Critical Commentary on the Stimulus for Muscle Hypertrophy in Experienced Trainees

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Abstract
Researchers have expressed concern recently for standardization of resistance training protocols so that valid comparisons of different training variables such as muscular fatigue, time under tension, pre-exhaust exercise and exercise order, pyramid and drop sets, amount of resistance (load), range of repetitions, frequency and volume of exercise, interset rest intervals, etc. can be more closely studied and compared. This Critical Commentary addresses some recent review articles and training studies specifically focused on the stimulus for muscle hypertrophy in participants with several years of resistance training experience. It reveals that many of the recommended resistance training protocols have their foundation in some long-held, self-described bias.

Blinding of assessors and statisticians, self-plagiarism, authorship responsibility, and conflicts of interest are briefly discussed as well. The conclusion is that most of the published peer-reviewed resistance training literature failed to provide any compelling evidence that the manipulation of any one or combination of the aforementioned variables can significantly affect the degree of muscle hypertrophy, especially in well-trained participants. Although the specific stimulus for optimal gains in muscle mass is unknown, many authors are desperately clinging to their unsupported belief that a greater volume of exercise will produce superior muscle hypertrophy.

Hypertrophic Stimulus

Dankel and colleagues (1) made several valid points regarding the potential problems when researchers report resistance training volume and load in studies examining muscle hypertrophy outcomes. Their opinion is that there should be a standardized protocol of performing sets of resistance exercise to what they called *volitional fatigue*, which potentially could increase the homogeneity of the exercise stimulus. They stated that if volitional fatigue is not mandated, training studies cannot be truly replicated. On the other hand, they noted also that there is very little evidence to suggest that a maximal effort is required at the termination of a set of repetitions for an optimal hypertrophic response and referenced a training study by Ogasawara and colleagues (2). It should be recognized that the corresponding author of the article by Dankel and colleagues was also an author of the referenced study by Ogasawara and colleagues.

Ogasawara and colleagues (2013)

Nine previously untrained young adult males participated in two 6-week resistance training programs separated by 12 months; one with a higher load (75% 1RM) and one with a lower load (30% 1RM) in the free weight bench press 3x/week (2). The researchers determined the cross-sectional area (CSA) of the triceps brachii and pectoralis major with magnetic resonance imaging (MRI) but did not indicate if the assessors were blinded to the training protocol (heavier or lighter loads). The trainees performed a specific number of repetitions (10-12 reps) while training with the heavier resistance but they performed reps with the lighter resistance until *volitional fatigue*, which they did not define. Ogasawara and colleagues did not clarify their set end-point with the heavier resistance. They noted also that during a set of repetitions with the heavier resistance, the rep duration increased from 1s/1s to 2s/2s, but they did not mention any change in rep duration when the trainees used the lighter resistance. Nevertheless, both training programs produced a significant increase in triceps (11.9 and 9.8%) and pectoralis CSA (17.6 and 21.1%), heavier and lighter loads, respectively, with no significant difference in CSA between the two training programs.

Although Ogasawara and colleagues (2) reported no significant difference between training protocols in hypertrophic responses (which supported their original hypothesis), they stated that there were significant differences in several potential confounding variables such as training volume, relative volume, and the number of sets and reps. However, there were also potential differences between training protocols for rep duration and set end-points. Steele and colleagues (3) recently discussed the history and ambiguity of set endpoints and how that may have created confounding issues in the interpretation of resistance training research. They noted that a set end-point for 10-12 reps (e.g., 10-12RM) has been described as a point where trainees complete the last repetition believing (but not actually knowing) that the next repetition would result in muscular failure. The topic of heavier versus lighter resistance as the optimal stimulus for muscle hypertrophy is discussed in another section of this Critical Commentary.

Considering the strong genetic influence on the heterogeneity of hypertrophic responses to resistance training (4), all these resistance training variables may have very little influence on the hypertrophic outcome and this was clearly supported in the aforementioned study by Ogasawara and colleagues (2). According to Dankel and colleagues (1) however, control of these variables could enhance the interindividual homogeneity of the hypertrophic stimulus in a research setting; that is, when comparing one tightly controlled training protocol with a different tightly controlled study (e.g., 1set/exercise versus multiple sets/exercise).

Termination of Sets

Dankel and colleagues (1) noted that if sets are not performed to *volitional fatigue*, studies cannot be replicated accurately because the hypertrophic stimuli may differ based on the individual physiology of the specific population. Therefore, they recommended applying a common stimulus (volitional fatigue) among trainees and among different studies to minimize unintentional variations in fatigue. However, neither Dankel and colleagues nor Ogasawara and colleagues (2) actually defined *volitional fatigue*. Steele and colleagues (3) specifically defined the term *momentary failure* as the inability to perform another concentric repetition without altering exercise form or repetition duration and causes an *involuntary* set endpoint, which they noted may differ from an RM set (e.g., 5RM, 10RM, etc).

Contrary to those definitions, the term *volitional fatigue* implies a conscious, deliberate, voluntary decision to terminate a set. However, individuals possess relatively different tolerances of psychological/physiological discomfort and consequently variable interindividual perceptions of *volitional fatigue*. Although these terms have different definitions, they are used interchangeably in the resistance training literature (3). For example, Fleck and Kraemer (5) claimed that an *exhaustion set* was synonymous with *volitional fatigue, sets to failure, sets to concentric failure, and repetition maximum* (5, p. 196). However, the actual meaning of each of these terms may result in large interindividual variability of set end-points and perhaps intra-individual variability among exercises (e.g., arm curls versus squats). In order to establish a standardization of the hypertrophic stimuli, perhaps set end-points such as *volitional fatigue* or *muscular failure* should be clearly defined and universally accepted.

Perhaps other terms such as *momentary failure* or *muscular failure* should be considered. It was used previously in another article (6) by the corresponding author of the article by Dankel and colleagues (1). Steele and colleagues (3) suggested that muscular failure could be defined as the trainee's inability to complete the final repetition despite a maximal effort. In addition, if researchers tried to determine if different rep durations (e.g., 1s/1s versus 4s/4s) elicited significantly different hypertrophic responses, many of the trainees would experience the greatest effort (and perhaps the hypertrophic stimulus) at or toward the end of the set where the rep duration may predictably be greater and similar because of fatigue. This may consequently negate any differences in assigned specified rep durations at the beginning of each set/exercise. Perhaps researchers should designate the set end-point (e.g., muscular failure) as the inability to maintain the assigned rep duration for concentric and eccentric muscle actions throughout the set while maintaining proper exercise form.

***Hypertrophic Stimulus and Termination of Sets Sections Summary*:** Set end-points have not been clearly defined or explained, which precludes an accurate interpretation of results among studies or among groups within a specific study.

Drop-Set Training

Drop-set training (a.k.a. descending sets) is a training protocol where the trainee performs a set of repetitions with a given amount of resistance to concentric muscular failure, immediately reduces the resistance ~10-25%, and performs as many repetitions as possible with the lighter load. Sometimes two or more drop-sets are performed. There are no specific guidelines for how much to reduce the resistance or how many drop sets should be used.

Schoenfeld and Grgic (7) noted recently that muscles are not completely fatigued at concentric muscular failure, and the muscles are still capable of producing additional repetitions if the resistance is reduced. They cited and discussed the methods and results of five training studies (8-12) that compared the efficacy of a drop-set training protocol with different traditional resistance training protocols. None of those studies reported a significant difference in training induced muscle hypertrophy between drop-set training and traditional training.

