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# When does the peak countermovement jump performance occur? Mapping peak-trial timing across six clustered repetitions

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**ABSTRACT**

Force-plate countermovement jump (CMJ) testing is widely used to assess neuromuscular status, yet monitoring interpretation often relies on best-trial values from abbreviated protocols, potentially biasing conclusions when the session maximum occurs beyond the sampled attempts. We mapped peak-trial timing across a six-trial clustered (3 + 3) CMJ protocol for seven outcome and strategy force–time metrics, and tested whether this peak-trial pattern was preserved between personal best (PB) and typical state (TS) monitoring sessions, both defined using modified reactive strength index (mRSI) as the primary selection variable. Retrospective dual-force-plate data (1000 Hz; August 2020–December 2025) were analysed from 314 athletes who performed two sets of three maximal no-arm-swing CMJs (5–10 s between repetitions; 60–90 s between sets). Peak-trial distributions deviated from uniform for all metrics (all Bonferroni-adjusted $p \leq 3.4 \times 10^{-3}$; Cramér's V = 0.12–0.26). On PB days, the most frequent peak occurred on repetition 4 for output metrics (25.8–30.3%) and propulsive power (29.3%), on repetition 6 for braking force/power (31.2–34.1%), and on repetition 1 for stiffness (27.4%) and propulsive force (31.8%). Critically, peak-repetition distributions were nearly identical between PB and TS days for every metric (all $p \geq 0.386$; Cramér's $V \leq 0.091$).

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Neuromuscular monitoring; vertical jump; force–time variables; testing protocol; performance profiling; testing standardisation

## Introduction

The increasing emphasis on evidence-informed decision-making in high-performance sport has accelerated the adoption of objective monitoring to support training prescription, recovery management, and injury-risk mitigation (Bourdon et al., 2017; Halson, 2014). In parallel, technological development means it is now feasible to collect kinetic and kinematic data during countermovement jump (CMJ) performance outside laboratory settings using portable force-plate systems that have demonstrated acceptable agreement with reference devices (Plakoutsis et al., 2023; Raymond et al., 2018). Within this landscape, the CMJ remains a central neuromuscular assessment because of its practicality and established measurement properties across clinical and sport settings. Accordingly, it is frequently used by practitioners as an objective tool to monitor neuromuscular readiness and preparedness (Alba-Jiménez et al., 2022). It also supports athlete management and generates population-specific normative benchmarks for performance profiling and return-to-sport benchmarking (Bourdon et al., 2017; J. J. McMahon et al., 2018). Because these reference values are intended to represent maximal neuromuscular capability, CMJ protocols commonly prioritise the best recorded trial (Claudino et al., 2017). However, CMJ monitoring protocols remain heterogeneous and there is little guidance on how to organise repetitions and rest to ensure maximum CMJ performance is captured (Badby et al., 2025). Addressing this gap requires mapping when CMJ variables peak within a standardised assessment session to inform protocol standardisation (Alba-Jiménez et al., 2022; Petrigna et al., 2019).

Force-plate CMJ testing is valued not only for output scores but because it also enables practitioners to quantify underlying force–time characteristics that may change without necessarily altering the headline performance result. For example, CMJ force–time analysis is commonly implemented to monitor changes in movement strategy that are interpreted as indicative of neuromuscular alterations associated with fatigue or supercompensation (J. J. McMahon et al., 2018). At the same time, vertical jump testing is methodologically and interpretively complex: many metrics can be derived,

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but only some are reliable, and optimal variables may vary by population and protocol (Anicic et al., 2023). Consequently, contemporary discussions in applied and academic sports science increasingly centre on the optimisation of CMJ monitoring workflows – spanning metric selection, phase definition, and processing decisions – because these choices can materially affect the validity and sensitivity of derived outcomes (Anicic et al., 2023; J. J. McMahon et al., 2018).

Despite extensive attention to measurement and analysis, uncertainty remains regarding the structure of the CMJ testing protocol itself. Reviews have highlighted considerable heterogeneity in CMJ testing procedures, including discrepancies in execution instructions, device characteristics, and the number of jumps performed (Badby et al., 2025; Petrigna et al., 2019). Similarly, work synthesising neuromuscular monitoring practices has noted that, although CMJ assessment is widely used, there is no general agreement regarding a unified CMJ protocol, and trial-number selection varies between studies (Alba-Jiménez et al., 2022). Within primary research, the field commonly employs reduced trial volumes: for instance, three maximal CMJs are frequently used in force-plate studies (Anicic et al., 2023; J. J. McMahon et al., 2018), whereas alternative monitoring approaches have implemented a five-jump CMJ test format (Jordan et al., 2022). Claudino et al. (2017) reported, in a meta-analysis focused on CMJ monitoring, that most studies used the highest rather than the average jump height (JH) when quantifying change (Claudino et al., 2017), indicating a prevailing emphasis on capturing maximal performance rather than central tendency.

Best-trial scoring leaves an underexplored issue: how many trials are needed to capture peak CMJ performance, whether this is metric-dependent, and how trial number and summary method affect reliability and sensitivity. This gap is supported by methodological evidence indicating that reliability may depend on both the number of trials collected and whether the best value across repeated attempts is retained (Bates et al., 1983; J. J. McMahon et al., 2018). Selecting the best value from a few trials implicitly assumes that the true session maximum occurs within this limited window. Practitioners may, however, accept that a real peak is not always captured and prioritise consistent, easily repeatable protocols for longitudinal monitoring. However, reduced or unstructured protocols may systematically underestimate current capability when peak-performance occurs beyond the first three trials. Such underestimation is not merely theoretical, as objective monitoring outputs are used to support training-load decisions in high-performance environments (Bourdon et al., 2017; Halson, 2014), and to inform clinically relevant judgments in rehabilitation settings, where vertical jump outcomes are used to evaluate performance and symmetry at return-to-sport (Kotsifaki et al., 2023). Consequently, protocol designs that fail to capture accurate session-best performance may introduce avoidable bias into performance profiling and readiness assessments.

Therefore, this study aimed to map where a range of force-plate-derived CMJ variables were maximised during a six-repetition protocol organised as two clusters of three maximal-effort attempts, using the testing day corresponding to each athlete's all-time personal best (PB) modified reactive strength index (mRSI), and to test whether this peak-timing pattern was robust to the choice of testing day by replicating the analysis on each athlete's typical state (TS) session. We hypothesised that a meaningful proportion of athletes would achieve their session-best performance outside the earliest repetitions commonly captured by reduced CMJ protocols and that peak-trial distributions would be comparable between PB and TS sessions, indicating that any delayed peak reflects the clustered protocol rather than PB day selection. The intended contribution was to inform evidence-based discussion around CMJ protocol structure for peak-capture contexts, while recognising that the optimal protocol may differ when the primary objective is time-efficient, mean-based longitudinal monitoring.

## Methods

### *Participants*

The final analytical sample included 314 athletes: 239 men (age: 23.8 ± 6.8 years, height: 181.7 ± 9.8 cm, body mass: 81.50 ± 16.0 kg) and 75 women (age: 22.9 ± 6.4 years, height: 167.1 ± 8.4 cm, body mass: 65.98 ± 10.89 kg). Participants were active amateur, semi-professional, and professional athletes who were engaged in regular training and structured strength and conditioning at the time of testing. Data were collected between August 2020 and December 2025. Eligibility for testing required the absence of movement-related musculoskeletal pain, operationalised as a score of 0 on a visual analogue scale (VAS; 0 = no pain, 10 = worst imaginable pain). All assessments formed part of routine performance monitoring and were conducted at a single high-performance training centre under the supervision of certified strength and conditioning coaches. Assessors involved in testing completed the same internal training in force-plate data acquisition and interpretation.

Given that the dataset was derived from applied monitoring rather than a prospective experimental protocol, several contextual variables were not collected

consistently across the full retrospective database. Detailed information on sport discipline, competitive level, season phase, previous musculoskeletal injury, surgical history, and long-term functional restrictions was therefore unavailable for reliable analyses of specific subgroups. These factors should be considered when interpreting the findings. Similarly, several variables that may influence acute neuromuscular performance were not standardised or systematically recorded. These included athlete wellness, psychological readiness, recent training load, accumulated fatigue, nutritional intake, caffeine use, supplementation, hydration status, and, in female athletes, menstrual cycle phase.

### Sample size estimation and justification

No a priori sample-size calculation was performed because this study analysed retrospectively collected routine monitoring data. Instead, all available eligible records meeting the predefined protocol-adherence and data-quality criteria were included in the analytical sample.

