The effect eccentric phase duration on maximal strength, muscular hypertrophy and countermovement jump: A systematic review and meta-analysis

Supplementary materials: https://osf.io/s6uqn/
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ABSTRACT

Some authors suggest slower eccentric tempos enhance resistance training (RT) adaptations, and narrative reviews yield divergent conclusions. PubMed, CINAHL, SPORTDiscus, ProQuest, and Google Scholar were searched following Cochrane Handbook and PRISMA-guidelines, to compare RT outcomes following fast (FEG) and slow (SEG) eccentric phases. Random-effects multi-level meta-analyses with robust variance estimation was performed for strength, hypertrophy, and countermovement jump (CMJ), with results interpreted relative to a region of practical equivalence. Evidence quality was assessed using the Cochrane RoB 2 and GRADE criteria. Eight studies involving 148 participants (52% trained, 80% male) were included. FEG enhanced CMJ by a practically worthwhile degree with moderate certainty (Hedge's *g* = -0.73 [90% CI = -1.34, -0.12; 90% PI = -1.34, -0.12]), while uncertain estimates for maximal strength (*g* = 0.18 [90% CI = -0.27, 0.63; 90% PI = -0.96, 1.31]) and muscle hypertrophy (*g* = 0.03 [90% CI = -0.30, 0.36; 90% PI = -0.32, 0.38]) meant that practically worthwhile effects could neither be supported nor rejected. SEG led to practically equivalent or enhanced strength gains, compared to FEG, in trained participants (*g* = 0.33 [90% CI = 0.07, 0.60; 90% PI = 0.07, 0.60]) and volume-load matched trials (*g* = 0.25 [90% CI = 0.04, 0.45; 90% PI = 0.04, 0.45]) with moderate certainty in subgroup analyses. FEG may enhance CMJ, while SEG may cause similar or higher maximal strength increases in trained participants and volume-load matched conditions. Given the uncertainty of estimates, more research is needed. This project was prospectively registered (https://osf.io/s6uqn/).

1. INTRODUCTION

Resistance training (RT) is the best-known tool to induce robust strength and hypertrophy adaptations. These adaptations are influenced by numerous variables, including volume [1], load [2], and proximity-to-failure [3], among others. However, the impact of tempo (i.e., total and phase-specific repetition speed) has received relatively little attention.

Schoenfeld and colleagues reported extremely slow repetitions (>10 seconds per repetition) produced inferior muscle hypertrophy compared to faster durations (0.5-8 seconds) [4]. Subsequent reviews reported mixed strength and hypertrophy results comparing fast and moderately slow execution tempos [5,6]. Notably, these conclusions were based on a limited number of studies, and many combined forms of tempo manipulation. Thus, it remains challenging to determine distinct effects of altering specific repetition-phase duration versus overall repetition duration.

Emerging evidence suggests individual contraction phase speed may be more relevant for strength and hypertrophy. For instance, a recent meta-analysis found greater strength gains when concentric phases were performed rapidly (<2 seconds) compared to slower (≥2 seconds) (ES = 0.21, $p = 0.029$) [7]. Therefore, faster concentric contractions may better enhance strength, potentially because efforts to slow concentric duration increases difficulty, diminishes neuromuscular performance, and possibly lowers motor unit recruitment, irrespective of total repetition duration [8–10].

Conversely, the impact of extending eccentric phases is less studied. Eccentric-only actions can produce greater or similar magnitudes of hypertrophy than concentric-only contractions and combined eccentric-concentric actions, respectively [11,12]. While slowing the eccentric phase can reduce RT performance [13–16], this acute performance decline may not negatively impact long-term adaptations [17]. Considering eccentric actions require less energy and eccentric strength is higher than concentric [18], but this strength differential decreases as eccentric duration increases [19], greater time under tension or impulse (force × time) per set could potentially be achieved by slowing eccentric duration to a point, making the “cost” of doing so possibly worthwhile. Consequently, slowing the eccentric – to a degree - while maintaining faster concentric contractions may promote greater hypertrophy without impeding strength improvements.

To date, two reviews have qualitatively addressed the impact of slowing down the eccentric duration [10,20], yielding differing conclusions. In addition, study quality assessments were not performed. Given the inconsistency and gaps in the literature, a meta-analytic approach may be more appropriate to assess the impact of different eccentric tempos on muscular performance and hypertrophy. Thus, we conducted a systematic review with meta-analysis seeking to quantitatively evaluate the impact of varying eccentric tempos on indices of muscular performance and hypertrophy.

2. METHOD

2.1 Overview and registration

This systematic review was performed in accordance with the *Cochrane Handbook for Systematic Reviews of Interventions* (version 6.4.0) and the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) statement [21] guidelines. The review was prospectively registered on the Open Science Framework after pilot searches (https://osf.io/s6uqn/), but before any formal searches or record screening.

2.2 Search strategy

A comprehensive search strategy using various terms related to resistance exercise and eccentric phase duration was developed in accordance with the PRISMA-S checklist [22], assisted by three specialised tools: the Word Frequency Analyzer (https://sr-accelerator.com/#/wordfreq), Research Refiner (https://sr-accelerator.com/#/searchrefinery), and Polyglot Search Translator (https://sr-accelerator.com/#/polyglot) [23,24]. The searches were conducted from inception to November 10th, 2023 in PubMed/MEDLINE, SCOPUS, SPORTDiscus, CINAHL, ProQuest, and Google Scholar. Search results from Google Scholar was mass exported using the Publish or Perish software [25]. This comprehensive approach was designed to maximise the chances of locating both peer-reviewed and grey literature. For Google Scholar, the search was limited to the first 1,000 hits (980 results were retrieved due to software error), and for ProQuest, searches were limited to scholarly journals, dissertations, and theses. No other predefined limits or filters were applied. The full search strings can be found in **Supplementary file 1**. Additionally, secondary searches were performed, including: 1) Screening reference lists of all included studies and relevant review papers; 2) Forward citation tracking through Google Scholar for the included studies; and 3) Ongoing search alerts after the initial search date until the week before submission.

2.3 Text screening and selection

After removal of duplicates, titles and abstracts were independently screened for eligibility by the two researchers (CA and AK) using the systematic review software Rayyan (https://rayyan.ai/). Disagreements were resolved through discussion. For all remaining articles, full-texts were included if they investigated the impact of eccentric phase duration as the independent variable, classified as either fast (i.e., ≤2 sec or maximal intent) or slow (i.e., >2 sec), and 1) were published in a peer-reviewed journal, on a pre-print repository, or as a Master's or PhD thesis; 2) written in English; 3) included healthy human participants with no disease or musculoskeletal injury; 4) used parallel groups or contralateral, within-participants designs to assess the effects of altered eccentric phase duration on indices of muscle strength, power, endurance, and/or hypertrophy; 5) used traditional combined eccentric/concentric isotonic contractions; 6) matched the duration of all contraction phases except the eccentric (i.e. concentric and possible isometric transition phases); 7) attempted to match frequency, relative-load (% of 1RM or RM relative to tempo), and volume (number of sets or volume-load) between groups; and 8) were at least 4 weeks long. Studies employing eccentric-only and overloaded eccentrics were excluded.

2.4 Study coding and extraction

The following data were extracted from the included studies: 1) number of participants and their age, sex, and training experience; 2) RT protocol including duration, frequency, volume, relative-load, rest periods, and exercise selection; 3) study design; 4) tempo of each contraction phase; 5) outcome measures; 6) the presence of concurrent training; and 7) mean changes and standard deviations of the relevant indices of muscular performance and hypertrophy, as well as within-group correlations of pre-post scores. Where insufficient data were reported, corresponding authors were contacted by email twice over two months. If they failed to provide the required data, data were extracted using WebPlotDigitizer and/or calculated in accordance with the Cochrane Handbook [26]. All data extraction was completed by CA and coding files were cross-checked by AK for accuracy and differences were resolved through discussion and consensus.