Goto and colleagues (2004)

Schoenfeld and Grgic (7) specifically described the study by Goto and colleagues (8) in an attempt to show some advantage to drop-set training. The statement by Schoenfeld and Grgic that the subjects were recreationally trained was misleading. Goto and colleagues specifically noted in their Methods section that their subjects had some resistance training in the last four years but had not participated in any regular resistance training for at least six months prior to the study. Therefore, Goto and colleagues considered all the subjects previously untrained. All their previously untrained young adult male subjects performed 10-15RM leg press and knee extension exercises 2x/week at 80-40% 1RM (10-15RM) for the 1st 6 weeks, which Goto and colleagues described as the *hypertrophy phase*. During the subsequent 4 weeks (their so-called *strength phase*), they performed 5 sets of each exercise with 90% 1RM (3-5RM). Half the group executed a 6th set of repetitions 30s after the 5th set with 50% 1RM (25-35RM). This protocol reduced the resistance from 90% 1RM to ~50% 1RM but is not considered a typical drop-set protocol (~10-25% reduction with no rest between sets). All the sets were performed until the subjects were unable to continue the movement. After 6 weeks of training, both groups significantly increased muscle cross-sectional area of the thighs ~4% as determined by magnetic resonance imaging. There was no significant difference in muscle mass between groups.

Schoenfeld and Grgic (7) claimed also that the drop-set group increased thigh muscularity by another 2% during the next 4 weeks. However, Goto and colleagues (8) reported that neither group showed a significant change in muscle hypertrophy during the last 4 weeks ($p = 0.08$). As previously noted, none of the studies that Schoenfeld and Grgic cited in their article supported the efficacy of drop-set resistance training. Nevertheless, they claimed that drop-set training is an effective way to increase training volume and that there is a well established dose-response relationship between training volume and muscle hypertrophy. The only reference they cited was a meta-analysis by Schoenfeld and colleagues (13), which is discussed in the Volume section of this Critical Commentary.

Despite the lack of evidence to support its efficacy, Schoenfeld and Grgic (7) constructed a table of drop-set recommendations for specific training variables such as the load (resistance), rest intervals (between the last set and the drop-sets), training volume (number of drop-sets), tempo (rep duration), exercise selection (single or multiple joint exercises), and frequency (how many times per week to use drop-sets). However, each recommendation is extremely vague and they did not cite any training studies to support any of those recommendations—perhaps because there is none.

Schoenfeld and Grgic (7) recommended decreasing the drop-set by approximately 20-25%. A greater level of fatigue may be referred to as a greater inroad into a trainee's starting strength. However, an alternative to drop sets would be to simply reduce the initial resistance by a given amount. For example, if the 1RM for a specific exercise is 100 kg, and that individual is using 85% 1RM for training (85 kg), at the point of concentric failure the trainee is no longer capable of generating enough force to complete the movement with 85 kg. This could be considered a 15% inroad into the trainee's starting strength level. By reducing the initial resistance by 20-25% (17-21 kg) and reaching concentric failure with 64-68 kg instead of applying a drop-set, the inroad into starting strength (level of fatigue and metabolic stress) would be 32-36%. That would be more than double the inroad into starting strength compared with the drop-set, and still require a sufficient amount of mechanical stress. However, it is not known what level of fatigue or inroad is required to elicit an optimal hypertrophic response and if it exists it may inherently possess significant interindividual and intra-individual heterogeneity. To stay within a specifically desired range of repetitions with the lighter resistance, trainees could increase the concentric time under tension (TUT) to induce muscular failure in a range of repetitions that would be similar to using the heavier resistance. TUT is discussed in the next section.

It should be noted that in an earlier article by Schoenfeld (14), he cited only the previously described training study by Goto and colleagues (8). He claimed that the addition of the drop set to the standard protocol resulted in a significant increase in thigh muscle cross-sectional area compared with the standard protocol. That was a misleading statement—at best. Schoenfeld claimed also that the drop-set protocols can stimulate greater muscular growth by increasing greater motor unit fatigue. He referenced an often cited brief review by Willardson (15). However, Willardson's article focused on whether or not training to failure was necessary to achieve the greatest strength gains, not muscle hypertrophy. In his one brief paragraph regarding drop-sets, Willardson cited the aforementioned study by Goto and colleagues (8) and incorrectly claimed that the study demonstrated that drop-sets provided a superior stimulus for muscle hypertrophy.

It is also worth mentioning that Willardson (15) cited a study by Drinkwater and colleagues (16) 17 times in his brief article as evidence to support performing sets to failure. However, Drinkwater and colleagues compared strength gains after training to failure (4 sets of 6 reps) or not to failure (8 sets of 3 reps), both with 85-105% 6RM (equal training volume), 2x/week for 6 weeks, in teenage male resistance trained elite junior athletes. Drinkwater and colleagues did not measure or report muscle hypertrophy and in fact specifically noted in their Discussion section that they speculated that the strength increases were probably related to neural adaptations.

Schoenfeld and Grgic (7) suggested that drop-sets may induce a greater level of fatigue and metabolic stress, and therefore enhance anabolism. Indeed, Schoenfeld has written extensively on the role of metabolic stress, and along with mechanical stress, its supposedly potential effect on muscle hypertrophy (17). Although many have written about the importance of metabolic stress to stimulate muscular hypertrophy (e.g., 18), other researchers from the same group have suggested that metabolites produced during resistance training do not have specific anabolic properties. However, they may indirectly induce muscle fatigue, which in turn augments motor unit activation (19).

***Drop-Set Section Summary*:** The significant implication from this section is that there is no evidence to suggest that drop-set training is superior to traditional resistance training for stimulating muscle hypertrophy.

Time under Tension (TUT)

Steele and colleagues (3) have suggested that repetition duration should be controlled. However, even when the total rep duration is controlled, the concentric, eccentric and perhaps isometric muscle action time under tension (TUT) may differ significantly.

Lacerda and colleagues, (2019)

For example, Lacerda and colleagues (20) compared acute responses to three different 6s rep durations but with specific concentric to eccentric muscle action ratios (2s:4s, 3s:3s, and 4s:2s) while performing the bench press for 3 sets of 6 reps with 60% 1RM. The pectoralis major activation was significantly greater with the 4s:2s protocol compared with the other two protocols throughout the repetitions and sets. The triceps brachii activation was significantly greater with the 3s:3s and 4s:2s protocols compared with 2s:4s.

Lacerda and colleagues (20) concluded that although all the protocols consisted of a 6s rep duration, the longer concentric TUT required the subjects to spend a longer time at a higher level of EMG activation during each repetition, and thereby placed a greater physiological demand compared with the shorter concentric TUT. They speculated that the greater TUT concentric muscle action may be a more appropriate strategy to enhance muscle activation and induce neuromuscular fatigue. Although the three protocols were all 6s duration, the stimulus for hypertrophy may vary significantly. Loenneke and colleagues (6) have hypothesized previously that metabolic stress (e.g., increased blood lactate) may play a significant role in resistance training adaptations and as long as the metabolic stress is sufficient to recruit the larger motor units, muscle hypertrophy is independent of the external load. The concept of load (amount of resistance) is discussed in the Load section of this Critical Commentary.