### Testing protocol

CMJ vertical ground reaction forces were collected at 1000 Hz using a wireless dual-force-plate system (Hawkin Dynamics, Westbrook, Maine, USA) and streamed in real time to the web-based Hawkin Dynamics Cloud via the Android Hawkin Capture application (Hawkin Dynamics, Westbrook, Maine, USA). Over the data collection period (2020–2025), software versions were updated as part of routine releases, and the mobile device used for acquisition was periodically updated. The system has demonstrated excellent criterion validity and reliability for CMJ assessment relative to laboratory-grade force-platforms (Badby et al., 2023; Dos'Santos et al., 2024). Testing was consistently scheduled as the first component of the training session, immediately following a standardised warm-up protocol. The warm-up consisted of 3 progressive blocks: (1) general aerobic (cycling/running) of ~5–8 min, (2) dynamic bodyweight exercises (e.g., glute bridge, lunges, calf raises), and (3) plyometric actions (rebound jumps and 3 submaximal CMJs for familiarisation), followed by 90–120 s rest before testing (Supplementary Material Table S1). Testing was conducted in usual training footwear using the same force plates and floor surface. Athletes adopted an upright, shoulder-width stance with hands-on-hips and, after a brief quiet-stance period, executed a self-selected countermovement and a maximal vertical jump in response to an audio-visual cue delivered via a tablet. The protocol comprised two sets of three maximal CMJs (six total trials), with 5–10 s of rest between repetitions and 60–90 s of rest between sets; the six repetitions were intentionally organised into two smaller clustered series to mitigate fatigue and help preserve maximal force- and power-related performance, consistent with recommendations from the cluster-set literature (Tufano et al., 2017) (Figure 1). Standardised instructions emphasised maximal performance using the cue: '*Jump as high and as fast as possible*.' Immediate feedback was provided after each trial in the form of JH only, and force-plate system was re-zeroed regularly.

Data were exported from the Hawkin Dynamics Cloud database using a dedicated software development toolkit (HDFORCE, v1.1.3; Hawkin Dynamics, Westbrook, Maine, USA). The raw dataset contained 998 athletes and 35,451 trials. To restrict the dataset to routine monitoring assessments conducted under the standardised protocol, only untagged records were retained; the absence of tags was used to indicate that a trial belonged to the regular monitoring workflow rather than ad hoc or non-standardised testing. In the centre's database, tags were applied to sessions that deviated from the standard CMJ monitoring procedure (e.g., modified instructions, non-

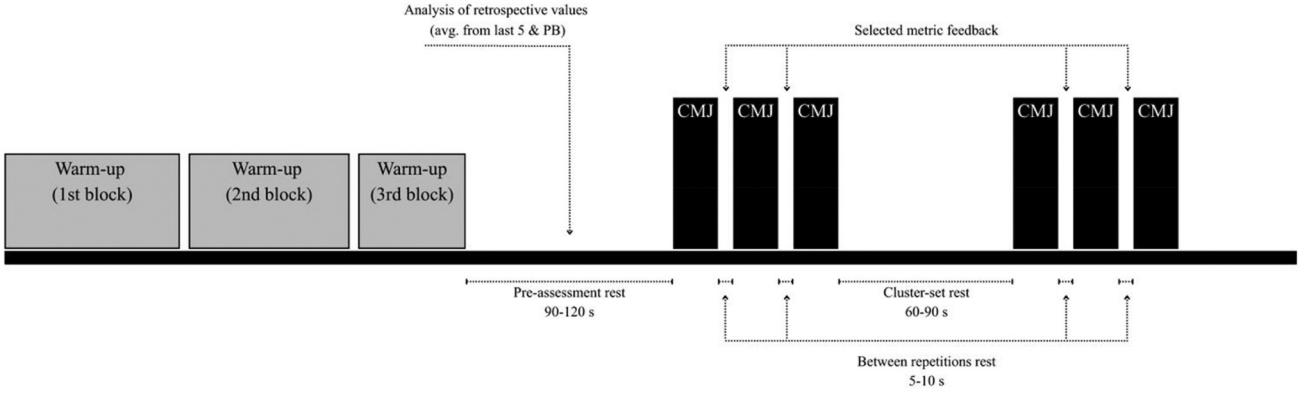



Figure 1. Visualisation of the research protocol.

standard test purpose, intra-training feedback). This filtering step yielded 22,096 trials from 996 athletes. To minimise learning and familiarisation effects associated with initial exposure to the testing procedure, each athlete's first recorded testing day was removed from further analysis and treated as a familiarisation session, provided that at least three CMJ repetitions were recorded on that day. After this step, the dataset comprised 21,716 trials from 937 athletes. Athletes who had no remaining testing sessions following removal of the initial day were excluded. At the session level, only testing days in which the CMJ protocol included at least six repetitions were retained. In addition, to verify adherence to the intended two-set structure (3 + 3), a minimum inter-set break of 50 s was required after the first three repetitions; specifically, the elapsed time between the third and fourth trial (derived from recorded time stamps) had to be ≥ 50 s. Testing days that did not satisfy either criterion were removed. When more than six repetitions were recorded on a given testing day, only the first six trials were retained to ensure a consistent repetition structure; trials were ordered chronologically using the recorded date and time stamps. This filtering procedure retained 10,386 trials from 314 athletes, corresponding to 1,731 eligible testing sessions, each comprising six trials (3 + 3). The final analytical sample therefore contained 314 sessions and 1,884 trials (314 athletes × 6 trials). Before PB day selection, athletes contributed a median of 3 eligible sessions (IQR: 1–6; range: 1–52). Subsequently, an athlete-specific all-time PB was defined as the single trial with the highest mRSI value across all retained sessions. For each athlete, only the testing day on which this PB occurred was carried forward for analysis, and all six repetitions from that day were retained to enable mapping of peak-trial occurrence across the repetition sequence. This PB selection was intentional for the primary analysis: the aim was not to describe typical monitoring sessions, which were examined separately using an auxiliary TS mRSI approach, but to examine peak-trial timing under conditions in which each athlete demonstrably expressed their highest recorded time-constrained CMJ performance. PB day selection was based solely on each athlete's highest single-trial mRSI; therefore, the analysis is partly circular for mRSI, which served as both the selection criterion and an outcome. Although the remaining six metrics were not used for selection, they were still analysed within an mRSI-defined performance state and may therefore be subject to selection-related interpretive bias.

Before statistical analyses, the exported dataset was screened for extreme values. Univariate outliers were defined a priori as observations lying outside the mean ±4 SD (i.e., absolute z-score > 4) for each metric, and these observations were excluded from subsequent analyses. This cut-off represents a conservative criterion for extreme observations: values several standard deviations away from the centre are expected to be rare even under broad distributional assumptions (Pukelsheim, 1994), and ≥4 SD thresholds have been used in large datasets as an error-risk filter to remove extreme values (Komatsu et al., 2024). The outlier rule was specified before inferential testing to minimise analytical flexibility in outlier management and to avoid iterative re-trimming (Leys et al., 2019). Given that a small number of extreme scores can disproportionately influence descriptive statistics and associations, excluding only the most extreme observations helps reduce undue leverage on results (Osborne & Overbay, 2004). PB and TS selection approaches were performed after adherence filtering and before metric-specific outlier screening. For the peak-trial mapping analyses, however, metric-specific peak-trial assignment was performed after outlier screening. This metric-specific procedure produced minimal data loss (<0.13% of observations). Therefore, if an observation was removed as an extreme value for a given metric, it was treated as unavailable for that metric only, and the peak-repetition was identified from the remaining valid repetitions for that athlete and metric.

To assess whether peak-trial timing patterns were specific to PB selection, a secondary supplementary TS analysis was performed. The TS analysis re-tested all seven metrics to confirm that the peak-trial pattern was not an artefact of PB day choice. For each athlete, session-average mRSI was calculated across all eligible 3 + 3 testing sessions, and the athlete-specific median of these session averages was identified. The retained typical session was defined as the testing day whose session-average mRSI was closest to this athlete-specific median. The same metric-specific outlier screening, peak-trial assignment, repetition-count summaries, cumulative capture probabilities, and goodness-of-fit tests were then repeated for this typical-session dataset.

### Statistical analysis

The analysis was primarily descriptive and exploratory, focusing on where session-best values occurred within the six-repetition protocol. In the absence of prior evidence indicating whether peak-trial occurrence should be uniform or repetition-specific, a uniform distribution was used only as a neutral reference within a limited confirmatory component. JH and mRSI were treated as primary output variables, representing the conventional CMJ performance outcome and the time-constrained performance index used for PB and TS selection, respectively.

For each metric, within-athlete z-scores were computed across all retained repetitions after protocol filtering, using $z = (x-\mu)/\sigma$, where $\mu$ and $\sigma$ denote the athlete's mean and standard deviation for that metric within the retained session. Results were reported as repetition-specific median z-scores with 95% confidence intervals. Confidence intervals around medians were estimated using non-parametric bootstrap resampling with 5,000 iterations. Distributions were visualised using raincloud/violin plots with overlaid points and boxplots. No zero-variance cases were observed for any analysed metric at the athlete level after protocol filtering.

For each metric, the peak-trial was identified for each athlete as the repetition with the largest absolute metric value within the selected session; the distribution of peak-trials across repetitions was summarised as n (%) values. Cumulative capture probability was calculated as the proportion of athletes whose peak-trial occurred within repetitions 1–N (N = 1–6).

To formally test whether peak-trial occurrence was evenly distributed across the six repetitions, chi-square goodness-of-fit tests ($df = 5$; expected proportion = 1/6 per repetition) were performed for each metric, with likelihood-ratio G-tests reported as a sensitivity check. Effect size was quantified using Cramér's V, and p-values were Bonferroni-adjusted across the seven metrics to control family-wise error (Armstrong, 2014; Kim, 2017). All data-processing and filtering procedures were implemented in Python using custom-written code utilising the following libraries: Matplotlib 3.10.8 - generated Figures 2 and 3; Polars 1.36.1 - data processing engine. Chosen due to efficiency and clear API.; NumPy 2.3.5 - low level computations where Polars was not enough.; Jupyter 1.1.1 - interactive execution of data processing code.; SciPy 1.16.3 - statistical testing toolkit; used for goodness-of-fit tests (Chi-square/G-test via power_divergence) and exact binomial p-values (binomtest) to quantify modal-vs-other comparisons reliably.