2.5 Risk of bias assessment

Risk of bias (RoB) was performed using the second version of the Cochrane RoB tool for randomised trials (RoB 2) [27], in accordance with the Cochrane Handbook [28]. The effect of adhering, not assignment, to the protocol was the effect of interest. Studies were categorised as: "Low risk of bias" if all domains were assessed as low risk; "Some concerns" if concerns were raised in one domain, but no domain was determined to be high risk; "High risk of bias" if at least one domain was classified as high risk or if multiple domains raised some concern. The Grading of Recommendations Assessment, Development and Evaluation (GRADE) system was used to evaluate evidence certainty for studies included in the quantitative synthesis, using the GRADEpro GDT software [29], in accordance with the GRADE [30] and Cochrane [31] handbooks, including guidance updates for inconsistency [32] and imprecision [33]. Both RoB and GRADE assessment was performed independently by two researchers (CA and AK). Disagreements were resolved through discussion.

2.6 Statistical analysis

2.6.1 Multi-level synthesis

Quantitative synthesis was performed for separate outcomes when reported by at least two studies. Analyses were performed using R Statistical Software (v4.3.0; R Core Team 2021) [34]. Given that multiple effect sizes were reported in included studies (e.g., more than one slower eccentric phase duration or multiple measures for the same outcome within a study), a three-level (i.e., study, group, and outcome) mixed-effect meta-analysis with robust variance estimation were fitted with *metafor* [35] and *clubSandwich* [36] packages. All multi-level meta-analysis models were based on a t-distribution due to the small sample sizes of the included studies [37]. Model parameters were estimated using restricted maximum likelihood estimation. Effect sizes were weighted based on their inverse sampling variance. A within-study effect size sampling error correlation coefficient of $\rho = 0.6$ was assumed for robust variance estimation. Sensitivity analyses with lower ($\rho = 0.4$) and higher ($\rho = 0.8$) values were performed to check robustness. Sufficient studies were found for maximal strength, hypertrophy, and countermovement jump height (CMJ). For all multi-level models, I^2 and τ^2 were calculated to quantify relative and absolute between study heterogeneity, respectively. I^2 and τ^2 were partitioned across levels 2 (within-study) and 3 (between study) of the multi-level meta-analysis. Publication bias was assessed using the multi-level extension of the Egger's test [38] and by funnel plot visual inspection for observed effect sizes and conditional residuals [39].

Pre-planned subgroup analyses were performed for i) study design (categorical: between- and within-participant designs) and ii) training status (categorical: trained and untrained), while exploratory subgroup and moderator analyses were performed when at least two studies were represented in each subgroup, based on visual inspection of the forest plot and the study characteristics. These subgroups were: i) exercise selection (categorical: multi-joint and single-joint), ii) muscle group (categorical: elbow flexor and anterior thigh), iii) volume matching (categorical: sets to failure and volume-load).

2.6.2 Effect size calculation

Due to the different tests used to measure maximal strength (e.g., Scott curl, back squat, etc.), hypertrophy (e.g., ultrasound and MRI), and CMJ (e.g., with or without arm swing), calculating a standardised mean difference (SMD) was deemed more appropriate than using raw mean differences. SMD with a small sample size correction (Hedges' *g*) was calculated, in accordance with Borenstein and colleagues [40], such that a positive effect size favoured SEG. To account for the inter-dependencies of effects in within-participant designs, separate effect size calculations were used for between-participant (i.e., parallel group designs [equations 1-3]) and within-participant designs (i.e., contralateral limb designs [equations 4-7]), and then combined in the final analysis [40].

Equation 1: $$g = \frac{\overline{X_1} - \overline{X_2}}{SD_{pooled}} J,$$

Equation 2: $$SD_{pooled} = \sqrt{\frac{(n_1 - 1)SD_1^2 + (n_2 - 1)SD_2^2}{n_1 + n_2 - 2}},$$

Equation 3: $$J = 1 - \frac{3}{4(n_1 + n_2 - 2) - 1},$$

where $\overline{X_1}$ and $\overline{X_2}$ are the mean group change scores, SD_{pooled} is the pooled standard deviation, J is a small sample size correction, and n_1 and n_2 are the sample sizes of each group.

Equation 4: $$g = \frac{\overline{Y_1} - \overline{Y_2}}{SD_{pooled}} J,$$

Equation 5: $$SD_{pooled} = \frac{S_{diff}}{\sqrt{2(1-r)}},$$

Equation 6: $$SD_{diff} = \sqrt{SD_1^2 + SD_2^2 - 2 * r * SD_1 * SD_2},$$

Equation 7: $$J = 1 - \frac{3}{4(n-1) - 1},$$

where $\overline{Y_1}$ and $\overline{Y_2}$ are the mean change scores for each limb, SD_{diff} is the standard deviation of the within-participant difference between change scores, and *r* is correlation of the between-limb within-participant change scores.

2.6.3 Model Diagnostics

To identify potential outliers and influential observations, Cook’s distance and hat values were calculated in *metafor* [35]. Model diagnostics were not performed for jump height due to the small number of included studies (k = 3). Cook’s distance was judged according to the F distribution where α = 0.50 and [41]:

$$df = (k + 1, n - k - 1)$$

Hat values (*h*) were deemed noteworthy where exceeding twice the average [42]:

$$\bar{h} = \frac{k + 1}{n}$$

Where potential outliers or influential observations were identified, a separate meta-analysis was performed with the corresponding potential outlier/influential study removed and the results were compared to the main meta-analysis to check the interpretation (i.e., direction of effect, confidence [CI] and prediction intervals [PI]) [43]. This check was conducted as a sensitivity analysis since it is not possible to distinguish between (large) sampling error and true outliers [44].

2.6.4 Statistical inferences

A limitation of traditional meta-analyses with null hypothesis significance testing (NHST) is that they can reject the absence of an effect but cannot support it [45,46]. This often leads to erroneous conclusions of 'no effect' when no statistically significant differences are observed. A non-significant difference may simply indicate substantial uncertainty and the need for additional research [46,47]. Therefore, we adopted equivalence and minimum effects procedures [45,48], within an estimation-based approach [49]. As such, the practical implications of all results compatible with the data, including their precision (90% CI and PI), were interpreted, relative to our region of practical equivalence (ROPE) [48]. Evidence of no practically worthwhile effects is supported if the point estimate and its entire 90% CI and PI falls within our ROPE (i.e., effects are practically equivalent to zero) [45]. Conversely, to support a practically worthwhile effect, the point estimate and its entire 90% CI and PI must fall outside of our ROPE. Any 90% CIs and PIs that overlap with our ROPE are considered uncertain, making the absence or presence of practically worthwhile effects unclear. Inferences from equivalence and minimum effects procedures are normally only interpreted relative to the 90% CI [48]. To account for between-study heterogeneity, 90% PIs were also used to understand the precision and reliability of the point estimate from included studies (CI), as well as the possible range of effects when applying the results of this meta-analysis to new studies or practical scenarios (PI). However, 95% CI, 95% PI and *p*-values (two-tailed NHST) were reported to allow readers to interpret results through their preferred lens, but they did not influence our inferences, except for GRADE imprecision [33].