Goto and colleagues (2009)

In another study, Goto and colleagues (21) compared four concentric:eccentric TUT protocols: 5s:1s, 1s:5s, 3s:3s at 50% 1RM, and 1s:1s at 80% 1RM. The participants performed four sets of bilateral knee extension exercise to *exhaustion* for each protocol. The authors did not define exhaustion. When they compared the two 6s protocols with antithetical con:ecc TUT (5s:1s versus 1s:5s), the 5s:1s TUT resulted in a significantly higher blood lactate concentration and significantly fewer average number of repetitions than the 1s:5s protocol. Dankel and colleagues (19) have noted previously that increased metabolites (e.g., blood lactate) indirectly promote muscular hypertrophy by inducing muscular fatigue, which augments muscle activation through the size principle. Dankel and colleagues noted also that because of the strong association of metabolites with the level of muscular fatigue, reaching or approaching *volitional fatigue* appears to be the most important factor related to muscular hypertrophy. However, they did not define *volitional fatigue,* nor did the authors of the two studies, who Dankel and colleagues cited in their article, define their set end points (22-23).

Gillies and colleagues (2006)

In furtherance of the concept of different concentric:eccentric ratios of TUT, Gillies and colleagues (24) reported the results of a training study that compared 6s:2s and 2s:6s TUT protocols. The resistance-trained adult female participants performed 2 sets of 6-8RM for 4 lower body exercises 3x/week for 9 weeks. The researchers performed biopsies of the vastus lateralis but they did not indicate if the technician or assessor was blinded to the training protocol. Both groups significantly increased vastus lateralis cross-sectional area (CSA) of type I muscle fibers, with no significant difference between groups. However, only the 6s:2s group significantly increased type IIA CSA (~26%). Gillies and colleagues speculated that because both groups trained with the same workload relative to their concentric strength using the same repetition duration (8s), the metabolic demand (although not assessed) for the concentric muscle actions was probably greater with the 6s:2s TUT protocol compared with the 2s:6s TUT. They concluded that putting emphasis on the eccentric muscle action (6s) limited muscular hypertrophy only to type I muscle fibers and placing emphasis on the longer concentric TUT (6s) significantly increased type I and type IIA CSA. Perhaps the specific muscle action TUT (concentric and eccentric) should be a controlled variable as well as total rep duration.

Hollander and colleagues (2007)

Hollander and colleagues (25) compared maximal eccentric and concentric strength on a gravity dependent Universal-type weight stack machine. They calculated the ratio of eccentric:concentric 1RM strength in 10 resistance trained adult females and 10 resistance trained adult males for 3 upper body and 3 lower body exercises. The eccentric and concentric 1RMs were performed with a controlled repetition duration of 3 seconds each and failure was designated as the inability to maintain proper form and rep duration. Females had a higher eccentric to concentric ratio than males in 2 out of 3 upper body and 2 out of 3 lower body exercises, which ranged from 1.56:1 to 2.87:1 compared with the males who ranged from 1.30:1 to 1.51:1. Hollander and colleagues did not speculate on the reason for the greater eccentric strength compared with concentric strength in females for all 6 exercises.

Some authors have speculated on different proposed internal mechanisms such as differences in contractile mechanism function, internal muscular friction, or external friction when weight stack or plate loaded machines are used. Perhaps the overriding factor is that during a concentric muscle action, the agonist muscles must generate enough force to overcome gravitational force (the force generated on the resistance mass plus the force necessary to accelerate the mass against gravity) versus an eccentric muscle action where only enough force is needed to resist the force of gravity (gravitational force plus just enough force to keep the mass from falling too rapidly). The main point is that the level of effort may differ significantly depending of the velocity of concentric and eccentric movement for each muscle group in females and males.

In their review on the effects of different repetition durations on muscle hypertrophy, Schoenfeld and colleagues (26) reported the results of eight studies in their meta-analysis. All the male and female adult participants were previously untrained and five of those studies used what the authors described as a direct measure of muscle hypertrophy (ultrasound, MRI or muscle biopsy). The authors of all eight studies reported no significant difference in the changes in muscle hypertrophy (for any of the muscle groups assessed) between any of the different rep duration groups, which ranged from 0.5s to 8s. Schoenfeld and colleagues did note that a limitation of their review was that the inclusive studies did not address the separate concentric and eccentric phases of muscle actions.

Schoenfeld and colleagues (26) discussed specifically the methodology and results of a study by Schuenke and colleagues (27) who compared shorter (1-2s) and longer (10s concentric and 4s eccentric) muscle actions and claimed at least twice that changes in the mean muscle fiber cross-sectional was markedly greater in the shorter repetition duration group. This was apparently the basis for the claim by

Schoenfeld and colleagues that those results suggested longer rep durations are inferior for hypertrophic adaptations. However contrary to the claim by Schoenfeld and colleagues, Schuenke and colleagues reported that their changes in mean muscle cross-sectional area, which they assessed with muscle biopsies, were not significantly different between the shorter and longer rep duration groups.

Schoenfeld summerized the results of the aforementioned review (26) on his personal website (https://www.lookgreatnaked.com/blog/how-fast-should-you-lift-to-maximize-muscle-growth/) and claimed that *superslow* lifting, which he defined as durations greater than 10s, is inferior for enhancing muscle hypertrophy. It is not clear if he was referring to a 10s concentric rep or total TUT (concentric + eccentric muscle actions). Schuenke and colleagues (27) used a 10s concentric:4s eccentric protocol in their longer duration group. Hutchins, who was a staunch advocate of super slow training, had specifically designated 10s concentric and 5s eccentric durations for the *Super Slow* protocol (28). Nevertheless, Schoenfeld cited the previously discussed study by Schuenke and colleagues (27) and a study by Tanimoto and colleagues (29) in an attempt to support his claim that longer repetition durations are inferior to shorter rep durations for stimulating muscle hypertrophy.

Tanimoto and colleagues (2006)

The study by Tanimoto and colleagues (29) had at least a couple of potential confounding variables in their training groups: the 3s rep group used ~55-60% 1RM resistance with no relaxing phase versus ~80-90% 1RM resistance with a 1s relaxing phase in the 1s group. Schoenfeld (on the aforementioned website) stated that the changes in muscle hypertrophy were not significantly different between groups but that Tanimoto and colleagues reported what they defined as a *substantially* greater effect size as a result of the shorter duration compared with the longer duration. The mean increase in muscle thickness for the seven muscle groups, which were assessed with ultrasound, was 0.21 mm in the longer duration group and 0.27 mm in the shorter duration group (Table 3, p. 1934). Does anyone, except perhaps Schoenfeld and colleagues (26), really believe that a difference of 0.06 mm between groups was *substantially* different or that anyone can accurately estimate muscle thickness with ultrasound to the hundredth of a millimeter. Even if that really was the actual difference in muscle thickness, does it have any *substantial* practical significance for anyone?

Tanimoto and colleagues (29) also assessed lean soft tissue mass (LSTM) with DXA and reported no significant difference between groups for the arms, thighs or whole body LSTM. Tanimoto and colleagues referenced a previous study from their department (30) that followed a similar training protocol but was localized to the knee extensors. There was no significant difference between groups for the changes in the cross-sectional area of the knee extensors, which they assessed with MRI. The researchers did not indicate if the assessors of muscle hypertrophy were blinded to the training protocols in either study (29, 30).