Sex-specific inferential analyses were not performed because the retrospective dataset was not designed to isolate sex effects and was characterised by an unequal sex distribution.

### Metrics selection

JH was calculated from take-off velocity, derived from the impulse–momentum relationship, using the equations of uniform motion. It was retained as the principal performance outcome because it remains the most frequently reported CMJ variable and provides a simple, intuitive descriptor of task success, defined as the peak positive displacement of the centre-of-mass (COM). At the same time, exclusive reliance on JH provides limited insight into how the result is achieved, and may therefore obscure meaningful trial-to-trial changes in movement strategy across repeated efforts. For this reason, additional metrics were selected to capture temporal constraints, force-displacement behaviour, and phase-specific mechanical outputs alongside the global outcome.

The mRSI was included as an outcome-oriented metric that quantifies performance within a time constraint. In CMJ contexts, mRSI is commonly operationalised as JH divided by time to take-off (TTTO), where TTTO reflects the total generation time across the summed durations of the unweighting, braking, and propulsive phases. This index was selected to represent the capacity to attain a given JH rapidly, aligning more closely with the time-pressured demands characteristic of many sport-specific actions than height alone; accordingly, the research team prioritised mRSI as the primary decision metric and filtered the dataset based on performance as defined by mRSI.

An index of stiffness was calculated as the vertical ground reaction force at the instant of peak negative COM displacement divided by the peak negative COM displacement (i.e., countermovement depth), representing a force–displacement ratio at the transfer point. Therefore, this index should not be interpreted as a direct measure of musculotendinous stiffness, but rather as a task-specific mechanical ratio. This approach enables interpretation of whether comparable outputs are achieved via comparatively larger forces over smaller displacements (a stiffer strategy) or smaller forces over larger displacements (a more compliant strategy), which is particularly relevant when performance is evaluated across repeated trials.

To provide mechanistically grounded descriptors of braking and propulsive function, average relative braking force (ARBF) and average relative propulsive force (ARPF) were selected as phase-averaged, body mass-normalised kinetic variables. Averaging across the complete braking and propulsive phases was prioritised to characterise phase-wide mechanical demand rather than isolated force extrema. System mass was derived from system weight measured during the quiet weighing phase before movement onset. Based on Newton's second law and the impulse–momentum relationship, phase-average net force normalised to system mass is equivalent to the mean COM acceleration required to produce the observed change in velocity over the corresponding phase. Accordingly, ARBF was derived from the change in COM velocity from peak negative velocity to zero velocity across the braking phase, whereas ARPF was derived

from the change in COM velocity from propulsive onset to take-off across the propulsive phase. Because COM velocity was obtained through numerical integration of the net force–time signal, this velocity–duration approach represents an algebraically equivalent expression of phase-average net force rather than an independent kinematic estimate. The resulting mass-normalised net-force equivalents were scaled relative to gravitational acceleration and increased by 1.0 to reinstate the system-weight component. Final ARBF and ARPF values were therefore expressed as multiples of system weight, consistent with the default Hawkin Dynamics reporting convention. Complementing these force-based descriptors, average relative braking power (ARBP) and average relative propulsive power (ARPP) were included as body mass-normalised phase-average power variables. These metrics were defined as the phase average of the instantaneous, sample-by-sample product of vertical ground-reaction force and COM velocity and were expressed in W/kg. Because COM velocity is negative during braking and positive during propulsion, ARBP quantified mean negative mechanical power during the braking phase, whereas ARPP quantified mean positive mechanical power during the propulsive phase.

## Results

The results of this study showed that the timing of peak values varied considerably across dependent variables within the standardised six-repetition clustered CMJ protocol.

Across the six repetitions, JH showed small, unsystematic changes in central tendency. The highest median standardised score was recorded in repetition 2 (z median = 0.740, 95% CI 0.587–0.823), with a comparably high median in repetition 4 (z median = 0.714, 95% CI 0.562–0.865). In the final two repetitions, the medians were noticeably lower: repetition 5 (z median = 0.292, 95% CI 0.166–0.504); repetition 6 (z median = 0.332, 95% CI 0.202–0.463) (Table 1 and Figure 2). In raw terms, median JH differed only marginally across the protocol (peak-to-lowest median difference ≈0.004 m), indicating that output-based interpretation alone would suggest broadly stable performance. At the individual level, the session maximum for JH most frequently occurred in repetition 4 (n = 81, 25.8%) and repetition 2 (n = 69, 22.0%). Overall, 54.1% of participants achieved their JH maximum during the first cluster, while 45.9% achieved it during the second cluster (Table 2).

**Table 1.** Repetition-specific within-athlete standardised outcomes across PB and TS session selections.

| Metric / session type | Repetition 1 | Repetition 2 | Repetition 3 | Repetition 4 | Repetition 5 | Repetition 6 |
|---|---|---|---|---|---|---|
| JH (PB) | 0.618 [0.417, 0.785] | **0.740 [0.587, 0.823]** | 0.617 [0.464, 0.707] | 0.714 [0.562, 0.865] | 0.292 [0.166, 0.504] | 0.332 [0.202, 0.463] |
| *JH (TS)* | *0.094 [−0.052, 0.204]* | *0.172 [0.094, 0.344]* | *0.211 [0.079, 0.347]* | ***0.298 [0.184, 0.413]*** | *0.000 [−0.141, 0.136]* | *-0.099 [−0.228, 0.040]* |
| mRSI (PB) | 0.730 [0.600, 0.885] | 0.686 [0.555, 0.811] | 0.494 [0.320, 0.594] | **0.905 [0.723, 1.072]** | 0.531 [0.361, 0.652] | 0.407 [0.232, 0.567] |
| *mRSI (TS)* | *0.098 [−0.012, 0.230]* | *0.066 [−0.026, 0.224]* | *-0.040 [−0.130, 0.051]* | ***0.340 [0.204, 0.435]*** | *-0.034 [−0.151, 0.036]* | *-0.142 [−0.260, −0.028]* |
| Stiffness (PB) | 0.509 [0.396, 0.698] | 0.228 [0.066, 0.388] | 0.126 [−0.054, 0.227] | **0.546 [0.429, 0.707]** | 0.145 [0.023, 0.327] | 0.118 [−0.011, 0.226] |
| *Stiffness (TS)* | ***0.131 [−0.064, 0.326]*** | *-0.148 [−0.236, −0.066]* | *-0.194 [−0.343, −0.064]* | *0.080 [−0.047, 0.249]* | *-0.268 [−0.358, −0.144]* | *-0.247 [−0.401, −0.124]* |
| ARBF (PB) | −0.252 [−0.354, −0.050] | 0.347 [0.257, 0.485] | 0.400 [0.297, 0.521] | 0.415 [0.312, 0.573] | 0.577 [0.459, 0.751] | **0.762 [0.650, 0.912]** |
| *ARBF (TS)* | *-0.594 [−0.754, −0.433]* | *-0.110 [−0.220, 0.028]* | *-0.027 [−0.123, 0.112]* | *0.081 [−0.044, 0.226]* | *0.281 [0.111, 0.385]* | ***0.359 [0.198, 0.493]*** |
| ARBP (PB) | −0.591 [−0.735, −0.438] | 0.267 [0.122, 0.417] | 0.408 [0.264, 0.514] | 0.259 [0.114, 0.379] | 0.633 [0.519, 0.763] | **0.746 [0.593, 0.919]** |
| *ARBP (TS)* | *-0.814 [−0.934, −0.595]* | *-0.124 [−0.227, −0.003]* | *0.131 [−0.017, 0.207]* | *-0.004 [−0.135, 0.122]* | *0.387 [0.305, 0.506]* | ***0.509 [0.392, 0.638]*** |
| ARPF (PB) | **0.983 [0.819, 1.142]** | 0.508 [0.386, 0.698] | 0.346 [0.202, 0.422] | 0.889 [0.767, 1.006] | 0.250 [0.021, 0.513] | 0.170 [0.038, 0.244] |
| *ARPF (TS)* | ***0.383 [0.228, 0.563]*** | *-0.022 [−0.126, 0.087]* | *-0.131 [−0.257, 0.021]* | *0.290 [0.157, 0.391]* | *-0.248 [−0.368, −0.148]* | *-0.358 [−0.484, −0.281]* |
| ARPP (PB) | 0.743 [0.601, 0.931] | 0.705 [0.596, 0.845] | 0.509 [0.433, 0.617] | **0.974 [0.866, 1.091]** | 0.464 [0.285, 0.646] | 0.383 [0.268, 0.583] |
| *ARPP (TS)* | *0.114 [0.018, 0.259]* | *0.077 [−0.068, 0.204]* | *0.042 [−0.089, 0.140]* | ***0.351 [0.227, 0.487]*** | *-0.066 [−0.168, 0.027]* | *-0.181 [−0.276, −0.059]* |

Legend: Repetition-specific standardised outcomes are presented as median z-score [95% CI] across a six-trial, clustered countermovement jump (CMJ) protocol. Rows are shown separately for the personal best (PB) and typical state (TS) selection paths. PB indicates the testingsession in which the athlete achieved their highest retained single-trial mRSI value. TS indicates the eligible testing session whose average session mRSI was closest to that athlete's median eligible-session average mRSI (this auxiliary interpretation is shown in italics in the table). Positive and negative z-scores indicate higher and lower within-athlete standardised metric magnitudes, respectively. CI = confidence interval. Repetitions 1–6 correspond to consecutive jumps performed within the same testing session. The highest values within each metric and selection path are marked in bold. JH = jump height [m]; mRSI = modified reactive strength index [a.u.]; Stiffness = vertical stiffness [N/m]; ARBF = average relative braking force [N/kg]; ARBP = average relative braking power [W/kg]; ARPF = average relative propulsive force [N/kg]; ARPP = average relative propulsive power [W/kg].