We based our ROPEs on meta-analytic data [1,50], with effects less than ±0.25, ±0.28, and ±0.11 for maximal strength, hypertrophy, and CMJ, respectively, considered practically not worthwhile. These thresholds align with the 25th percentile of RT effects on the given outcomes [50]. Although no specific recommendations were provided for hypertrophy, the 95% credible interval (0.10 to 0.66) of RT vs. control for hypertrophy from 119 studies by Currier and colleagues [1] was used to estimate the 25th percentile of effects, assuming a normal distribution. The same data were also used to delineate small (maximal strength: 0.25 to <0.59; hypertrophy: 0.28 to <0.38; CMJ: 0.11 to <0.38), moderate (50^{th} percentile [maximal strength: 0.59 to <0.98; hypertrophy: 0.38 to <0.48; CMJ: 0.38 to <0.67]), and large (75^{th} percentile [maximal strength: ≥0.98; hypertrophy: ≥0.48; CMJ: ≥0.67]) effect size magnitudes for each outcome.

3. Results

3.1 Selection of sources of evidence

Our systematic search yielded 3227 results. After removing duplicates, 2195 articles were eligible for title and abstract screening. Subsequently, 48 articles underwent full-text review, with 38 excluded due to: i) non-accessible full-text, ii) incorrect study design, iii) combined ECC-CON isotonic contractions not performed, iv) no manipulation of eccentric tempo, v) lack of matched concentric tempo between groups, and vi) comparison of two eccentric tempos, both of which considered slow by the inclusion criteria. Consequently, 9 articles were included, with two reporting data from the same study, resulting in 8 unique studies [17,51–57]. For PRISMA-flowchart, see Figure 1. Additional data was requested from all first and/or corresponding authors, of which all but one [52] responded. As such, mean changes, change SD, correlations of within-group pre-post scores and within-participant between-limb change scores (for within-participant designs) or sufficient data to estimate them were provided either directly by authors [51,53], calculated from raw data [17,54–57] or estimated from extracted full-text data [52], in accordance with the Cochrane Handbook [26].

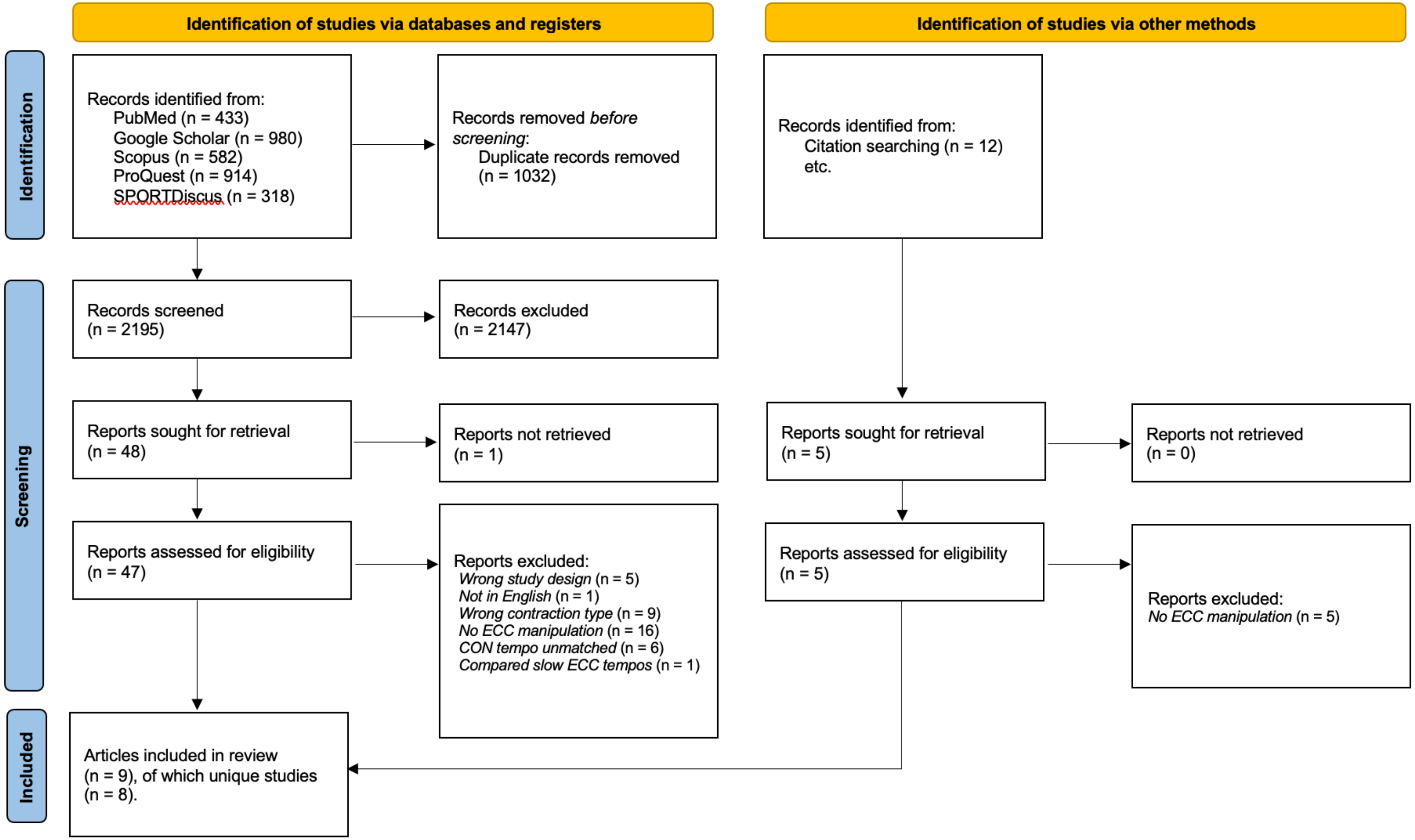
Identification of studies via databases and registers
Identification of studies via other methods
Identification
Screening
Included
Records identified from:
PubMed (n = 433)
Google Scholar (n = 980)
Scopus (n = 582)
ProQuest (n = 914)
SPORTDiscus (n = 318)
Records removed before screening:
Duplicate records removed (n = 1032)
Records identified from:
Citation searching (n = 12)
etc.
Records screened (n = 2195)
Records excluded (n = 2147)
Reports sought for retrieval (n = 48)
Reports not retrieved (n = 1)
Reports sought for retrieval (n = 5)
Reports not retrieved (n = 0)
Reports assessed for eligibility (n = 47)
Reports excluded:
Wrong study design (n = 5)
Not in English (n = 1)
Wrong contraction type (n = 9)
No ECC manipulation (n = 16)
CON tempo unmatched (n = 6)
Compared slow ECC tempos (n = 1)
Reports assessed for eligibility (n = 5)
Reports excluded:
No ECC manipulation (n = 5)
Articles included in review (n = 9), of which unique studies (n = 8).

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Fig. 1 PRISMA-flowchart. *ECC* eccentric tempo, *CON* control

3.2 Study characteristics

The pooled number of participants across all studies was 148, of which 52% (n = 77) and 80% (n = 118) were resistance-trained and male, respectively. The median sample size per comparator group was 10 participants (range: 6-13). Two studies employed a within-participants design [56,57], while the remaining utilised a parallel-groups design. The median intervention period was 7.5 weeks (range: 4-12 weeks). All studies required participants to train twice weekly, with a median 3.5 sets per exercise (range: 2-5 sets). Two studies incorporated 5-7 exercises per session [51,54], while the rest included one. The fast eccentric groups (FEG) maintained a median tempo of 1.25 seconds (range: 1-2 seconds) and 1 second (range: 1-2 seconds) in the eccentric and concentric parts of the movement, respectively. The slow eccentric groups (SEG) took a median 4 seconds (range: 3-6 seconds) and 1 second (range: 1-2 seconds) to lower and lift the weight, respectively. Four studies matched volume-load (sets × repetitions × load) between groups [51,52,54,56], allowing for variations in proximity-to-failure. The remaining four had groups perform an equal number of sets to failure [17,53,55,57], allowing differences in performed repetitions or absolute load. All studies evaluated 1RM strength, on either the free-weight [53,54] or smith-machine [52] back squat, leg press [51], leg extension [51,56,57], or Scott curl [17,55]. Five studies measured hypertrophy of the elbow flexors [17,55] or anterior thigh [53,56,57] using ultrasound [17,55–57] or MRI [53]. Three studies assessed countermovement jump (CMJ) [52–54]. Tensiomyography [55], squat jump [53], loaded CMJ [52], functional capacity tests (6 m walk, timed up-and-go, stair-climbing, and stair-rising) [51], 20 m sprint [54], 505 agility [54], T-test [53], and Yo-Yo intermittent recovery test level 2 [53] were also assessed. However, these results were not extracted for meta-analysis due to an insufficient number of studies. An overview of the studies is available in Table 1.

