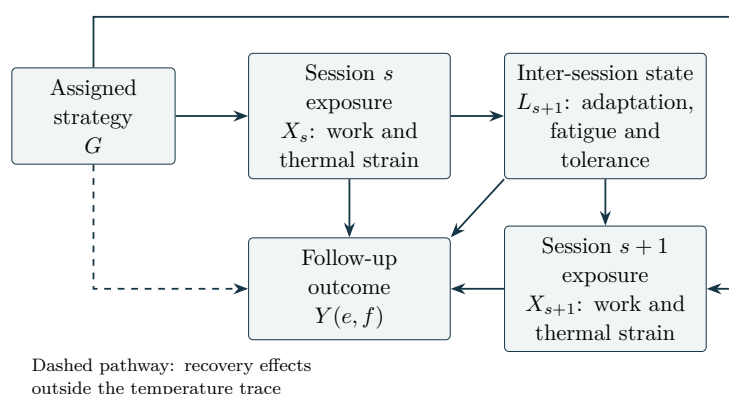


FIGURE 1. SESSION ORDERING IN HEAT ACCLIMATION. G DENOTES ASSIGNED STRATEGY; X_s AND X_{s+1} COMPRISE WORK AND THERMAL STRAIN IN SUCCESSIVE SESSIONS; L_{s+1} COMPRISES ADAPTATION, FATIGUE AND TOLERANCE; $Y(e, f)$ DENOTES PERFORMANCE IN ENVIRONMENT e AT FOLLOW-UP f . ARROWS ENCODE CAUSAL ASSUMPTIONS. THE DASHED PATH INCLUDES RECOVERY EFFECTS OUTSIDE THE RECORDED TEMPERATURE TRACE. PRE-ASSIGNMENT COMMON CAUSES, ADDITIONAL SESSIONS AND MEASUREMENT ERROR ARE OMITTED. THE DIAGRAM IDENTIFIES ADJUSTMENT PROBLEMS RATHER THAN FURNISHING A COMPLETE CAUSAL IDENTIFICATION MODEL.

measurement and appropriate modelling (Daniel et al., 2013). Those assumptions cannot be established from aggregate article summaries. Randomized assignment remains a separate target of inference; mediation requires additional measurements and assumptions.

TABLE 1. INTERVENTION ASSIGNMENT, EXPOSURE CHECKS AND HANDLING OF INCOMPLETE SESSIONS FOR EACH CAUSAL COMPARISON.

Target	Intervention contrast	Exposure check	Incomplete exposure
Programme	Assignment to heat programme versus defined alternative	Record all work, thermal strain and recovery consequences	Retain assignment; seek outcomes after discontinuation
Added heat	Shared exercise prescription plus hot versus neutral immersion	Verify prescribed/achieved exercise; record immersion depth and duration	Retain assignment; do not select work-preserving completers
Thermal-target strategy	Exercise-based versus passive target algorithms	Assess prespecified trajectory and duration comparability	Retain assignment; report target failures and safety exits

REQUIREMENTS FOR INTERPRETING AN ACCLIMATION COMPARISON

ALLOCATION, EXPOSURE DELIVERY AND MEASUREMENT

Programme assignment, added immersion and temperature-targeting strategies are distinct comparisons. A parallel randomized design compares them without assuming that acclimation was washed out. In a crossover design, the relevant physiological state must return sufficiently close to its pre-intervention level for the next condition to remain interpretable. An elapsed calendar interval alone cannot establish that recovery.

Allocation concealment protects the intervention comparison, while assessor masking limits measurement-related bias. A common background training schedule and test familiarization reduce differences unrelated to the assigned heating method. Sensor agreement, target attainability and the precision of the performance test determine whether the intended contrast can be delivered and measured. Exposure duration and follow-up timing must be set before recruitment; choosing them after inspecting group differences changes the question being tested.

Each session record should contain prescribed and achieved duration, exercise power or modality-specific output, intensity distribution, temperature trace, measurement coverage, ambient conditions, hydration, immersion depth where applicable, and stopping reasons. Repeated measurements under the same conditions establish the repeatability of Equation (2). Its interpretation also depends on reference-temperature sensitivity and its association with prespecified physiological responses after accounting for initial temperature and exercise exposure. Prediction requires a separate sample or held-out observations; association within the fitted sample is insufficient to justify an individual prescription.