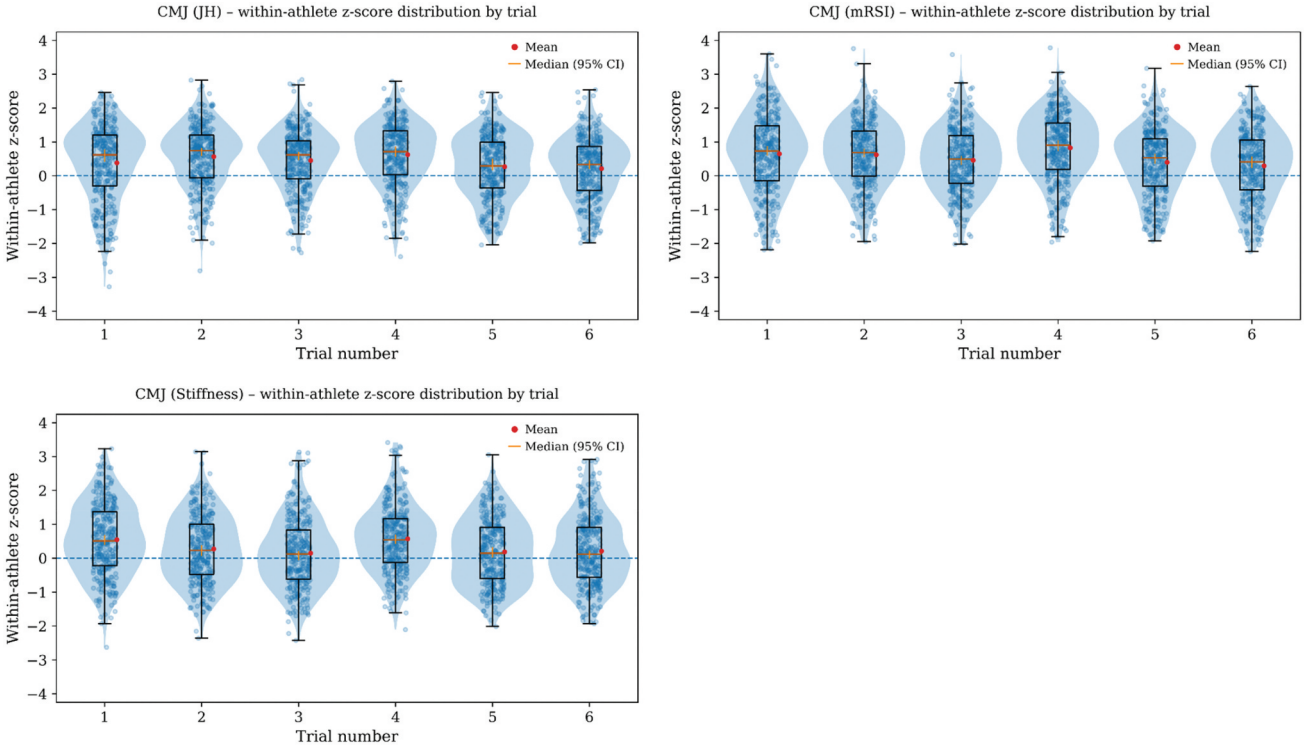



**Figure 2.** Within-athlete z-score distributions for selected CMJ metrics across a six-trial clustered protocol. Legend: JH = jump height [m]; mRSI = modified reactive strength index [a.u.]; Stiffness = vertical stiffness [N/m]. Raincloud plots combine kernel density (violin), individual observations (points), and an embedded boxplot (median and interquartile range). The red dot indicates the mean, and the orange marker indicates the median with its 95% confidence interval. The dashed horizontal line at z = 0 denotes each athlete's session mean for the given metric. The boxplot whiskers extend to 1.5×IQR, and observations outside the whiskers are shown as points.

**Table 2.** Comparison of peak-trial occurrence between PB and TS sessions across the six-trial clustered CMJ protocol for jump height, modified reactive strength index, vertical stiffness, and braking and propulsive kinetic metrics.

| Metric / session type | Repetition 1 | Repetition 2 | Repetition 3 | Repetition 4 | Repetition 5 | Repetition 6 |
|---|---|---|---|---|---|---|
| JH (PB) | 61 (19.4)% | 69 (22.0)% | 40 (12.7)% | **81 (25.8)%** | 36 (11.5)% | 27 (8.6)% |
| *JH (TS)* | *52 (16.6)%* | *65 (20.7)%* | *46 (14.6)%* | *82 (26.1)%* | *35 (11.1)%* | *34 (10.8)%* |
| mRSI (PB) | 63 (20.1)% | 48 (15.3)% | 41 (13.1)% | **95 (30.3)%** | 33 (10.5)% | 34 (10.8)% |
| *mRSI (TS)* | *56 (17.8)%* | *47 (15.0)%* | *46 (14.6)%* | *95 (30.3)%* | *38 (12.1)%* | *32 (10.2)%* |
| Stiffness (PB) | **86 (27.4)%** | 44 (14.0)% | 39 (12.4)% | 69 (22.0)% | 40 (12.7)% | 36 (11.5)% |
| *Stiffness (TS)* | *78 (24.8)%* | *46 (14.6)%* | *41 (13.1)%* | *64 (20.4)%* | *41 (13.1)%* | *44 (14.0)%* |
| ARBF (PB) | 26 (8.3)% | 43 (13.7)% | 46 (14.6)% | 47 (15.0)% | 54 (17.2)% | **98 (31.2)%** |
| *ARBF (TS)* | *23 (7.3)%* | *49 (15.6)%* | *50 (15.9)%* | *38 (12.1)%* | *71 (22.6)%* | *83 (26.4)%* |
| ARBP (PB) | 16 (5.1)% | 39 (12.4)% | 49 (15.6)% | 32 (10.2)% | 71 (22.6)% | **107 (34.1)%** |
| *ARBP (TS)* | *17 (5.4)%* | *39 (12.4)%* | *46 (14.6)%* | *30 (9.6)%* | *73 (23.2)%* | *109 (34.7)%* |
| ARPF (PB) | **100 (31.8)%** | 48 (15.3)% | 29 (9.2)% | 81 (25.8)% | 28 (8.9)% | 28 (8.9)% |
| *ARPF (TS)* | *98 (31.2)%* | *48 (15.3)%* | *32 (10.2)%* | *80 (25.5)%* | *27 (8.6)%* | *29 (9.2)%* |
| ARPP (PB) | 68 (21.7)% | 51 (16.2)% | 34 (10.8)% | **92 (29.3)%** | 31 (9.9)% | 38 (12.1)% |
| *ARPP (TS)* | *69 (22.0)%* | *46 (14.6)%* | *40 (12.7)%* | *90 (28.7)%* | *34 (10.8)%* | *35 (11.1)%* |

Legend: Values are n (%), indicating the number and proportion of athletes for whom the highest within-session value for a given metric occurred in each repetition. Personal best (PB) refers to the testing day on which the athlete achieved their all-time highest mRSI trial. Typical state (TS) refers to the eligible testing day whose session-average mRSI was closest to that athlete's median session-average mRSI (this auxiliary interpretation is shown in italics in the table). Bold values indicate the most frequent peak-repetition with in each metric and session type. JH = jump height [m]; mRSI = modified reactive strength index [a.u.]; Stiffness = vertical stiffness [N/m]; ARBF = average relative braking force [N/kg]; ARBP = average relative braking power [W/kg]; ARPF = average relative propulsive force [N/kg]; ARPP = average relative propulsive power [W/kg].

A different pattern emerged when performance was evaluated using mRSI, which embeds the jump outcome within a time constraint. The highest median mRSI occurred at repetition 4 (z median = 0.905, 95% CI 0.723–1.072), representing the first effort of the second cluster. This value exceeded the medians observed in repetitions 1–3; repetition 3 (z median = 0.494, 95% CI 0.320–0.594) and remained higher than those observed in repetitions 5–6; repetition 6 (z median = 0.407, 95% CI 0.232–0.567). The best mRSI

was most often observed in the second half of the protocol, despite JH showing relatively small changes from repetition to repetition. All results are presented in Table 1 and Figure 2. Consistent with this, the peak-value repetition associated with each athlete's maximal mRSI was observed as repetition 4: n = 95 (30.3%) and repetition 1: n = 63 (20.1%), with 51.6% of maximal mRSI values occurring in repetitions 4–6 and 48.4% in the first cluster repetitions 1–3 (Table 2).