268 Table 1 Study overview

Study	Participants	Training protocol	Tempo (s) [ECC/ISO/CON]	Primary outcome(s)
Dias et al., 2015	FEG: 68 ± 7 y *n* = 10F SEG: 66 ± 6 y *n* = 9F RT experience: Untrained	Exercise/s: Leg press, leg extension, among others*. Prescription: 12 weeks, 2 sessions/week. 2-3 sets of 8-12 repetitions at 45-70% of 1RM, 2-3 mins inter-set rest.	FEG: 1.5/0/1.5 SEG: 4.5/0/1.5	Strength: 1RM leg press and leg extension.
Pereira et al., 2016	FEG: 28 ± 8 y *n* = 6M SEG: 30 ± 6 y *n* = 6M RT experience: Trained (≥1 y)	Exercise/s: Scott curl. Prescription: 12 weeks, 2 sessions/week. 3 sets of 8 repetitions at 8RM, 2 mins inter-set rest.	FEG: 1/0/1 SEG: 4/0/1	Strength: 1RM Scott curl; Hypertrophy: Biceps brachii MT via US.
Mike et al., 2017	FEG: 22 ± 2 y *n* = 10M SEG1: 22 ± 2 y *n* = 9M SEG2: 23 ± 4 y *n* = 11M	Exercise/s: Smith machine squat. Prescription: 4 weeks, 2 sessions/week. 4 sets of 6 repetitions at 80-85% of 1RM, 3 mins inter-set rest.	FEG: 2/1/2 SEG1: 4/1/2 SEG2: 6/1/2	Strength: 1RM Smith machine squat; Power: CMJ.

	RT experience: Trained (≥3 y)			
Kojić et al., 2021	FEG: 28 ± 8 y *n* = 6M, 4F SEG: 30 ± 6 y *n* = 5M, 5F RT experience: Untrained	Exercise/s: Scott curl. Prescription: 7 weeks, 2 sessions/week. 3-4 sets at 60-70% of 1RM to failure, 2 mins inter-set rest.	FEG: 1/0/1 SEG: 4/0/1	Strength: 1RM Scott curl; Hypertrophy: Biceps brachii MT via US.
Shibata et al., 2021	FEG: 20 ± 1 y *n* = 11M SEG: 20 ± 1 y *n* = 11M RT experience: Untrained	Exercise/s: Back squat. Prescription: 6 weeks, 2 sessions/week. 3 sets at 75% of 1RM to failure, 3 mins inter-set rest.	FEG: 2/0/2 SEG: 4/0/2	Strength: 1RM back squat; Hypertrophy: Quadriceps CSA at 30%, 50% and 70% of femur length via MRI; Power: CMJ.
Pearson et al., 2022	FEG: 23 ± 10 y *n* = 13M SEG: 23 ± 10 y *n* = 13M RT experience: Trained (≥3 y)	Exercise/s: Unilateral leg extension. Prescription: 8 weeks, 2 sessions/week. 3-4 sets of 8-10 repetitions at 8-10RM, 2 mins inter-set rest. Within-participants design.	FEG: 1/0/1 SEG: 3/0/1	Strength: 1RM unilateral leg extension; Hypertrophy: Anterior thigh MT at 40% and 60% of femur length via US.

Azevedo et al., 2022	FEG: 25 ± 5 y *n* = 8M, 2F SEG: 25 ± 5 y *n* = 8M, 2F RT experience: Untrained	Exercise/s: Unilateral leg extension. Prescription: 8 weeks, 2 sessions/week. 5 sets at 70% of 1RM to failure, 3 mins inter-set rest. Within-participants design.	FEG: 2/0/1 SEG: 4/0/1	Strength: 1RM unilateral leg extension; Hypertrophy: Rectus femoris, vastus medialis and vastus lateralis MT via US.
Segers et al. 2022	FEG: 22 ± 3 y *n* = 11M SEG: 22 ± 3 y *n* = 11M RT experience: Trained (≥2 y)	Exercise/s: Back squat, hex bar deadlift, among others. Prescription: 4 weeks, 2 sessions/week. 3-4 sets of 3-4 repetitions at 70-80% of 1RM.	FEG: 1/0/X SEG: 4/0/X X = maximal intent	Strength: 1RM back squat; Power: CMJ.

269 * 'Among others' indicate that other exercises were also performed, typically as part of a whole-body routine, but were either irrelevant
270 for the measured outcomes or not subject to tempo manipulation. *RT* resistance training, *ECC* eccentric, *ISO* isometric, *CON* concentric,
271 *M* males, *F* female, *RM* repetition maximum, *CMJ* countermovement jump, *MT* muscle thickness, *US* ultrasound

3.3 Study quality and certainty of evidence

One study [56] had a low risk of bias regarding the randomisation process, while the remaining seven studies were rated as "some concerns" due to insufficient information about randomisation procedures and allocation sequence concealment. For deviations from intended interventions, seven studies had a low risk of bias. One study [17] had some concerns due to a lack of blinding and insufficient information about external training influences. All studies had a low risk of bias concerning missing outcome data, as either no dropouts or equally distributed dropouts were reported. Regarding the measurement of outcomes, one study [17] had a high risk of bias for strength measurements, while seven had some concerns. For hypertrophy outcomes, three studies [17,55,57] had some concerns, while two had a low risk of bias [53,56]. Regarding power measurements, one study was low risk [53], while the remaining two had some concerns [52,54]. The primary sources of bias in these domains were lack of blinding of assessors and use of methods susceptible to human influence. All studies had some concerns related to the selection of reported results because none had pre-registered methods, making it impossible to assess deviations from a specified plan. In summary, one study [17] had a high overall risk of bias, while the remaining seven studies had some concerns overall (Figure 2, Supplementary files 2 and 3). Funnel plot visual inspection did not suggest publication bias (Supplementary file 3), which was corroborated by the Egger's test for maximal strength ($p = 0.75$), hypertrophy ($p = 0.31$), and CMJ ($p = 0.39$). The certainty of evidence for pooled and subgroup analyses can found in Table 2 and Supplementary file 4.