Three tests distinguish exposure feedback from performance transfer. First, fixed-work sessions can be compared for changes in the complete temperature trace while measurement conditions remain constant. Second, temperature-targeting methods can be compared for trajectory, peak and duration alongside their work requirements. Third, physiological changes can be evaluated separately from a performance contrast whose confidence interval excludes a prespecified worthwhile benefit. Each test requires the relevant between-condition estimate and uncertainty; a nonsignificant result alone cannot establish absence of an effect.

OUTCOME DEFINITION AND STATISTICAL CONTRASTS

A performance endpoint must identify the task, environment and follow-up: for example, mean power during a fixed-duration cycling trial in heat. A standardized-work heat-response test measures physiological adaptation separately from self-paced performance. Resting temperature and the exercise-induced rise remain distinct measurements, as do heart rate at fixed work and heart rate during a self-paced trial. Benjamin et al.'s meta-analysis treats time to exhaustion, time-trial performance, power and maximal oxygen uptake as distinct outcomes (Benjamin et al., 2019).

In a parallel trial, the primary contrast is a between-group effect adjusted for the pre-intervention value of the outcome, accompanied by a confidence interval and the prespecified worthwhile-effect threshold. Repeated physiological measurements can be analysed with participant-level mixed models containing occasion and group-by-time terms. Transfer between hot and cool assessments requires a treatment-by-environment interaction. Secondary outcomes require an explicit multiplicity strategy. Equivalence requires a justified margin and a sufficiently precise interval lying within that margin; a nonsignificant difference is insufficient.

Sample-size determination depends on the chosen contrast, outcome reliability, worthwhile effect and anticipated attrition. These quantities must be established for the intended population before recruitment. Reasons for missing outcomes and follow-up after programme cessation are necessary to interpret the assignment effect. Imputation and sensitivity analysis require explicit assumptions about missingness. Adherence analyses answer a different question and must retain that distinction from the assignment-based primary comparison (Hernán and Robins, 2017).

PARTICIPANT CHARACTERISTICS, PERSISTENCE AND WITHDRAWAL

The small samples and male predominance in much of the selected evidence restrict generalization. Mee et al.'s eight-man/eight-woman comparison supports investigating temporal differences, but not fixed schedules for all men or women (Mee et al., 2015). Body size, fitness, prior acclimation and relevant hormonal context should be recorded. Subgroup conclusions need a planned interaction and adequate precision; separate within-group significance tests do not demonstrate subgroup differences.

Willmott et al. found similar adaptations with once- and twice-daily schedules under their tested conditions (Willmott et al., 2018). Session count, calendar duration and spacing therefore need separate descriptions. Daanen et al. identify response-specific decay and re-induction, with heat-response assessments capable of supplying further exposure (Daanen et al., 2018). Assessment sessions belong in the exposure record when persistence is the outcome.

Heating targets and withdrawal decisions require supervised procedures justified for the participants and intended environment. Racinais et al. emphasize heat preparation, hydration and prevention of heat illness (Racinais et al., 2015). A temperature-area integral cannot determine a universal safe ceiling. Medical withdrawal, early exits and missed sessions are consequences of intervention delivery and must remain identifiable when estimating a programme effect.

CONCLUSIONS

Heat acclimation changes physiological responses and can thereby alter the thermal strain generated by later sessions. Assignment to a whole programme, added heating after common prescribed exercise and alternative temperature-targeting methods identify different effects. Equal prescribed work does not establish equal achieved work, and equal temperature area does not establish equal temperature trajectories. The available comparisons also separate thermal adaptation from performance: some hot-condition trials improve, whereas cool-condition gains and endurance-capacity differences remain inconsistent or uncertain. Interpretation requires distinct physiological and performance endpoints, the assessment environment, follow-up timing and uncertainty around the relevant between-group contrast. Complete exposure records preserve the distinction between the intended intervention and what participants tolerate.

DECLARATIONS

CONFLICT OF INTEREST

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

DATA AVAILABILITY

Not Applicable.

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