Garg (2009)

Schoenfeld (31) had previously claimed that a longer eccentric rep duration is necessary to maximize the hypertrophic response and cited a study by Garg (32) who trained previously untrained young adult males and females 3x/week for 4 weeks. One group performed longer duration eccentric-only muscle actions (~10s/rep) and another group used a shorter eccentric-only duration (~1s/rep) throughout 90^{o} range of motion of knee extension exercise. The participants performed 6 sets of 5 eccentric reps with 100% of the concentric 1RM. They did not have to perform any concentric muscle actions because the resistance was lifted to the horizontal position by the investigator. There was no significant increase in thigh circumference in either training group. Consequently, the claims by Schoenfeld in both of his articles (14, 31) regarding training recommendations to maximize muscle hypertrophy are without any scientific support—then and now.

It is worth mentioning that in his website summary Schoenfeld denoted rep duration in seconds, as it was in the title of their review and in the studies cited by Schoenfeld and colleagues (26). He interchangeably used the words *cadence, tempo, slower*, and *faster* in the same paragraph. However, those words are not comparable when describing the duration of a repetition. He commented also that the concentric duration is a moot point when lifting heavy loads because although the trainees may attempt to move the resistance quickly, the actual concentric movement will be slow; but with a lighter load, depending on the magnitude of the load, various volitional durations are possible. One of the six eligibility criteria in their meta-analysis was that all the participants performed each set to *muscular failure*, which they defined as the inability to complete another repetition in good form. As trainees approach the point of muscular failure in a set of repetitions, most trainees will be moving slowly as they struggle to complete their last repetition of the set—regardless of the assigned rep duration or the amount of resistance.

***Time under Tension Section Summary*:** The significant implication of this section is that there is no resistance training study to support the superiority or inferiority of any specific TUT for stimulating muscle hypertrophy.

Pre-Exhaust Training (Sequence of Exercise)

Pre-exhaust exercise is a method of resistance training where two exercises are performed in sequence with minimal rest between the exercises. It is based on the opinion that there is a weak link between the larger torso muscles and the alleged weaker limb muscles, and that pre-exhausting the larger muscles will minimize the limiting effect of the supposedly weaker smaller muscles. This sequence of pre-exhaust exercises should allegedly force the larger muscles to work beyond the capacity of the weaker link.

A couple of hypothetical examples are that the biceps would be the weak link between the latisimus dorsi muscles and the resistance used in a lat pull-down exercise, and the triceps are the weak link between the deltoid muscles and the resistance used for overhead pressing exercises, as well as the weak link between the pectoral muscles and the resistance used for bench press exercises. Jones (33) claimed that the best results could only be achieved by INSTANTLY (Jones used upper case letters for that word several times in his chapter on pre-exhaust exercise) or within 2-3s transition between the pre-exhaust exercise and the secondary exercise.

In fact, he designed and manufactured several compound machines that allowed two specific exercises to be executed—one immediately after the other—on the same machine. For example, the Nautilus Double Shoulder machine provides resistance for the lateral raise exercise, immediately (INSTANTLY) followed by an overhead press. Detailed descriptions of several attempts to perform pre-exhaust exercise and a lengthy discussion of the acute and chronic effects those studies have been published previously (34-35).

In a recent review of pre-exhaustion exercise, Ribeiro and colleagues (36) reported the acute neuromuscular responses, performance, training volume, and chronic effects of what they mistakenly claimed to be pre-exhaustion resistance exercise. None of the 10 studies in their Neuromuscular Activity section actually performed pre-exhaustion exercise, nor did any of those studies report exercise volume. They briefly mentioned 3 studies (37-39) and discussed one study (40) in their Performance and Training Volume section; however, none of those studies incorporated pre-exhaustion exercise. Ribeiro and colleagues (37) and de Faria and colleagues (38) reported the volume of exercise for a so-called *tri-set training method* (no transition time reported and 5-8s time between exercises, respectively), while Vilaca-Alves and colleagues (39) had a transition time of 90s.

Ribeiro and colleagues (36) discussed the details of the study by de Salles and colleagues (40) who reported the number of repetitions during four sets of knee extension (KE) and leg press (LP) exercises. Thirteen resistance trained young males performed KE or LP either before or after the other with 20s rest between exercises at different sessions with an 8RM load. When they executed the KE before the LP, they completed a significantly greater average number of KE repetitions compared with performing KE as a secondary exercise (6.7 versus 3.6 reps). These results would be expected when KE is performed before the LP. Their own data demonstrated that their KE exercise and the 20s transition to the LP was not very effective in actually pre-exhausting the quadriceps because there was no significant difference in the number of LP reps between the two sequences of exercise. However, even those people who still believe that a greater volume of exercise results in greater muscle hypertrophy should have trouble believing that an additional 3 reps per set would result in any measureable clinically significant difference in muscle hypertrophy.

It should be recognized that one hypothetical function of pre-exhaust exercise is to actually reduce the volume of exercise in the secondary exercise (33); that is, if the same RM is attempted immediately following the pre-exhaust exercise, fewer repetitions would be expected (lower volume); or if a specific range of repetitions is desired, than the resistance must be reduced (lower volume).

Fisher and colleagues (2014)

Ribeiro and colleagues (36) cited and discussed only one longitudinal pre-exhaust training study (41). Fisher and colleagues (41) reported the chronic effect of training three groups of middle-aged previously trained females and males. They performed either pre-exhaustion (≤5s transition) or two other more traditional training protocols for eight upper and lower body exercises 2x/week for 12 weeks. There was no significant change in body composition for any group. However, as noted by Ribeiro and colleagues, Fisher and colleagues used air displacement plethysmography (Bod Pod) to estimate overall body composition and did not measure the primary targeted muscles such as the pectoralis or latissimus dorsi with muscle biopsies, magnetic resonance imaging, or ultrasound.

In the last paragraph of their Longitudinal Studies section, Ribeiro and colleagues (36) claimed that conflicting results limited their ability to draw inferences on the effect of pre-exhaustion on muscle hypertrophy. However, the data are not conflicting; they cited only one pre-exhaust training study (41) and it reported no significant difference in any outcome. In other words, there was no evidence for support.

In the Practical Application section of their article, Ribeiro and colleagues (36) correctly noted in one sentence that they could not draw conclusions regarding the effects of pre-exhaustion on muscle hypertrophy. However, in the next sentence they concluded antithetically that pre-exhaustion may be a viable training strategy for muscle hypertrophy. They desperately attempted to justify their recommendation by claiming that pre-exhaustion exercise may help increase training volume.

As a sidebar to the aforementioned review by Ribeiro and colleagues (36), they referenced their source of pre-exhaustion exercise as a book entitled Ultimate Bodybuilding: Principles of Training and Nutrition by Weider and Reynolds (42). The actual book title is Joe Weider's Ultimate Bodybuilding: The Master Blaster's Principles of Training and Nutrition. Because Weider and Reynolds referred to pre-exhaustion as the *Weider Pre-Exhaustion Training Principle* several times, the implication in the book is that The Master Blaster (Weider) claimed credit for pre-exhaustion exercise. However, Jones (33) wrote about pre-exhaustion almost 20 years prior to the Master Blaster's claim. To his credit, Jones humbly noted in the first sentence of his chapter on pre-exhaustion that it should be clearly understood that this subject was neither new nor original and had been previously described several times in print.