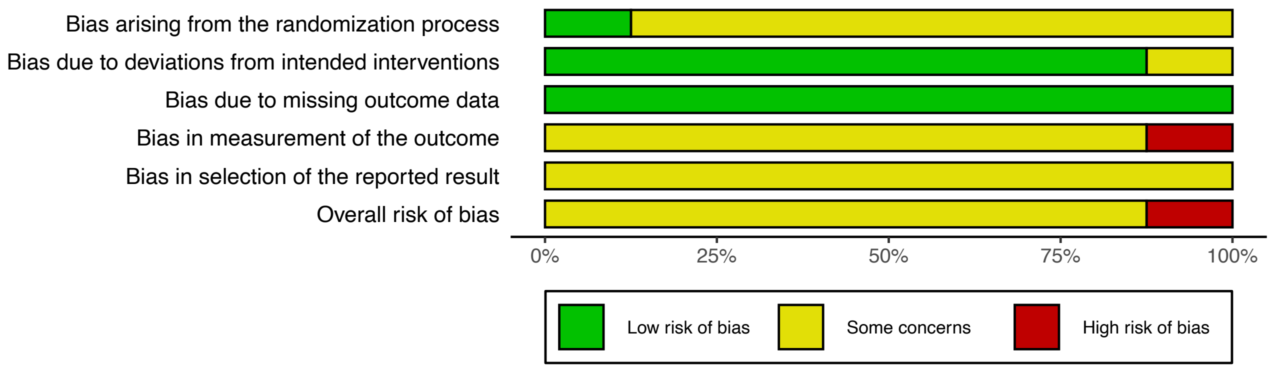

Fig. 2 Risk of bias assessment

3.4 Quantitative analysis

3.4.1 Maximal strength

The pooled analysis for maximal strength revealed that the average effect most compatible with our data, given our statistical model, was a practically not worthwhile effect favouring SEG with poor precision (Figure 3). The effect size ranged from a small effect favouring FEG to a moderate effect favouring SEG (0.18 [90% CI = -0.27, 0.63; 90% PI = -0.96, 1.31; 95% CI = -0.39, 0.74; 95% PI = -1.24, 1.60], p = 0.48, I^2_{within} = 0%, $I^2_{between}$ = 65.8%, τ^2_{within} < 0.01, $\tau^2_{between}$ = 0.30). Considering our ROPE (±0.25), the 90% CI, and 90% PI, these results are highly uncertain, and the presence or absence of a practically worthwhile effect of eccentric tempo manipulation on maximal strength can neither be supported nor rejected.

Subgroup analyses found small effects with poor precision, ranging from practically not worthwhile to small or moderate effects favouring SEG. This was observed when i) participants were trained (0.33 [90% CI = 0.07, 0.60; 90% PI = 0.07, 0.60]), and ii) volume-load was matched (0.25 [90% CI = 0.05, 0.44; 90% PI = 0.05, 0.44]). Considering our ROPE (±0.25), the 90% CI, and 90% PI, SEG does not practically harm maximal strength gains in these subgroups, but whether it enhances maximal strength gains by a practically worthwhile degree cannot be inferred with certainty.

The remaining subgroup analyses and moderator analyses found practically not worthwhile to small average effects, with very poor precision, ranging from small to large in both directions (Tables 2 and 3). Considering our ROPE (±0.25), the 90% CI, and 90% PI, these results are highly uncertain, and the presence or absence of a practically worthwhile effect of eccentric tempo manipulation on maximal strength cannot be supported or rejected.

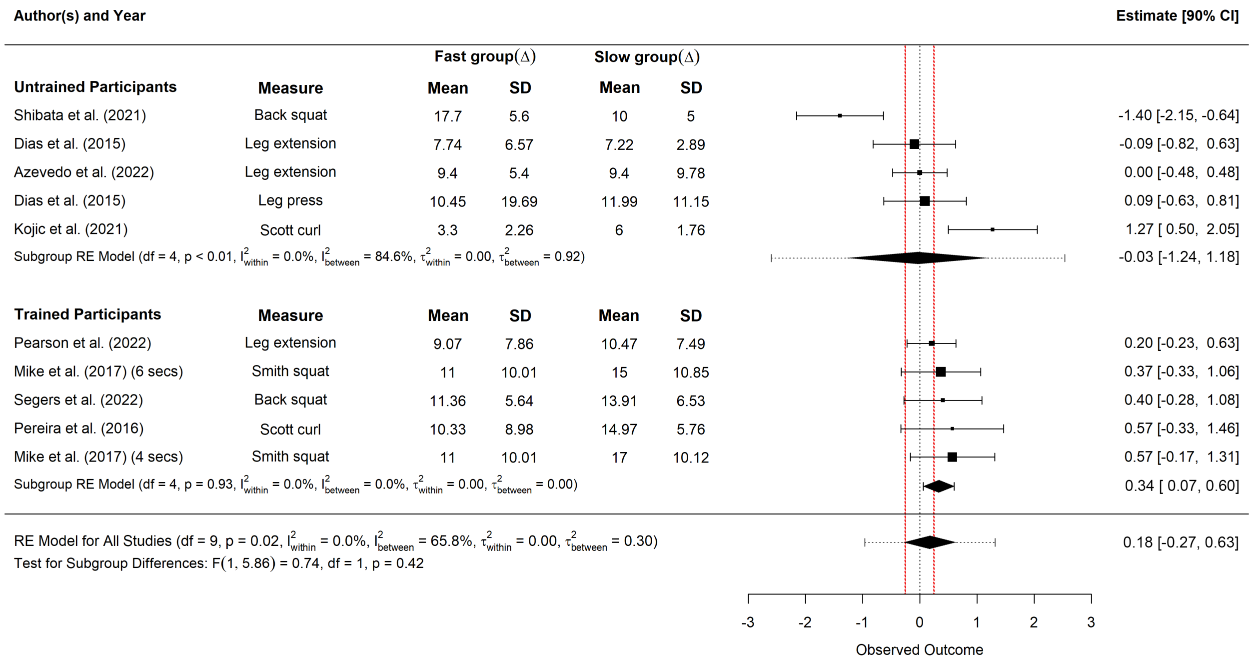

Fig. 3 Forest plot for pooled maximal strength analysis, including subgroup and moderator analysis for training status. The red lines represent the lower and upper bounds of our region of practical equivalence (±0.25). *CI* confidence interval, *SD* standard deviation

3.4.2 Muscle hypertrophy

The pooled analysis for hypertrophy revealed that the average effect most compatible with our data, given our statistical model, was a practically not worthwhile effect favouring SEG with poor precision (Figure 4). The effect size ranged from a moderate effect favouring FEG to a large effect favouring SEG (0.03 [90% CI = -0.30, 0.36; 90% PI = -0.32, 0.38; 95% CI = -0.42, 0.48; 95% PI = -0.45, 0.51], p = 0.86, I^2_{within} = 0.0%, $I^2_{between}$ = 2.2%, $\tau^2_{within} < 0.01$, $\tau^2_{between} < 0.01$). Considering our ROPE (±0.28), the 90% CI, and 90% PI, these results are highly uncertain, and the presence or absence of a practically worthwhile effect of eccentric tempo manipulation on hypertrophy can neither be supported nor rejected.

Subgroup analyses and moderator analyses found practically not worthwhile to moderate average effects with very poor precision and large effects in both directions (Tables 2 and 3). Considering our ROPE (±0.28), the 90% CI, and 90% PI, these results are highly uncertain, and the presence or absence of a practically worthwhile effect of eccentric tempo manipulation on hypertrophy cannot be supported or rejected.