Stiffness also peaked in the second cluster. The highest median was observed in repetition 4 (z median = 0.546, 95% CI 0.429–0.707), and the result in repetition 1 was very similar (z median = 0.509, 95% CI 0.396–0.698). In the remaining repetitions, both 2–3 and 5–6, the medians were much lower. These data are presented in Table 1 and Figure 2. Most maximised stiffness during either the first (n = 86, 27.4%) or fourth (n = 69, 22.0%) repetition. A total of 46.2% of the maximum stiffness values occurred in the second cluster, and 53.8% occurred in the first cluster.

The strongest and most consistent late-peak pattern was observed for braking phase metrics. ARBF increased progressively across repetitions, shifting from a below-baseline median at repetition 1 (z median = −0.252, 95% CI − 0.354 to −0.050) to the highest median at repetition 6 (z median = 0.762, 95% CI 0.650–0.912). A similar late-shift pattern was evident for ARBP, with the lowest median at repetition 1 (z median = −0.591, 95% CI − 0.735 to −0.438) and the highest median at repetition 6 (z median = 0.746, 95% CI 0.593–0.919). These findings identify a clear special case in which the most favourable values, as defined by braking performance, occurred in the second cluster, specifically in the final repetition of the protocol (Table 1 and Figure 3). Aligning with this, maximal ARBF was most commonly achieved at repetition 6 (n = 98, 31.2%). Thus, 63.4% of ARBF maxima occurred in repetitions 4–6 and 36.6% within repetitions 1–3. Similarly, maximal ARBP was most frequently achieved at repetitions 6 (n = 107, 34.1%). Thus, 66.9% of ARBP maxima occurred in the second cluster and 33.1% in the first cluster (Table 2).

For propulsive phase variables, peak-performance was split between early and post-rest repetitions. ARPF reached its highest median at repetition 1 (z median = 0.983, 95% CI 0.819–1.142), but it should be noted that it also showed a marked increase in repetition 4 (z median = 0.889, 95% CI 0.767–1.006), exceeding repetition 3 by Δz median = 0.543. Similarly, ARPP peaked at repetition 4 (z median = 0.974, 95% CI 0.866–1.091), again highlighting the first jump of the second cluster as a key point at which optimal concentric power expression was most likely to be captured (Table 1 and Figure 3). When analysing individual maxima, peak ARPF occurred most often at repetition 1 (n = 100, 31.8%) and repetition 4 (n = 81, 25.8%). Across all trials, 56.4% maximised ARPF in the first cluster, while 43.6% maximised ARPF during the second cluster. For ARPP, maximal values were most frequently observed at repetition 4 (n = 92, 29.3%) and repetition 1 (n = 68, 21.7%). Across the sample, 51.3% of peaks occurred 51.3% of peaks occur in repetitions 4–6 of the second cluster, and 48.7% within repetitions 1–3 (Table 2).

Cumulative capture probabilities for reduced protocols (i.e., using the first N repetitions) are summarised in Table 3. As expected, capture reached 100% once all six repetitions were included, while the accumulation rate differed across metrics.

Peak-trial timing deviated significantly from a uniform distribution across the six repetitions for every metric, in both the PB ($\chi^2$ (5) = 39.69–100.50) and TS ($\chi^2$ (5) = 22.19–107.07) selections (all Bonferroni-adjusted $p \leq 3.4 \times 10^{-3}$; Cramér's V = 0.12–0.26; Table 4). Peak occurrences were therefore not evenly distributed across the standardised six-repetition clustered CMJ protocol, with small-to-moderate effect sizes (Table 4).

To examine whether the observed peak-trial patterns were specific to PB selection, a secondary analysis was performed using each athlete's TS (defined as the eligible testing day whose session-average mRSI was closest to that athlete's median session-average mRSI). The selected TS session matched the PB testing unit in 135 of 314 athletes (43.0%), indicating that the two analyses frequently sampled different testing days. In the subset of athletes whose TS and PB days did not coincide (n = 179), the modal peak-repetition was unchanged for five of the seven selected metrics. Despite this, the distribution of peak-trial occurrence was highly similar between the two session-selection approaches (Table 5). The modal peak-repetition remained unchanged for mRSI and ARPP at repetition 4, for ARBF and ARBP at repetition 6, and for ARPF at repetition 1. Formal comparison of the peak-trial distributions between the PB and TS sessions showed no significant difference for any metric ($\chi^2$ (5) range = 0.21–5.25; all p_raw ≥ 0.386 and all Bonferroni-adjusted p = 1.000; Cramér's V = 0.018–0.091; minimum expected cell count = 16.5; Table 5), confirming that the two analyses produced statistically comparable peak-timing profiles.

## Discussion

### *Interpretation of findings and mechanisms underlying a delayed performance peak*

The results of this study indicate that peak CMJ performance most often emerged after several repetitions, typically in the second half of the protocol following

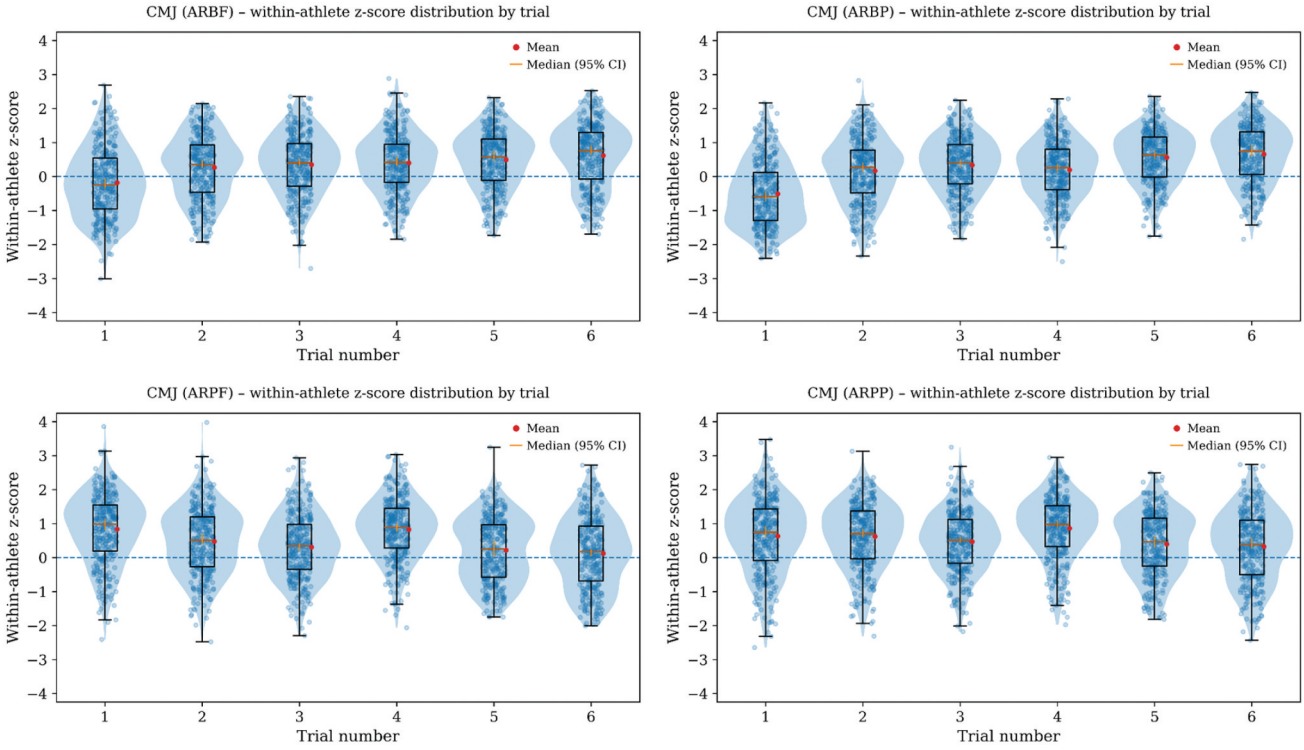



**Figure 3.** Within-athlete z-score distributions for phase-averaged CMJ metrics in a six-trial, clustered protocol, exemplified by ARBF, ARBP, ARPF, and ARPP. Legend: ARBF = average relative braking force [N/kg], ARBP = average relative braking power [W/kg], ARPF = average relative propulsive force [N/kg], ARPP = average relative propulsive power [W/kg]. Raincloud plots combine density estimation (violin), individual observations (points), and a built-in boxplot (median and interquartile range). The red dot indicates the mean, and the orange marker indicates the median with a 95% confidence interval. The dashed horizontal line at z = 0 indicates the average session value for the given athlete for the analysed metric. The boxplot whiskers extend to 1.5×IQR, and observations outside the whiskers are shown as points.