Ribeiro and colleagues (36) cited only one study (41) that came close to the pre-exhaust training protocol and they reported no significant difference in body composition outcomes. In fact, considering the pre-exhaustion hypothesis as originally described by Jones (33), and because the Nautilus machines that provided the instantaneous transitions from the pre-exhaust exercise to a secondary exercise are scarce, no one has actually tested Jones' pre-exhaustion hypothesis for its effect on hypertrophy of specific muscle groups. Perhaps researchers should refrain from declaring whether pre-exhaust exercise is effective or not until they understand exactly what it is and then test their hypothesis for its effectiveness on muscle hypertrophy.

Most recently, Nunes and colleagues (43) reported a systematic review and meta-analysis of 11 resistance training studies (only 3 studies recruited previously trained subjects with at least 6 months resistance training experience) where they compared different orders of exercise (single-joint followed by multiple- joint exercises, and multiple-joint followed by single-joint exercises). There was no significant effect of exercise order on muscle hypertrophy for either site specific assessments (ES = -0.02), which the authors listed as MRI, CT, ultrasound, and muscle biopsy, or indirect assessments (ES = 0.06), which they defined as DEXA, hydrostatic weighing, bioimpedance, air displacement plethysmography, and skinfold measures, or the combination of site specific and indirect measures (ES = 0.03). Nunes and colleagues concluded that there was a similar increase in muscle hypertrophy regardless of the order of exercise. It should be recognized that none of the three studies cited with previously trained participants used direct measures of muscle hypertrophy.

***Pre-Exhaust Training Section Summary*:** There is no evidence to suggest that pre-exhaust training—a half-century old untested hypothesis—is superior to traditional resistance training or that manipulating the order of exercise will influence the degree of muscular hypertrophy.

Load (Amount of Resistance)

Schoenfeld (44) reviewed nine studies that compared the hypertrophic responses after training with a lighter or heavier amount of resistance performed with different ranges of repetitions. Various researchers used CT scans, ultrasound imaging, MRI, muscle biopsies, or anthropometric estimates to assess muscle hypertrophy. All the inclusive studies recruited previously untrained participants. Only three (27, 45-46) of the nine studies reported significantly greater muscle hypertrophy with a heavier versus a lighter load. These three studies are briefly discussed next.

Campos and colleagues (2002)

Campos and colleagues (45) randomly assigned 32 previously untrained young adult males to 1 of 3 resistance training protocols, which they designated as high load (4 x 3-5RM), intermediate load (3 x 9-11RM) and low load (2 x 20-28RM), or a non-exercising control group. The subjects performed leg press, squat and knee extension exercises to *muscular failure* 3x/week for 8 weeks. Muscle biopsies of the vastus lateralis showed a significant increase in all three fiber types in the intermediate (~19%) and high load (~19%) groups but the increase was not statistically significant in the low load group (~11%). There was no significant difference in the hypertrophic response between the intermediate and high load groups.

Schoenfeld (44) summarized the results of Campos and colleagues (45) by stating only that the high load group significantly increased muscle cross-sectional area and there was no significant increase in the low load group. Schoenfeld did not mention the intermediate load group and therefore his conclusion was misleading at best. In addition, neither he nor Campos and colleagues discussed the potential confounding variables such as the different numbers of sets and rest between sets among the three training groups, which the self-proclaimed experts have previously claimed over decades to significantly affect chronic outcomes such as muscle hypertrophy. Campos and colleagues did not control for rep duration and did not indicate if the assessors for the muscle biopsies were blinded to the training protocol. None of the previously untrained young adult males in any group showed significant changes in anthropometric-estimated lean body mass or percent fat.

As a sidebar unrelated to muscle hypertrophy, the study by Campos and colleagues (45) has been cited ad infinitum for almost two decades as the go-to study when attempting to support the so-called *strength-endurance continuum* and the results have never been challenged. The heavier load group (3-5RM) significantly increased the leg press 1RM by 61%. However, the authors reported a significant decrease in the number of repetitions (-20%) with 60% 1RM in that group. This decrease in muscular endurance is curiously inexplicable in a group that increased their strength for that exercise by 61%. And if the post-training resistance for muscular endurance was adjusted to 60% of the new post-training increased 1RM, Campos and colleagues failed to report that adjustment.

As previously noted, the study by Campos and colleagues (45) has been cited ad infinitum throughout the years in an attempt to show that the heavier resistance group (3-5RM) produced significant muscle hypertrophy, while the lighter resistance (20-28RM) showed no significant hypertrophy. However, there was a follow-up study by Leger and colleagues (47) that used the exact heavier and lighter protocol as Campos and colleagues in previously untrained but physically active middle-age males: 4 sets of 3-5RM with 3-minute interset rest or 2 sets of 20-28RM with 1-minute interset rest on the same 3 exercises (leg press, squat and knee extension) in the same order, 2x/week for the 1st 4 weeks and 3x/week for the final 4 weeks. They used CT to estimate quadriceps CSA, whereas Campos and colleagues performed muscle biopsies. The authors did not indicate if the assessors were blinded to the training protocol. CT results showed that quadriceps CSA significantly increased ~10% in both groups and there was no significant difference between the lighter and heavier groups in muscle hypertrophy.

Readers may question why numerous authors have cited the study by Campos and colleagues (45) to support their opinion regarding the use of a heavier resistance for hypertrophy but very rarely cite the study by Leger and colleagues (47) that reported no significant difference between groups after following a training protocol that was almost identical to Campos and colleagues. Schoenfeld included the study by Leger and colleagues in his first review of resistance load and muscle hypertrophy (44); however, it is curiously missing from his next two reviews (48-49) on the same topic.

Schoenfeld (44) used the word *intensity* throughout his review article to represent the percent of 1RM but he subsequently incorrectly described (Table 1, p.1283) the same training protocols for Campos and colleagues (45), Leger and colleagues (47) and Lamon and colleagues (50) as 3-5RM *low intensity* and 20-28RM as *high intensity*. The reviewers and editors of *Sports Medicine* obviously accepted Schoenfeld's definition of *intensity*; therefore, 3-5RM should have been described as high intensity and 20-28RM as low intensity.

The frequency of citing the study by Campos and colleagues (45) is analogous to citing the now infamous study by Berger (51) that went unchallenged for 40 years (52) primarily because Berger's study supported most opinions that 3 sets were superior to a single set for strength gains.

Holm and colleagues (2008)

Holm and colleagues (46) randomly assigned contralateral limbs in 11 previously untrained young adult male subjects to perform seated knee extension exercise 3x/week for 12 weeks. One limb executed 10 sets of 8 repetitions with 70% 1RM (~25s/set), which was alternated with 10 sets of 36 reps at 15.5% 1RM (~180s/set) through ~70^{o} range of motion. It should be recognized that the researchers did not state if the 8 reps with 70% 1RM required a significant level of effort. The knee extensions with the contralateral limb (15.5% 1RM) only completed one rep every 5s, which the authors noted were not exhaustive sets. As determined with MRI, the quadriceps cross-sectional area showed a similar and significant increase in the distal and proximal quadriceps of both limbs. The only significant difference between limbs was in the middle thigh location where the heavier load elicited a significantly greater response than the lighter resistance (7.6 versus 2.6%). Holm and colleagues did not indicate if the radiographic assessors were blinded to the training protocol. Schoenfeld (44) noted correctly that because the level of fatigue was questionable in the low load limb (as it was in the higher load limb as well), it obscured his ability to draw conclusions from the study by Holm and colleagues.