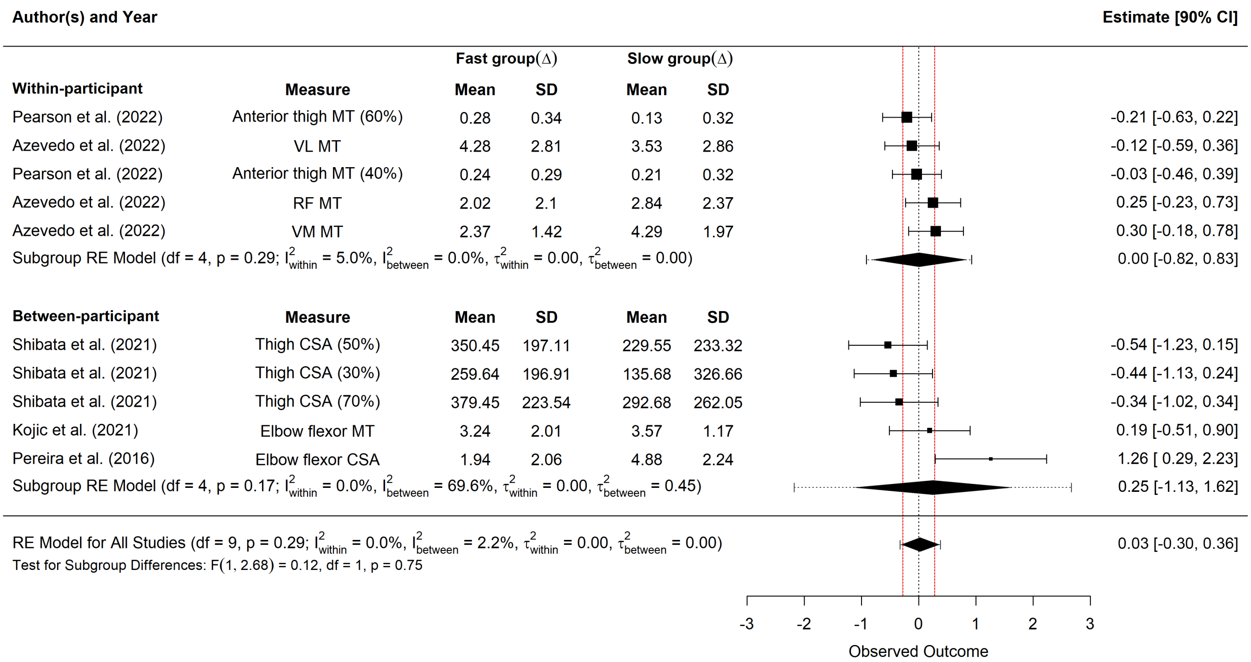

Fig. 4 Forest plot for pooled muscle hypertrophy analysis, including subgroup and moderator analysis for study design. The red lines represent the lower and upper bounds of our region of practical equivalence (±0.28). *CI* confidence interval, *SD* standard deviation, *CSA* Cross-sectional area, *MT* muscle thickness, *VL* vastus lateralis, *RF* rectus femoris, *VM* vastus medialis

3.4.3 Countermovement jump height

The pooled analysis for CMJ revealed that the average effect most compatible with our data, given our statistical model, was a large effect favouring FEG with poor precision (Figure 5). The effect size ranged from large effects favouring FEG to small effects favouring SEG (-0.73 [90% CI = -1.34, -0.12, 90% PI = -1.34, -0.12; 95% CI = -1.64, 0.17; 95% PI = -1.64, 0.17], $p = 0.07$, $I^2_{within} = 0\%$, $I^2_{between} = 0\%$, $\tau^2_{within} < 0.01$, $\tau^2_{between} < 0.01$). Considering our ROPE (±0.11), the 90% CI, and 90% PI, there is evidence to support that FEG enhances CMJ improvement by a practically worthwhile degree, compared to SEG.

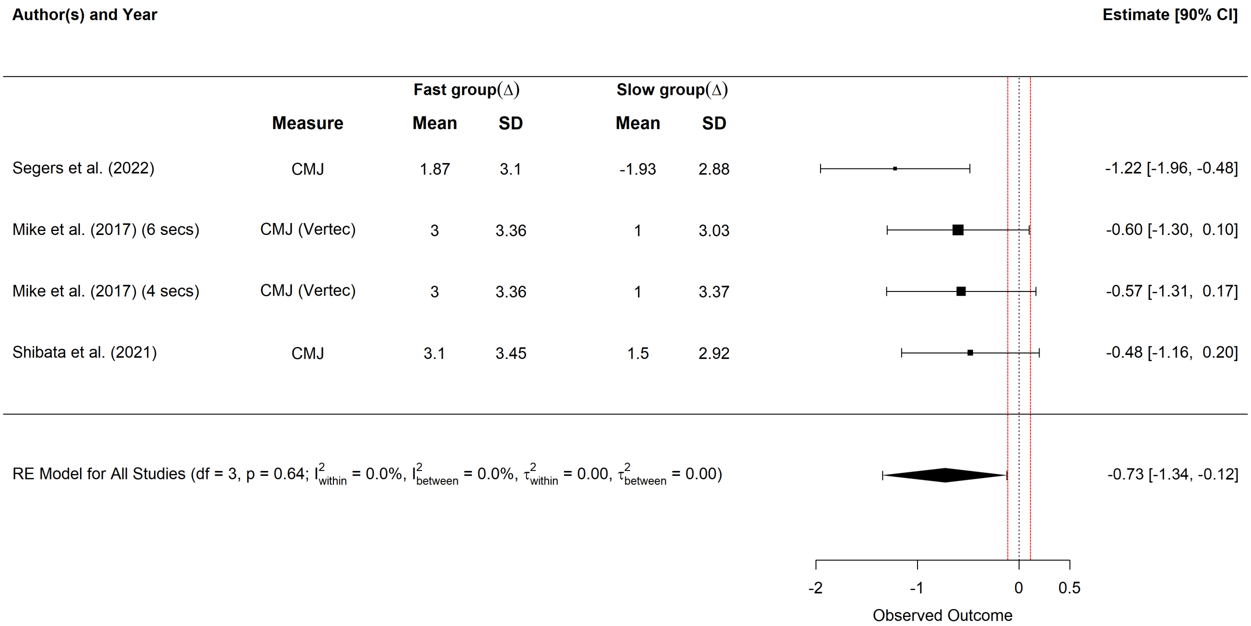

Fig. 5 Forest plot for pooled countermovement jump analysis. The red lines represent the lower and upper bounds of our region of practical equivalence (±0.11). *CI* confidence interval, *SD* standard deviation, *CMJ* Countermovement Jump

3.4.4 Sensitivity analyses

The results for maximal strength and hypertrophy were both robust to sensitivity analysis with a lower and higher imputed sampling error correlation co-efficient (ρ = 0.4 and 0.8) (see full sensitivity analyses in **Supplementary file 5**). Hat values were not violated for maximal strength or hypertrophy, nor Cook's distance for maximal strength, but a potential outlier was identified as violating the threshold for Cook's distance for hypertrophy. Removing this study [56] from the analysis did not change the interpretation of the model estimate (practically not worthwhile effect size favouring SEG; from g = 0.03 to 0.16) but increased its' uncertainty (95% CI from [-0.43, 0.48] to [-0.71, 1.03].

Table 2 Summary of findings

Outcome (*n, k*)	**Hedge's *g* (90% CI) [90% PI]**	***p*-value (95% CI) [95% PI]**	I^2_{within}, $I^2_{between}$ [τ^2_{within}, $\tau^2_{between}$]	**Certainty of evidence (GRADE)**
Strength				
Pooled (*n = 148, k = 8*) *Follow-up: 4-12 weeks*	**0.18** (-0.27, 0.63) [-0.96, 1.31]	0.48 (-0.39, 0.74) [-1.24, 1.60]	0.0%, 65.8% [< 0.01, 0.30]	⊕⊕⊖⊖ **Low**[b,f,h]
Trained (*n = 77, k = 4*) *Follow-up: 4-12 weeks*	**0.34** (0.07, 0.60) [0.07, 0.60]	0.06 (-0.04, 0.72) [-0.04, 0.72]	0.0%, 0.0% [<0.01, <0.01]	⊕⊕⊕⊖ **Moderate**[e]
Untrained (*n = 71, k = 4*) *Follow-up: 6-12 weeks*	**-0.03** (-1.24, 1.18) [-2.60, 2.53]	0.95 (-1.68, 1.61) [-3.50, 3.43]	0.0%, 84.6% [<0.01, 0.92]	⊕⊖⊖⊖ **Very low**[c,g]
Multi-joint (*n = 93, k = 4*) *Follow-up: 4-12 weeks*	**-0.11** (-1.08, 0.86) [-2.07, 1.85]	0.80 (-1.43, 1.21) [-2.76, 2.54]	0.0%, 73.3% [<0.01, 0.52]	⊕⊖⊖⊖ **Very low**[c,g]
Single-joint (*n = 74, k = 4*) *Follow-up: 7-12 weeks*	**0.40** (-0.22, 1.03) [-0.68, 1.48]	0.23 (-0.47, 1.27) [-1.10, 1.90]	24.9%, 24.9% [0.06, 0.06]	⊕⊖⊖⊖ **Very low**[c,f]
Sets-to-failure (*n = 64, k = 4*) *Follow-up: 7-12 weeks*	**0.10** (-1.21, 1.41) [-2.64, 2.84]	0.87 (-1.67, 1.87) [-3.60, 3.81]	42.6%, 42.6% [0.52, 0.52]	⊕⊖⊖⊖ **Very low**[d,g]