**Table 3.** Cumulative capture probability of the session maximum across the six-trial clustered CMJ protocol for PB and TS session selections.

| Metric / session type | Repetition 1 | Repetitions 1–2 | Repetitions 1–3 | Repetitions 1–4 | Repetitions 1–5 | Repetitions 1–6 |
|---|---|---|---|---|---|---|
| JH (PB) | 19.40% | 41.40% | 54.10% | 79.90% | 91.40% | 100.00% |
| *JH (TS)* | *16.60%* | *37.30%* | *51.90%* | *78.00%* | *89.20%* | *100.00%* |
| mRSI (PB) | 20.10% | 35.40% | 48.40% | 78.70% | 89.20% | 100.00% |
| *mRSI (TS)* | *17.80%* | *32.80%* | *47.50%* | *77.70%* | *89.80%* | *100.00%* |
| Stiffness (PB) | 27.40% | 41.40% | 53.80% | 75.80% | 88.50% | 100.00% |
| *Stiffness (TS)* | *24.80%* | *39.50%* | *52.50%* | *72.90%* | *86.00%* | *100.00%* |
| ARBF (PB) | 8.30% | 22.00% | 36.60% | 51.60% | 68.80% | 100.00% |
| *ARBF (TS)* | *7.30%* | *22.90%* | *38.90%* | *51.00%* | *73.60%* | *100.00%* |
| ARBP (PB) | 5.10% | 17.50% | 33.10% | 43.30% | 65.90% | 100.00% |
| *ARBP (TS)* | *5.40%* | *17.80%* | *32.50%* | *42.00%* | *65.30%* | *100.00%* |
| ARPF (PB) | 31.80% | 47.10% | 56.40% | 82.20% | 91.10% | 100.00% |
| *ARPF (TS)* | *31.20%* | *46.50%* | *56.70%* | *82.20%* | *90.80%* | *100.00%* |
| ARPP (PB) | 21.70% | 37.90% | 48.70% | 78.00% | 87.90% | 100.00% |
| *ARPP (TS)* | *22.00%* | *36.60%* | *49.40%* | *78.00%* | *88.90%* | *100.00%* |

Legend: Values indicate the percentage of athletes whose within-session maximum for a given metric was achieved within the first N repetitions, shown as Repetition 1; Repetitions 1–2; Repetitions 1–3; Repetitions 1–4; Repetitions 1–5; and Repetitions 1–6. Rows are shown separately for the personal best (PB) and typical state (TS) selection paths. PB indicates the testing session in which the athlete achieved their highest retained single-trial mRSI value. TS indicates the eligible testing session whose average session mRSI was closest to that athlete's median eligible-session average mRSI (this auxiliary interpretation is shown in italics in the table). JH = jump height [m]; mRSI = modified reactive strength index [a.u.]; Stiffness = vertical stiffness [N/m]; ARBF = average relative braking force [N/kg]; ARBP = average relative braking power [W/kg]; ARPF = average relative propulsive force [N/kg]; ARPP = average relative propulsive power [W/kg].

a brief mid-test break. Across the analysed variables, peak-values were therefore more frequently achieved after several lead-in attempts than on the first jump. This pattern is unlikely to be a trivial measurement artefact; rather, there is both a theoretical and applied rationale for this phenomenon.

**Table 4.** Deviation of peak-trial occurrence from a uniform distribution across six CMJ repetitions for PB and TS session selections. ($\chi^2$ goodness-of-fit; effect size as Cramér's V).

| Metric / session type | N | Peak-value counts (1–6) | Chi-square | df | p (Chi-square) | p (Chi-square) Bonferroni | Cramer's V | G-test (LRT) | p (G-test) | p (G-test) Bonferroni |
|---|---|---|---|---|---|---|---|---|---|---|
| JH (PB) | 314 | 61, 69, 40, 81, 36, 27 | 42.71 | 5 | $4.23 \times 10^{-8}$ | $2.96 \times 10^{-7}$ | 0.17 | 43.44 | $3.01 \times 10^{-8}$ | $2.11 \times 10^{-7}$ |
| *JH (TS)* | *314* | *52, 65, 46, 82, 35, 34* | *32.82* | *5* | *$4.09 \times 10^{-6}$* | *$2.86 \times 10^{-5}$* | *0.15* | *31.81* | *$6.48 \times 10^{-6}$* | *$4.54 \times 10^{-5}$* |
| mRSI (PB) | 314 | 63, 48, 41, 95, 33, 34 | 53.34 | 5 | $2.86 \times 10^{-10}$ | $2.00 \times 10^{-9}$ | 0.18 | 48.59 | $2.69 \times 10^{-9}$ | $1.88 \times 10^{-8}$ |
| *mRSI (TS)* | *314* | *56, 47, 46, 95, 38, 32* | *48.18* | *5* | *$3.26 \times 10^{-9}$* | *$2.28 \times 10^{-8}$* | *0.18* | *43.09* | *$3.54 \times 10^{-8}$* | *$2.48 \times 10^{-7}$* |
| Stiffness (PB) | 314 | 86, 44, 39, 69, 40, 36 | 39.69 | 5 | $1.72 \times 10^{-7}$ | $1.21 \times 10^{-6}$ | 0.16 | 36.95 | $6.13 \times 10^{-7}$ | $4.29 \times 10^{-6}$ |
| *Stiffness (TS)* | *314* | *78, 46, 41, 64, 41, 44* | *22.19* | *5* | *$4.82 \times 10^{-4}$* | *$3.37 \times 10^{-3}$* | *0.12* | *20.86* | *$8.61 \times 10^{-4}$* | *$6.03 \times 10^{-3}$* |
| ARBF (PB) | 314 | 26, 43, 46, 47, 54, 98 | 56.13 | 5 | $7.64 \times 10^{-11}$ | $5.35 \times 10^{-10}$ | 0.19 | 51.10 | $8.25 \times 10^{-10}$ | $5.78 \times 10^{-9}$ |
| *ARBF (TS)* | *314* | *23, 49, 50, 38, 71, 83* | *45.31* | *5* | *$1.25 \times 10^{-8}$* | *$8.78 \times 10^{-8}$* | *0.17* | *46.72* | *$6.48 \times 10^{-9}$* | *$4.54 \times 10^{-8}$* |
| ARBP (PB) | 314 | 16, 39, 49, 32, 71, 107 | 100.50 | 5 | $4.15 \times 10^{-20}$ | $2.90 \times 10^{-19}$ | 0.25 | 97.58 | $1.71 \times 10^{-19}$ | $1.20 \times 10^{-18}$ |
| *ARBP (TS)* | *314* | *17, 39, 46, 30, 73, 109* | *107.07* | *5* | *$1.70 \times 10^{-21}$* | *$1.19 \times 10^{-20}$* | *0.26* | *102.12* | *$1.89 \times 10^{-20}$* | *$1.32 \times 10^{-19}$* |
| ARPF (PB) | 314 | 100, 48, 29, 81, 28, 28 | 92.51 | 5 | $1.99 \times 10^{-18}$ | $1.40 \times 10^{-17}$ | 0.24 | 87.69 | $2.05 \times 10^{-17}$ | $1.44 \times 10^{-16}$ |
| *ARPF (TS)* | *314* | *98, 48, 32, 80, 27, 29* | *85.40* | *5* | *$6.21 \times 10^{-17}$* | *$4.34 \times 10^{-16}$* | *0.23* | *81.10* | *$4.94 \times 10^{-16}$* | *$3.46 \times 10^{-15}$* |
| ARPP (PB) | 314 | 68, 51, 34, 92, 31, 38 | 53.83 | 5 | $2.27 \times 10^{-10}$ | $1.59 \times 10^{-9}$ | 0.19 | 50.67 | $1.01 \times 10^{-9}$ | $7.08 \times 10^{-9}$ |
| *ARPP (TS)* | *314* | *69, 46, 40, 90, 34, 35* | *48.25* | *5* | *$3.16 \times 10^{-9}$* | *$2.21 \times 10^{-8}$* | *0.18* | *44.89* | *$1.53 \times 10^{-8}$* | *$1.07 \times 10^{-7}$* |

Legend: Peak-value counts (Rep 1-6) indicate the number of athletes whose within-session maximum for a given metric occurred on repetitions 1–6 in the CMJ protocol (3 + 3). Rows are shown separately for the personal best (PB) and typical state (TS) selection paths. Chi-square goodness-of-fit tests and likelihood-ratio G-tests assessed deviation from a uniform distribution of peak occurrences across repetitions, using an expected proportion of 1/6 per repetition and df = 5. Bonferroni-adjusted p-values control the family-wise error rate across the seven metrics within each selection path. Cramer's V is reported as the effect size for the chi-square test. JH = jump height [m]; mRSI = modified reactive strength index [a.u.]; Stiffness = vertical stiffness [N/m]; ARBF = average relative braking force [N/kg]; ARBP = average relative braking power [W/kg]; ARPF = average relative propulsive force [N/kg]; ARPP = average relative propulsive power [W/kg].