Schuenke and colleagues (2012)

Schuenke and colleagues (27) randomy assigned 34 previously untrained young adult females to 1 of 4 groups: traditional (6-10RM with ~80-85% 1RM), super slow (6-10RM with ~40-60% 1RM), endurance (20-30RM with 40-60% 1RM), or a control group. All the trainees performed 3 sets each of leg press, squat and knee extension exercises until *failure* ~3x/week for 6 weeks. Estimated fat-free mass did not change significantly in any group. Muscle biopsies of the vastus lateralis showed a significant increase in muscle cross-sectional area for the three training groups, with no significant difference in hypertrophy among the groups. Schoenfeld (44) noted in his text that the increase in hypertrophy for the super slow group was less than half the increase in the traditional group but failed to note that the difference was not statistically significant. In his Table 1 (p. 1283), Schoenfeld concluded that there was a significant increase in cross-sectional area in the traditional group (~80-85% 1RM) but no significant increase in the 40-60% group; another misleading statement—at best. In fact, Schuenke and colleagues reported a significant increase ($p = 0.026$) in mean muscle fiber cross-sectional area for the super slow group.

Based on the results reported in these three aforementioned studies (27, 45-46) with previously untrained subjects, Schoenfeld (44) commented that it was questionable if lower load training would have any effect in well-trained subjects. However, he presented no evidence to support one training load over another in experienced trainees.

Schoenfeld and colleagues (2014)

Schoenfeld and colleagues (53) recruited 20 young adult males who were currently training ~3x/week for approximately 4 years (range: 1-10 years). They were randomly assigned to one of two groups: a hypertrophy group (H) who performed 3 sets of 8-12 RM for each of 9 upper and lower body exercises, or a strength group (S) who performed 7 sets of 2-4RM 3x/week for 8 weeks. The H group performed 3 exercises per session that focused on specific muscle groups (split routine) and the S group executed 3 exercises per session for all the muscle groups (total body routine). There was no non-exercising control group. All the exercises in both groups were completed to the point of *muscular failure*. However, their description of rep duration was vague: the concentric phase was executed quickly and the eccentric phase was lowered under control. They defined *muscular failur*e as the inability to perform another concentric rep in proper form but their vagueness raises the question of what constitutes maintaining proper form. Was the group who used the lighter load moving more quickly than the heavier load group? And were both groups moving at similar rep durations on the last rep of each set?

Nevertheless, pre- and post-training ultrasound imaging of biceps brachii thickness revealed almost identical significant increases for the H (12.6%) and S (12.7%) groups, with no significant difference between groups (53). Schoenfeld and colleagues assessed and reported on only the one muscle group. They did not indicate if the ultrasound technician was blinded to the specific training protocols and did not mention the lack of blinding in their list of several study limitations. It is curious why most researchers in the field of resistance training do not consider a lack of blinding their assessors to be a major limitation to a study.

It is worth mentioning that Schoenfeld and colleagues (53) claimed that there was a *clear* dose-response association between multiple set protocols and muscle hypertrophy, but the only reference they cited was a review by Krieger (54), which is discussed in the Volume Section of this Critical Commentary.

When discussing muscle hypertrophy, Schoenfeld and colleagues (53) believed that there may be additional improvements with up to 8 sets of each exercise and they cited a study by Marshall and colleagues (55). However, Marshall and colleagues did not measure, report or discuss muscle hypertrophy. Schoenfeld and colleagues claimed also that it was well established that muscular adaptations in highly trained individuals respond differently from previously untrained subjects and they cited a study by Peterson and colleagues (56) who reported only on strength gains, not muscle hypertrophy. The two studies cited by Schoenfeld and colleagues not only failed to support their opinion regarding the relationship of training volume and muscle hypertrophy, but those studies (55-56) did not even assess or report on muscle hypertrophy.

Schoenfeld and colleagues (2015)

Schoenfeld and colleagues (57) recruited 24 young adult males who had an average of 3.4 years resistance training experience to perform 3 sets for each of 7 exercises: bench press, military press, pull-down, cable row, squat, leg press and knee extension. They randomly assigned them to train with a low load (LL) 30-50% 1RM (25-35 reps/set) or high load (HL) 70-80% 1RM (8-12 reps/set) 3x/week for 8 weeks. The sets were completed until the trainee failed to perform another concentric repetition, which they designated as *muscle failure*. Ultrasound imaging revealed a pre- to post-training increase in muscle thickness of the elbow flexors (5.3 and 8.8%), elbow extensors (6.0 and 5.2%), and the quadriceps femoris (9.3 and 9.5%), HL and LL groups, respectively. There was no significant difference between groups for the hypertrophic response in any of the muscle groups.

Schoenfeld and colleagues (57) reported that the LL group performed ~3 times the volume of training (sets x reps) compared with the HL group and they hypothesized that the greater volume in the LL group was responsible for the similar hypertrophic response. Their rationale was based on what they claimed to be *compelling* evidence for the relationship between muscle hypertrophy and resistance training volume. They cited only the meta-analysis by Krieger (54), which is critically challenged in the Volume section of this Critical

Commentary and strongly suggests that Krieger's evidence was weak and far from *compelling*. Because Schoenfeld and colleagues presented no compelling evidence, an alternative hypothesis could be that the volume of exercise had nothing to do with the similar results and that as long as the exercises were carried out to *muscle failure* (maximal effort), one set of each exercise (instead of 3) in the LL group would have resulted in similar muscle hypertrophy. In addition, Schoenfeld and colleagues specifically indicated that their ultrasound technician was not blinded to the group assignments but they obviously did not consider that to be one of the several limitations noted in their study.

Schoenfeld and colleagues (2016)
Schoenfeld and colleagues (58) randomly assigned 26 young experienced (~5 years) adult male lifters to either a heavy (2-4RM) or moderate (8-12RM) resistance training protocol 3x/week for 8 weeks. The subjects performed 3 sets for each of 7 upper and lower body muscle groups to the point of *concentric muscular failure*. Ultrasound imaging revealed a significant increase in muscle thickness for the elbow flexors, with no significant difference between the moderate and heavy load groups. Elbow extensors significantly increased only in the moderate group. Lateral thigh muscles significantly increased in both groups and the increase in the moderate group was significantly greater than the heavy group. Schoenfeld and colleagues did not indicate if the ultrasound technician was blinded to the training protocols. However, they concluded that moderate load training (8-12RM) was superior to a heavier load (2-4RM) for muscle hypertrophy. Interestingly, in this study by Schoenfeld and colleagues (58) they designated 8-12RM as a *moderate* load and in the previously discussed study (57) Schoenfeld and colleagues classified 8-12RM as a *high* load.

Schoenfeld and colleagues (2016)
Schoenfeld and colleagues (59) performed a meta-analysis of 8 resistance training studies that assessed upper- and lower-body muscle hypertrophy with MRI, CT, ultrasound, or muscle biopsy. All their inclusive studies involved previously untrained subjects and they performed each set of every exercise until they were unable to complete another concentric repetition in proper form. Six out of the 8 studies reported no significant difference in effect sizes for muscle hypertrophy as a result of training with what they described as a low load (≤ 60% 1RM) or high load (≥ 65% 1RM). The two studies that reported a significantly greater increase were the previously discussed studies by Schuenke and colleagues (27) and Campos and colleagues (45).