Volume-load (*n = 84, k = 4*) *Follow-up: 4-12 weeks*	**0.25** (0.04, 0.45) [0.04, 0.45]	0.07 (-0.03, 0.53) [-0.03, 0.53]	0.0%, 0.0% [<0.01, <0.01]	⊕⊕⊕⊖ **Moderate**[e]
Between-participants (*n = 125, k = 6*) *Follow-up: 4-12 weeks*	**0.21** (-0.50, 0.93) [-1.46, 1.88]	0.58 (-0.70, 1.13) [-1.92, 2.34]	0.0%, 73.4% [<0.01, 0.56]	⊕⊖⊖⊖ **Very low**[c,g]
Within-participants (*n = 23, k = 2*) *Follow-up: 8 weeks*	**0.11** (-0.52, 0.75) [-0.52, 0.75]	0.47 (-1.16, 1.39) [-1.16, 1.39]	0.0%, 0.0% [<0.01, <0.01]	⊕⊖⊖⊖ **Very low**[g]
Hypertrophy				
Pooled (*n = 77, k = 5*) *Follow-up: 6-12 weeks*	**0.03** (-0.30, 0.36) [-0.32, 0.38]	0.86 (-0.42, 0.48) [-0.45, 0.51]	0.0%, 2.2% [<0.01, <0.01]	⊕⊖⊖⊖ **Very low**[c,f]
Trained (*n = 25, k = 2*) *Follow-up: 8-12 weeks*	**0.46** (-3.84, 4.77) [-6.49, 7.42]	0.62 (8.20, 9.13) [-13.54, 14.47]	0.0%, 87.5% [<0.01, 0.75]	⊕⊖⊖⊖ **Very low**[a,c,g]
Untrained (*n = 52, k = 3*) *Follow-up: 6-8 weeks*	**-0.01** [-0.62, 0.61] [-0.68, 0.67]	0.97 (-0.97, 0.96) [-1.06, 1.05]	1.2%, 4.3% [<0.01, <0.01]	⊕⊖⊖⊖ **Very low**[c,g]
Arm (*n = 32, k = 2*) *Follow-up: 7-12 weeks*	**0.29** (-0.82, 1.40) [-1.57, 2.16]	0.51 (-1.39, 1.97) [-2.53, 3.11]	0.0%, 66.4% [<0.01, 0.23]	⊕⊖⊖⊖ **Very low**[a,c,g]
Thigh (*n = 45, k = 3*) *Follow-up: 6-8 weeks*	**-0.09** (-1.89, 1.71) [-2.60, 2.42]	0.80 (-3.73, 3.54) [-5.14, 4.97]	1.3%, 38.5% [<0.01, 0.08]	⊕⊖⊖⊖ **Very low**[c,g]
Between-participants (*n = 54, k = 3*) *Follow-up: 6-12 weeks*	**0.25** (-1.13, 1.62) [-2.18, 2.67]	0.65 (-1.79, 2.29) [-3.34, 3.84]	0.0%, 69.6% [<0.01, 0.45]	⊕⊖⊖⊖ **Very low**[d,g]

Within-participants (*n = 23, k = 2*) *Follow-up: 8 weeks*	**0.00** (-0.82, 0.83) [-0.91, 0.92]	0.98 (-1.66, 1.67) [-1.84, 1.85]	5.0%, 0.0% [<0.01, <0.01]	⊕⊖⊖⊖ **Very low**[g]
CMJ				
Pooled (*n = 74, k = 3*) Follow-up: 4-6 weeks	**-0.73** (-1.34, -0.12) [-1.34, -0.12]	0.07 (-1.64, 0.17) [-1.64, 0.17]	**0.0%, 0.0%** **[<0.01, <0.01]**	⊕⊕⊕⊖ **Moderate**[f,i]

Explanations

[a] Serious risk of bias due to allocation concealment and lack of blinding.

[b] Serious inconsistency (moderate heterogeneity I^2 > 50% and point estimates vary considerably), that may be explained by training status.

[c] Serious unexplained inconsistency (point estimates vary considerably).

[d] Very serious unexplained inconsistency (large point estimates in each direction).

[e] Serious imprecision (95% CI crosses one equivalence bound).

[f] Very serious imprecision (95% crosses both equivalence bounds)

[g] Extremely serious imprecision (95% CI crosses both equivalence bounds with large effects in both directions).

[h] Plausible residual confounder of training status.

[i] Presence of large effects.

CI confidence interval, *PI* prediction interval, *SD* standard deviation, *CMJ* countermovement jump, *k* number of studies, *n* number of participants

Table 3 Results from moderator analyses

	Hedge's *g* [90% CI]	***p*-value**	**95% CI**
Strength			
Training status	-0.42 [-1.37, 0.53]	0.42	-1.62, 0.78
Exercise type	0.55 [-0.40, 1.50]	0.31	-0.65, 1.75
Volume matching	0.17 [-0.88, 1.24]	0.76	-1.16, 1.52
Study design	-0.11 [-1.20, 0.99]	0.80	-1.73, 1.52
Hypertrophy			
Training status	-0.33 [-2.17, 1.51]	0.66	-2.98, 2.32
Muscle group	-0.36 [-1.53, 0.81]	0.49	-2.01, 1.29
Study design	-0.17 [-1.31, 0.98]	0.75	-1.74, 1.41

CI confidence interval

4. Discussion

The aim of this systematic review was to quantitatively explore the effects of manipulating eccentric tempo duration on indices of muscular performance and hypertrophy. The main results from the meta-analysis indicated that faster eccentric tempo durations, with moderate certainty of evidence, result in large and practically worthwhile increases in CMJ height following RT. For strength and hypertrophy, results were generally too uncertain to statistically support or reject the presence or absence of a practically worthwhile impact of eccentric tempo manipulation, highlighting the need for further research. Notably, when participants are trained or when volume-load is matched, slower eccentric tempo may, with moderate certainty of evidence, lead to practically similar or higher strength gains compared to faster eccentrics.

Our results indicate that faster eccentric phase durations lead to larger improvements in CMJ across 4-6 weeks of squat training, suggesting that prolonging the eccentric phase duration, at least in the short-term, may not maximise ballistic performance. This might be a result of the reduced residual force enhancement from the longer eccentric phase [16,58], possibly attenuating the stretch-shortening cycle (SSC) specific training adaptations associated with faster eccentrics. Interestingly, while Shibata and colleagues [53] did report larger CMJ effects after faster eccentric phase durations (1.5 cm [4.2%] vs. 3.1 cm [8.4%]), they also found larger effects for squat jump height (i.e., without an SSC) after slower eccentric phase durations (2.4 cm [7.2%] vs. 1.9 cm [5.3%]). While these results were not statistically different, others also found larger effects in non-SSC performance tests by reducing residual force enhancement (i.e., by imposing a pause between the eccentric and concentric phase during squat training [59]). Therefore, modifying residual force enhancement via tempo alteration to more precisely improve specific strength and conditioning outcomes may be viable. However, more research exploring tempo manipulation on SSC and non-SSC performance is needed.