**Table 5.** Homogeneity of the peak-trial distribution between PB and TS session selections ($\chi^2$ test of independence, $2 \times 6$ contingency tables, df = 5).

| Metric | $\chi^2$ (5) | p (raw) | p (Bonferroni) | Cramér's V | Min. expected cell |
|---|---|---|---|---|---|
| JH | 2.08 | 0.838 | 1.000 | 0.058 | 30.5 |
| mRSI | 1.12 | 0.952 | 1.000 | 0.042 | 33.0 |
| Stiffness | 1.49 | 0.915 | 1.000 | 0.049 | 40.0 |
| ARBF | 5.25 | 0.386 | 1.000 | 0.091 | 24.5 |
| ARBP | 0.24 | 0.999 | 1.000 | 0.019 | 16.5 |
| ARPF | 0.21 | 0.999 | 1.000 | 0.018 | 27.5 |
| ARPP | 1.04 | 0.960 | 1.000 | 0.041 | 32.5 |

Legend: $\chi^2$ tests of independence compared peak-trial occurrence (six repetitions) between personal best (PB) and typical state (TS) selections for each metric ($2 \times 6$ table, df = 5; N = 628; 314 athletes per path). Bonferroni-adjusted across seven metrics. Cramér's V = effect size. Abbreviations as in Table 4.

Furthermore, this distribution was not limited to PB testing days. The secondary TS analysis showed highly similar peak-trial distributions, with the modal repetition unchanged for most analysed metrics. This suggests that delayed peak expression is not simply an anomaly of selecting each athlete's PB, but may reflect a more general feature of the six-trial clustered CMJ protocol, and confirms the validity of further in-depth exploration of these observations.

From an ecological dynamics perspective, maximal CMJ performance represents an emergent solution shaped by interacting constraints among the performer, task, and environment, rather than by the execution of a fixed motor program (Wade & Whiting, 1986). Consequently, the first repetitions likely function as a short phase of exploration in which the athlete tunes into task-relevant information and stabilises a functional coordination pattern under the specific constraints of the testing context (e.g., force-plate system/contact

conditions, instructions, arm position, footwear), consistent with the notion that learning and performing are symbiotic and continuously co-adaptive processes in high-performance settings (Renshaw et al., 2022). Accordingly, early within-session variability should not be automatically dismissed as noise; it may reflect functional search processes that support the emergence of a more effective movement solution (Woods et al., 2020). Supporting evidence indicates that repeated exposures can promote: education of intention (aligning the athlete's goal with the task demands), education of attention (attunement to the specification of information), and calibration (scaling action to current action capabilities) (Davids et al., 2012). These processes, collectively described as perception–action tuning, provide a coherent theoretical rationale for why the best jump performance, as quantified by variables of interest, may be delayed until the athlete sufficiently tunes the perception – action system to the task (Seifert et al., 2022). This interpretation also aligns with representative learning design: the validity of an assessment depends on how well the task captures the informational constraints and action requirements of the target performance environment, and repeated trials may be necessary to reach adequate action fidelity when the testing context is unfamiliar or subtly different from habitual practice (Pinder et al., 2011).

A second, complementary explanation for the delay in achieving maximum performance involves acute performance-preparation effects. Despite a standardised warm-up preceding testing, successive maximal CMJ attempts within a session can further enhance performance through short-term potentiation-like mechanisms. Warm-up strategies that incorporate general and dynamic activity can meaningfully influence CMJ outcomes (Pagaduan et al., 2012), and the inclusion of explosive, movement-specific resistance exercise has been shown to acutely improve CMJ height and power (Sotiropoulos et al., 2010). Similarly, post-activation performance enhancement (PAPE) paradigms suggest that prior explosive actions (e.g., drop jumps or loaded squat jumps) can increase subsequent CMJ height, although the time course and magnitude of potentiation are heterogeneous and depend on strength level and protocol specifics (Yang et al., 2024). Importantly, recent randomised crossover evidence indicates that putative PAPE effects can be difficult to disentangle from well-executed warm-up effects, and additional potentiation protocols do not consistently outperform comprehensive warm-ups (Rappelt et al., 2024). Therefore, the presented pattern is best interpreted conservatively: early-trial improvements likely reflect a coupled process of physiological priming and sensorimotor calibration, rather than a single causal mechanism. This interpretation is further supported by evidence that high test–retest reproducibility of CMJ performance can be achieved even without extensive familiarisation sessions in physically active men (Moir et al., 2004), suggesting that the present phenomenon reflects short-timescale optimisation within a session rather than long-term learning the test. Therefore, to fully capture the maximum potential of CMJ, considering the best result from several trials as the most reliable indicator of performance seems justified, especially when the aim is to assess peak-capabilities rather than average organism behaviour (Wulf & Lewthwaite, 2016).

### *Metric-specific behaviour: braking phase versus propulsion*

A key finding of this study is that different aspects of CMJ performance are maximised at different repetitions depending on the variable used to quantify it. The most pronounced late-peak pattern was observed for braking phase variables, with ARBF and ARBP most frequently maximised during the final (sixth) repetition, with more than 60% of peaks occurring after the mid-test rest, within the second cluster. Because ARBF and ARBP most frequently peaked in repetition 6, the present design cannot determine whether an additional cluster would further increase peak-capture probability or instead introduce greater fatigue, variability, or measurement noise. This possibility should therefore be treated as a hypothesis for prospective protocol-comparison studies rather than as a direct implication of the present data. However, the authors emphasise that such an extension may have limited feasibility in high-performance sport and clinical contexts, where session time is tightly constrained; more protracted monitoring can also introduce organisational and communication burdens and may divert time away from higher-priority training or therapeutic activities. In contrast, outcome and propulsion-related indices showed somewhat different distributions. The mRSI, a variable that enables practitioners to consider the time taken to perform the CMJ, was most commonly maximised during repetition 4 (the first trial after the longer inter-set rest), a pattern also observed for ARPP. That said, JH was far more variable, with just over half of the participants maximising this during the first cluster, and just under half of participants maximising it during the second cluster. Collectively, these patterns indicate that some strategy-related force–time characteristics may continue improving even when the headline outcome (JH) appears broadly stable. This highlights an important applied point: relying solely on output stability could lead to the mistaken

inference that additional trials are redundant, whereas a multi-metric force-plate assessment shows that a fuller representation of neuromuscular status—encompassing both outcome and strategy—may emerge only when later repetitions are captured.

A further metric-specific observation concerns vertical stiffness, the only braking/transition index that most frequently peaked in the very first repetition (27.4% of athletes on PB). Three non-exclusive mechanisms may contribute. First, trial-to-trial variability in self-selected countermovement depth directly drives the denominator of our stiffness index, and depth tends to change across repeated efforts as athletes refine perception – action coupling to maximise outcome (Pinder et al., 2011; Sánchez-Sixto et al., 2018). Second, a stiffer motor strategy may be adopted early in the session, when neuromuscular pre-tensioning is highest before fatigue accumulates (J. J. McMahon et al., 2018). Third, anticipatory postural adjustments may diminish across repetitions as familiarisation with the task constraints develops (Renshaw et al., 2022). These mechanisms were not directly examined and should therefore be interpreted as candidate explanations rather than confirmed causes.

### The role of the inter-set rest in a clustered protocol

The findings also underscore the functional role of the 60–90 s inter-set rest in facilitating peak expression during CMJ testing. From a bioenergetic perspective, repeated maximal CMJs rely heavily on a very rapid ATP turnover, buffered by the phosphocreatine (PCr) system via the creatine kinase reaction; consequently, the recovery of high-energy phosphate availability between attempts is largely determined by PCr resynthesis, driven by oxidative ATP production during the resting period. PCr recovery in human muscle follows approximately first-order kinetics and is biphasic (fast and slower components), meaning that short rest periods can yield significant but incomplete PCr recovery, while near-complete recovery typically requires a longer recovery time (Harris et al., 1976; S. McMahon & Jenkins, 2002). In practical terms, a 60–90 s rest period is therefore long enough to significantly increase PCr availability compared to very short intervals between repetitions, yet short enough that some residual metabolic perturbation may persist, creating a plausible window in which fatigue is partially reduced without completely extinguishing the readiness associated with potentiation. Importantly, recovery of the ability to generate high-intensity mechanical power has been shown to be more strongly linked to PCr resynthesis than to normalisation of other parameters/metabolites (e.g., pH), supporting the interpretation that replenishment of high-energy phosphate stores is a key constraint on the subsequent expression of peak force and power (Bogdanis et al., 1995; S. McMahon & Jenkins, 2002). Importantly, neuromuscular activation (electromyography), direct potentiation markers, and phosphocreatine recovery kinetics were not measured in the present study; the mechanistic accounts above are therefore offered as plausible, non-exclusive contributors rather than empirically confirmed explanations.

The clustered 3 + 3 structure appears to support the co-occurrence of processes that are advantageous for maximal performance: preservation (or enhancement) of force- and power-producing capacity in the latter part of the protocol while mitigating fatigue accumulation that may otherwise blunt peak outputs in continuous sequences. This aligns with the broader literature on cluster-set structures, where dividing volume into smaller clusters separated by rest can better maintain or maximise force- and power-related outputs compared with uninterrupted sets (Tufano et al., 2017). In the CMJ context, the mid-protocol rest likely supported partial recovery and potentiation effects, increasing the probability that the second cluster contained the session's best attempt for multiple constructs (Mendez-Villanueva et al., 2012). These results challenge the assumption that peak-performance is necessarily captured within the earliest repetitions of a CMJ assessment. However, the present data should not be interpreted as a direct comparison between a 3 + 3 clustered protocol and continuous three- or five-trial protocols, because the mid-test rest alters the physiological and coordinative conditions under which later repetitions are performed. Rather, the findings indicate that, within the analysed 3 + 3 structure, the inclusion of a mid-test rest increased the opportunity to observe later-occurring peak-values. Consequently, efficiency in monitoring should be considered in relation to the intended use of the test: protocols designed to capture peak capability may require greater volume or a rest structure, whereas protocols designed for rapid longitudinal surveillance may prioritise consistency and feasibility.