Schoenfeld and colleagues (59) claimed several times that there was a *trend* for the effect size to be greater in the high load groups ($p = 0.076$). Their claim raises this question: what if $p = 0.024$ (the same distance from 0.05 in the opposite direction), should that p value be interpreted to mean that there is a *trend* for the results to *not* be statistically significant? Their claim that a trend was noted for the superiority of heavier loading actually was strong evidence to suggest that there was a trend for researchers with a strong, deeply held opinion about a topic, to desperately claim anything in an attempt to justify it. Other commonly used excuses may include claims that there were not enough subjects in a study or not enough studies in a meta-analysis, or that researchers employed different assessments for hypertrophy, or simply, that there were *trends*. Schoenfeld and colleagues claimed that heavier loads (compared with moderate loads) may be required for those who train on a regular basis. However, they did not cite any reference to support that claim, and most importantly, their own meta-analysis did not support it.

Schoenfeld and colleagues (2017)
In the most recent review and meta-analysis of of what they described as low load (≤60% 1RM and high load (>60% 1RM) resistance training, Schoenfeld and colleagues (60) reported on 21 inclusive studies. All the studies required the completion of each set to *momentary muscular failure*. Only 3 of those studies (57, 61-62) recruited previously trained subjects. However, none of them reported any significant difference in LBM, CSA or muscle thickness (Bodpod, DEXA and ultrasound assessments, respectively) as a result of training with low load or high load resistance. The effect size was 0.42 for low load and 0.53 for high load training, with no significant difference in those effect sizes ($p = 0.10$) between high and low load training. Schoenfeld and colleagues claimed also that the difference in effect size was within their designated level of $p \leq 0.10$ and therefore was a *trend* that suggested a likely probability in favor of heavy load training; however, they failed to cite any training studies to support that probability. As previously noted in this Critical Commentary, it appears that when one statistical analysis does not result in their desired outcome, the authors desperately resort to claiming a *trend* in favor of their opinion.

It is certainly worth noting that in his 2013 review (17) Schoenfeld claimed that Schuenke and colleagues (27) reported a significant increase in CSA for the high load group and no significant increase in the low load group (Table 1, p. 1283). However, Schuenke and colleagues actually reported a significant increase in all three fiber types (Type I, IIA and IIX) in the high load group and for 2 out of 3 fiber types (Type IIA and IIX) in the low load group (Table 3, p. 3591), with no significant difference between groups. They also reported that there was no significant change in body composition for any group, which they estimated with skinfold measurements. Schoenfeld did not report that none of the groups increased fat free mass post training. In the more recent review, Schoenfeld and colleagues (60) reported no significant difference between groups for the estimates in lean body mass but did not report the results of the muscle biopsy.

In that review, Schoenfeld and colleagues (60) listed also a study by Rana and colleagues (63) which they noted was the same study and research group as Schuenke and colleagues (27). Rana and colleagues noted that they employed air displacement plethysmography (Bod Pod) to estimate body volume, which they then used to estimate body density and body composition. They and Schoenfeld and colleagues (60) reported a significant increase in fat free mass in all the groups with no significant difference among the groups (Table 1, p. 121). Rana and colleagues stated in their Discussion that all the groups (including the control group) had a significant increase in lean body mass (p. 125), and then antithetically concluded that none of the groups demonstrated any change in body composition (p. 126). Neither Rana and colleagues, Schuenke and colleagues, nor Schoenfeld and colleagues gave any indication that the assessors were blinded to the specific training protocols for any estimate of muscle hypertrophy or body composition. The authors failed to mention the different methods for estimating body composition in both of their studies, or why the same study was published in two different journals four years apart.

Schoenfeld and Contreras (2014)
In another article, Schoenfeld and Contreras (64) wrote about the so-called *muscle pump*, presented a hypothetical (not theoretical) schematic effect of the muscle pump on chronic adaptations such as muscle hypertrophy (Figure, p. 22), and suggested different training manipulations such as drop-sets to maximize the pump. They claimed that a study by Goto and colleagues (65) showed that drop-set training resulted in a significant increase in muscle cross-sectional area compared with a traditional high intensity training protocol. However, the study they cited by Goto and colleagues (65) did not compare drop-set training with any type of training. In fact, previously untrained young adult males in the intervention groups performed 3 sets of 10RM for each of two upper body exercises and 5 sets of 10RM for bilateral knee extension exercise 2x/week for 12 weeks. The subjects in a no-rest group performed the set continuously without any rest between repetitions (NR group) and the other group rested for 30s between the 5th and 6th repetition (WR group). They assessed (not blinded) thigh muscle cross-sectional area with MRI. Both groups significantly increased CSA, and not surprisingly, the NR group showed a significantly greater increase compared with WR and the control groups. Contrary to the claim by Schoenfeld and Contreras (64), Goto and colleagues (65) did not use any form of drop-set training.

As a sidebar to the aforementioned article by Schoenfeld and Contreras (64), they also made antithetical statements in their Practical Applications section. They stated that there was a lack of resistance training studies directly assessing the effects of the muscle pump on muscle hypertrophy, which was followed directly with the claim that research provided *compelling* reasons to believe that the exercise-induced muscle pump enhances muscle hypertrophy. They failed to cite any evidence—much less compelling evidence.

Contreras and Schoenfeld made similar claims on the t-nation website (www.t-nation.com/training/6-lessons-learned-from-the-master-blaster) regarding the Master Blaster's (Joe Weider) Flushing Principle (the pump) (42). They rated the *flushing principle* #4 out of their six favorite Master Blaster training principles that were listed by Contreras and Schoenfeld. If hypertrophy is the primary goal, they suggested that after performing the heavy compound movements in a session, trainees should choose exercises that flush as much blood into the muscle as possible. Contreas and Schoenfeld failed to cite any evidence for their recommendation.

On an internet video entitled *5 things we can learn from Arnold about building muscle* (https://www.t-nation.com/training/5-things-we-can-learn-fromarnold-about-buildingmuscle), Contreras and Schoenfeld claimed that Arnold (Schwarzenegger) was a big proponent of the pump and that the pump was not just cosmetic. They noted that performing multiple sets with moderate to high numbers of repetitions would engorge muscle with blood and that this pump would contribute to muscle growth. They acknowledged correctly that many bodybuilding tactics are considered *broscience* (see Note below) and that no study has shown that pump-oriented training can affect any chronic adaptation such as muscle hypertrophy. They followed that statement with the antithetical claim that implied evidence gives them reasons to believe that the pump may have a positive effect on muscle hypertrophy. Their unsupported claim is an excellent example of what Contreras and Schoenfeld ironically termed *broscience*. Carpinelli noted almost 20 years ago that there was no evidence to suggest that the pump, which is mostly water—not blood, enhanced any long term adaptations such as muscle hypertrophy (66) and his statement is still valid.

Note: Although *broscience* is not a real word, it refers to anecdotal and usually clueless opinions about how to train for optimal physiological adaptations such as muscle hypertrophy. *Broscience* is not always wrong but usually is not substantiated with resistance training studies. Proponents of *broscience* (*bros*) believe that their opinions are more credible than the scientific research. The author of this Critical Commentary intentionally omited any of his opinions about resistance training, with the exception of the 2nd to last sentence in his Disclosure section.