For maximal strength, longer eccentric phase durations led to practically similar or larger improvements in than faster eccentric phase durations in trained participants and when volume-load was matched. It should be recognised that decisions regarding the ROPE are context-dependent [60], and we chose these values as they were likely representative of typical study participants. However, it could be argued that given the low real-world cost of tempo manipulation, any measurable effect may be relevant, particularly in highly trained athletes. For example, by converting our trained subgroup 90% CI into percentages ($\Delta M_{\%} = \Delta SD_{\%} \times ES$), using the only study that employed free-weight back squats in trained participants [54], we see that prolonging the eccentric enhances strength gains by an additional ~0.4-3.0% over 4-12 weeks compared to doing faster eccentrics. We therefore urge readers to interpret results based on what they would deem relevant in their practice.

Some explanations for why slower eccentrics may enhance strength gains may be that they i) lead to enhanced neural adaptations, ii) produce similar or larger training stimuli, and/or iii) incur less fatigue. First, a recent systematic review found tempo-controlled RT may enhance neural adaptation and task-specific motor learning, compared to self-paced RT [61], likely due to increased cognitive effort needed to meet the prescribed tempo. Given we only found equal or higher strength gains for trained participants, it may be that self-paced or faster eccentric durations provide sufficient motor learning for less trained populations; however, as skill increases, additional movement challenge is needed to progress motor skill, as suggested elsewhere [62]. However, the interaction between exercise execution/selection and training status is relatively unexplored.

Second, by prolonging the eccentric phase, total and eccentric time under tension increase [53,55,57], which typically leads to lower absolute loads or less overall repetitions completed when either repetitions or loads are kept constant, respectively, at the same proximity-to-failure [14,16,53]. Conversely, when repetitions per set at a submaximal load are fixed, slower eccentric tempos lead to closer proximities to failure [56], suggesting that prolonging the eccentric phase makes each repetition more strenuous. This might explain why Shibata and colleagues [53] observed large effects favouring fast eccentrics (-1.40), relative to the other squat studies in trained participants (0.37-0.57) [52,54] and the remaining studies in untrained (-0.09-1.27) [51,55,57]. These participants performed 3 sets to failure at 75% of 1RM, which, on average, had participants squatting for 42-78 sec per set, ultimately causing a large drop in completed repetitions in the SEG across six weeks. The other squat study participants only performed 3-6 repetitions per set at 70-85% of 1RM [52,54], did not reach failure when performing multi-joint exercises at higher repetitions [51] or used isolation exercises when reaching muscular failure [55,57], possibly allowing participants to perform each set with less cardiovascular strain and, thus, not interfering with the training stimulus. This might also explain why the limited data on very long eccentric tempo durations exhibit no signs of a dose-response gradient (ES: 4s = 1.68, vs. 6s = 1.38 [52]; 4s = 1.69, vs. 10s = 1.27 [63]), possibly due to performance declining as eccentric tempo durations increase [15,16]. As such, slower eccentrics may allow trainees to experience similar training stimuli with lower volume-loads or improve the stimulus with the same volume-load (see subgroup analyses for matching method), if eccentric duration is not taken to extremes.

Third, some studies [52,64], but not all [54], report that prolonging eccentric tempo, despite leading to acute performance decrements, may result in less neuromuscular fatigue and delayed onset muscle soreness after training, suggesting slower eccentric training allows trainees to reach similar or higher training stimuli with lower or similar volume-loads at a lower fatigue cost. Therefore, eccentric tempo manipulation may enhance strength gains in trained participants due a combination of factors possibly related to increased motor learning, and improved training stimulus with lower fatigue. However, successfully implementing eccentric tempo manipulation for maximal strength likely depends on striking a balance between eccentric phase duration and volume and load to minimise unnecessary performance decrements by limiting excessive muscular and/or cardiovascular fatigue. Future research should investigate the impact of how training parameters are matched and how this impacts the effect of eccentric tempo manipulation.

For muscle hypertrophy, results were too uncertain to infer the absence or presence of practically worthwhile effects. While point estimates of our subgroup analyses generally followed the trends from our strength results, implying that a possible reason for improved strength could be greater rates of muscle gain in trained participants, these results were very uncertain, and more research is needed. No studies explored muscle architecture, and two [53,56] that explored regional hypertrophy found diverging results. As such, research exploring the effects of eccentric tempo manipulation on muscle morphology is warranted. Ultimately, while including an eccentric phase is important for maximising muscle hypertrophy [11], whether its duration affects muscle hypertrophy by a practically worthwhile degree cannot be currently determined.

This systematic review with meta-analysis has several limitations. First, the included number of studies and participants per study was low. This was accounted for in our effect size calculations and robust variance estimations, leading to wide confidence intervals, which, combined with our interpretative lens, lead to more cautious conclusions based on the uncertainty of results. Second, some subgroup analyses were not pre-planned and should therefore be considered exploratory. Given the point estimates we found, we urge researchers to conduct studies exploring the impact of training status, how volume was matched between groups, impact of relative-load and exercise selection on the effect of eccentric tempo manipulation. Lastly, given the different adaptations from slow(er) and fast(er) eccentric tempos, it is unclear whether a combined approach would be more suitable to concurrently enhance multiple measures of performance, or possibly achieve a synergistic effect.

5. Conclusion

In conclusion, with moderate certainty, faster eccentrics are better for improving CMJ height, while relatively longer eccentric phase durations produce similar or larger strength gains in trained participants and under volume-load matched conditions. Due to very low certainty of evidence, the absence or presence of whether a practically worthwhile effect of eccentric tempo manipulation for muscle hypertrophy cannot be confidently inferred. More research is needed to explore the impact of eccentric tempo duration on SSC vs. non-SSC ballistic performance outcomes, maximal strength, and muscle morphology. Future research should aim to improve the certainty of inferences and examine the interactions with volume matching, training status, relative load, concurrent approaches and responses across different muscle groups and exercises.

6. Practical applications

Coaches and athletes may manipulate eccentric phase duration depending on their goal. Faster eccentric tempos are likely better suited for improving ballistic SSC performance, while slower eccentric tempos may be used to achieve similar or greater increases in non-SSC ballistic performance and maximal strength, in trained participants. The dose, exercise selection, athlete needs, and competition schedule should be considered, such that specific tempos allow for maximal development of the desired outcome, without hindering specific practice during important parts of the season. Regardless of goal, prolonging eccentric phases can serve as a load and fatigue management tool by allowing trainees to i) increase exertion levels with the same repetitions and load, ii) obtain a similar training stimulus with less absolute volume-load completed, and iii) reduce overall recovery demands from training. Therefore, worthy of specific note, is that in cases where discomfort or pain is associated with higher load training, slower eccentric durations may be a useful method to maintain strength adaptations with lower loads. Based on practical experience and limited evidence, we recommend: i) the eccentric phase should not be prolonged excessively (i.e., 3-5 seconds seems reasonable), ii) using low to moderate repetitions (1-6 repetitions) when employing slower eccentrics for multi-joint exercises, and iii) if using slower eccentric durations for higher repetitions to or near muscular failure, consider doing so with isolation exercises. This is to avoid excessive cardiovascular and metabolic fatigue that may impair progress and decrease the lifters' willingness to perform the tempo as prescribed.

Contributions

Both authors contributed to study conception, design, and literature search. Data extraction was performed by CHA. Data analysis and visualisation was performed by AK. The first draft was written by CHA and both authors provided revisions. Both authors read and approved the final manuscript.

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Data and Supplementary Material Accessibility

All supplementary materials, including statistical code and dataset, can be found at Open Science Framework (https://osf.io/s6uqn/).

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