### Limitations

Several limitations should be considered. First, the participant population was not homogeneous. Broad inclusion criteria were intentionally applied across sex, age, discipline, training status, and injury history. While this improves ecological validity and reflects real-world monitoring, it increases unexplained variance and limits subgroup-specific inference. Second, the analysis was retrospective and based on routine monitoring, restricting control over participants'

baseline state. Information on perceived readiness, recent training load, sleep, travel, or other contextual factors preceding CMJ assessments was unavailable, and these unmeasured acute factors may have influenced peak-trial timing. Third, female and male participants were pooled in the primary analysis. Although the overall sample included 75 women and 239 men, the retrospective dataset was not designed to isolate sex-specific effects because sex was not balanced. Consequently, the present findings should not be interpreted as evidence that optimal peak-capture behaviour is identical in women and men. Fourth, data were collected over multiple years in an applied environment with ongoing hardware/software updates, which may introduce subtle measurement differences over time. However, it is important to note that the data processing methods and variable calculations did not change during this period. Fifth, athletes received immediate feedback after each trial in the form of JH, This feedback may have influenced subsequent attempts by altering motivation, intent, or movement strategy. Importantly, the feedback was limited to JH, and no real-time scores were provided for mRSI, stiffness, or braking/propulsive force and power variables; nevertheless, feedback-guided adjustment remains a potential confounder in the interpretation.

Finally, the primary analysis used the single testing day on which each athlete achieved their PB mRSI. This design was intentional because the study aimed to examine when peak CMJ performance is captured within a six-trial clustered protocol. However, it also introduces selection bias. Because mRSI was used both to select the testing day and as one of the analysed outcomes, the PB analysis is partly circular for mRSI. The other metrics were not used to select the day, but they were still analysed within an mRSI-defined high-performance state and should therefore be interpreted with caution. For this reason, the findings should not be generalised uncritically to all routine CMJ monitoring sessions. To address this limitation, we repeated the analysis using each athlete's TS session, defined as the eligible testing day whose session-average mRSI was closest to that athlete's median session-average mRSI. Peak-trial distributions were statistically similar between PB and TS analyses for all metrics, suggesting that the late-peak pattern was not driven solely by PB selection. Nevertheless, the results are best interpreted as evidence for peak-capture behaviour within a 3 + 3 clustered CMJ protocol, rather than as a universal description of all monitoring contexts, especially where practitioners prioritise session averages or other robust summaries over single-trial maxima.

### *Practical implications*

From an applied perspective, these results support optimising CMJ monitoring protocols to maximise the probability of capturing the session's true best-trial. Specifically, the findings indicate that protocols should include sufficient trial volume and a clustered structure to capture both outcome and strategy force–time peak-performance. Short, uninterrupted protocols may substantially underestimate peak capability and provide an incomplete picture of neuromuscular status. This is illustrated by the capture probabilities: a three-trial, single-cluster protocol would capture only ~48% of mRSI maxima and ~54% of JH maxima compared with the full six-trial clustered format.

Accordingly, an evidence-informed candidate protocol is a six-jump clustered CMJ format (3 + 3): two sets of three maximal no-arm-swing CMJs with 5–10 s rest between repetitions and 60–90 s rest between sets, consistent with cluster-set evidence supporting maintenance of force and power outputs. Implementation should include strict standardisation of execution constraints (stance, hands-on-hips, no arm swing), a brief quiet-stance period, self-selected countermovement depth, initiation via a consistent audio-visual cue, and cueing that emphasises maximal intent ('*jump as high and as fast as possible*'). Such standardisation supports comparability and measurement stability. Importantly, identifying the best-trial is relevant even when monitoring relies on mean-based summaries, because the presence or absence of a high-quality repetition can meaningfully influence the session average and its interpretation.

Finally, these findings are critical for practitioners and organisations tasked with establishing normative performance benchmarks. Accurate normative databases rely on the premise that the collected reference values reflect the true upper limits of a population's capability. Since the presented data demonstrate that peak-performance is frequently missed in abbreviated protocols, we suggest that future normative data collection should consider using sufficiently voluminous and standardised clustered protocols, or, at minimum, report the trial structure clearly, because abbreviated protocols may underestimate peak-values for some metrics, most clearly for braking-phase variables across both PB and TS sessions. This helps ensure that percentile rankings more closely reflect maximal capability rather than artefacts of insufficient testing volume, reducing the risk of artificially deflated standards.

### *Future research directions*

Future research should extend peak-trial mapping beyond the current metric set by evaluating whether repetition-dependent peak occurrence generalises to additional force–time constructs (e.g., impulse-, velocity-, and phases duration variables) and to other neuromuscular tests frequently embedded in monitoring systems, including drop jumps, loaded CMJ variants, short distance acceleration (e.g., 5 m), and maximal speed assessments (e.g., 30 m). Peak-trial occurrence should also be mapped under different performance states. In practice, routine monitoring captures data across the full spectrum of neuromuscular readiness rather than peak-performance states alone. Future research should extend this comparison to poorer and more variable readiness conditions and examine whether PB and TS peak-trial distributions remain similar across sports, sexes, training phases, and rehabilitation contexts.

Additionally, subgroup-specific analyses are warranted to determine whether optimal protocol structure is population- and context-dependent, thereby enabling more defensible procedure recommendations for homogeneous cohorts. Protocol-design work should also experimentally compare alternative set structures and rest distributions (e.g., 3 + 3 vs 2 + 2 + 2), ideally using prospective and randomised designs, to clarify the required trial volume, optimal intra- and inter-set rest intervals, athlete engagement with different monitoring-session formats, and the balance between testing duration and protocol extension that meaningfully increases the likelihood of capturing the peak-trial. Researchers should remain guided by applied feasibility, because sports science ultimately serves practitioners who often have only a few minutes to implement effective monitoring. Finally, the role of augmented feedback warrants direct testing: manipulating feedback content (e.g., JH vs mRSI vs peak braking velocity) and delivery strategy (athlete-specific vs standardised) may clarify whether feedback systematically shifts peak-trial occurrence and/or improves the representativeness of both peak- and mean-based monitoring approaches.

## Conclusions

The present study provides exploratory evidence that, under the analysed six-trial clustered protocol and PB conditions, the best CMJ attempt often occurred later in the testing session, typically after a short mid-session rest (i.e., in the second cluster of a 3 + 3 protocol). Critically, this pattern was also observed under the TS selection approach, with similar peak-trial distributions between the two methods for every metric. Therefore, the late-peak tendency appears to reflect the 3 + 3 clustered protocol rather than an artefact of PB selection. Within the proposed six-trial clustered protocol, retrospective data showed that five of seven metrics had their highest median standardised score in the second half of the protocol (mRSI, stiffness, ARBF, ARBP, and ARPP), indicating a general tendency towards higher values in later repetitions across both output and strategy variables. Reduced or uninterrupted protocols limited to 3–5 consecutive trials may miss session-best performance and underestimate current neuromuscular capability, particularly when force–time strategy metrics continue to improve despite relatively stable JH. These findings suggest that CMJ monitoring efficiency should be judged against the aim of the assessment: additional attempts combined with brief mid-test recovery may increase the likelihood of capturing session-best values across different force–time metrics. Accordingly, sufficiently voluminous clustered protocols may improve readiness assessment, neuromuscular profiling, normative benchmarking, and return-to-sport evaluation, while, from a theoretical perspective, the delayed peak pattern appears broadly consistent with task-specific tuning, short-term potentiation, and progressive coordination refinement across attempts.

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## Author contributions

CRediT: **Karol Kruczek:** Conceptualization, Formal analysis, Investigation, Methodology, Project administration, Resources, Software, Validation, Visualization, Writing – original draft, Writing – review & editing; **Szymon Karpęcki:** Data curation, Formal analysis, Investigation, Resources; **Tim Gabbett:** Supervision; **Jason Lake:** Supervision; **Michał Nowak:** Supervision, Writing – original draft, Writing – review & editing.

## Disclosure statement

This study was conducted using the Hawkin Dynamics force-plate system and associated software/cloud platform. One author has served as a scientific consultant for Hawkin Dynamics. This relationship had no influence on the study design, data collection, data processing/analysis, interpretation of the results, or manuscript preparation. All other authors declare no conflicts of interest.

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## Consent for publication

All authors have read and approved the final manuscript and agree with the order of authorship.

## Data availability statement

The full script and data are available as supplementary material in a public repository via Zenodo https://doi.org/10.5281/zenodo.18207715.

## Ethics approval and consent to participate

The study was conducted in accordance with the principles of the Declaration of Helsinki. The analysis was based on retrospective data from routinely performed diagnostic and training assessments. The study received a positive opinion from the Research Ethics Committee at the Jan Długosz University in Częstochowa. Resolution No. KE-U/9/2026 of 19 January 2026. Participants were informed that their results could be used for scientific analyses and that they had the right to refuse or withdraw consent to data use at any stage, without any sporting or organisational consequences. All data were processed to ensure confidentiality and protect participant identity.

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