In their Conclusion section, Schoenfeld and Contreras (64) claimed that heavy loads maximize motor unit activation, heavy multi-joint exercises should be the foundation of a hypertrophy training program, and that trainees should dedicate a component of their training (exercises with higher reps and shorter rest periods) to achieving the pump, which would provide an optimal hypertrophic stimulus. They failed to cite any evidence to support those recommendations.

Klemp and colleagues (2016)
Klemp and colleagues (67) randomly assigned 21 resistance trained young adult males in a so-called daily undulating periodization program (squat and bench press exercises only) to either a lighter load (4 x 12 @ 60% 1RM, 4 x 10 @ 65% 1RM and 5 x 8 @ 70% 1RM) or heavier load group (8 x 6 @ 75% 1RM, 9 x 4 @ 80% 1RM and 10 x 2 @ 85% 1RM) on Monday, Wednesday and Friday, respectively, for 8 weeks. Although they employed progressive training, none of the sets were carried out to the point of failure. Relative, absolute and total training volume was equated between the two groups. They used ultrasound imaging to assess muscle thickness of the chest and thigh, but did not indicate if the assessor was blinded to the training protocol. Chest muscle thickness significantly increased in both groups, with no significant difference between groups; lateral and anterior quadriceps significantly increased in both groups (except for mid thigh in the lighter load group), with no significant difference between groups. Klemp and colleagues concluded that both the lighter and heavier load groups similarly increased muscle hypertrophy, and the lighter load group was much more time efficient (~93-129 min/session) compared with the heavier load group (~185-257 min/session).

Morton and colleagues (2016)
Morton and colleagues (62) randomly assigned 56 resistance trained young adult males to a heavier load (3 x 8-12RM, ~75-90% 1RM) or lighter load group (3 x 20-25RM, 30-50% 1RM). The subjects performed total body progressive resistance training (5 exercises/session) 4 days/week (2 different total body sessions 2x/week) for 12 weeks. They completed each set of all the exercises to *volitional failure*.

The researchers used DEXA to assess fat- and bone-free mass, biopsies for muscle fiber CSA, and blinded the assessors to the specific training protocols. The researchers reported a significant increase in Type I and II muscle CSA, with no significant difference between groups. There was a significant increase in total lean body mass and limb mass, with no significant difference between groups for any of those outcomes.

Morton and colleagues (62) noted that lifting a lighter load required a greater volume of exercise (more reps x load) to reach volitional failure. They speculated that if one group uses 80% 1RM and another group uses 40% 1RM, at the point of volitional failure the low load group would have made a 60% inroad into their force generating capacity while the heavier load group only a 20% inroad. It should be recognized that the level of inroad required for an optimal hypertrophic stimulus is unknown and it may be considerably different among individuals or within an individual for different muscle groups. The apparently different inroads in their groups did not affect hypertrophic outcomes. Morton and colleagues concluded that either lower or higher load progressive resistance training carried out to volitional failure results in comparable hypertrophic adaptations.

Morton and colleagues (62) noted that the American College of Sports Medicine position stand on resistance training (68) recommended 70-100% 1RM for enhancing muscle hypertrophy in advanced trainees, and the authors of that position stand (Ratamess and colleagues) cited three references (69, 70-71). Two of those studies recruited previously untrained participants and the third reference (69) was a database of 5 *experiments* that Kraemer resurrected 15 years prior to publication. Kraemer did not report any measure of muscle hypertrophy in any of his five *experiments*. Consequently, none of those cited studies supported any specific load for enhancing muscle hypertrophy in advanced trainees. The ACSM made the same claim in their 2002 position stand (72) and they failed again to cite any references for support. Dedicated trainees and coaches may still be waiting patiently for an apology and retraction of the misinformation presented in the ACSM's 2002 (72) and 2009 (68) position stands.

One of the problems that has not been addressed by any of the researchers is the ambiguity of what constitutes a low load and high load in resistance training. For example, Schoenfeld and colleagues were not consistent when they classified an 8-12RM. In one study (Schoenfeld and colleagues, (64) they designated 8-12RM as a low load; in another study (57) they called 8-12RM a high load; and still another study (58) they classified 8-12RM as moderate load. In addition, they classified 8-12RM as a low load group in one study (53) and 20-35RM as a low load group in another study (57). When comparing different amounts of resistance, perhaps researchers could simply classify the loads as *heavier* or *lighter* loads and then specify those numbers in their studies and reviews.

Schoenfeld and colleagues (2016)

Schoenfeld and colleagues (73) randomly assigned 19 young adult males, who had been resistance training for ~4.7 years, to either a CONSTANT group (8-10RM 3x/week) or a VARIED group (2-4RM, 8-12RM and 20-30RM at sessions 1, 2 and 3, respectively). Both groups performed 3 sets for each of 7 upper and lower body exercises to the point of *momentary concentric muscular failure* for 8 weeks. They did not identify the ultrasound technician or indicate if that person was blinded to the training protocol, and they did not believe it was important enough to mention in their list of study limitations. Ultrasound imaging revealed a significant pre- to post-training increase in elbow flexor, elbow extensor and knee extensor muscle thickness. Schoenfeld and colleagues concluded that there was no significant difference between groups for the elbow flexors ($p = 0.33$), elbow extensors ($p = 0.22$) or knee extensors ($p = 0.74$) and that both the CONSTANT and VARIED training strategies were equally effective for increasing muscle hypertrophy in this group of experienced trainees.

Nevertheless, Schoenfeld and colleagues (73) claimed that there was a *possible* benefit in favor of the VARIED group for the elbow flexors (the difference between groups was only 0.6 mm), and that there was a *likely* benefit in favor of the VARIED group for elbow extensor muscle thickness (the difference between groups was only 1.1 mm). The authors incorrectly labeled muscle thickness in cm rather than mm in their Table 2 (p. 444). All the authors, as well as the reviewers and the editor, are responsible for errors such as those.

Readers may find it difficult to believe that the ultrasound technician was actually capable of detecting those tiny insignificant differences (0.6 and 1.1 mm), and more importantly, accept the authors' claim that one protocol may have *possibly* or *likely* favored the other (73). The source of these so-called *qualitative probabilistic terms* was a note on inferences in an article by Hopkins and colleagues (74), and the only reference that Hopkins and colleagues cited was a spreadsheet by Hopkins (75) on mechanistic and clinical inferences.

Schoenfeld and colleagues (73), who are advocates of higher volume training, did not attempt to speculate why the significantly greater ($p = 0.02$) volume load (load x reps x sets) for the two upper body so-called *pushing* movements (free weight bench press and military press) in the CONSTANT group did not produce a significantly greater hypertrophic response for the elbow extensors in that group. Their only comment was that the VARIED group produced a comparable hypertrophic response with a significantly lower volume load.

Schoenfeld and colleagues (73) claimed that all the sessions were directly supervised by the research team but two of the six authors were from Bronx, New York, and the other four from Issaquah, WA; Aukland, New Zealand; Fullerton, CA; and Hamilton, Canada, respectively. Schoenfeld and colleagues did not state where the study took place or the contribution to the study by each author. This topic is addressed again in the Author Contribution section of this Critical Commentary.

Load Section Summary: Many reviewers and editors obviously did not check if the references cited actually support the authors' claims and opinions. Despite the opinions from the heavier-is better advocates over the last several decades, there is no credible compelling evidence to support the claim that a heavier resistance is more effective than a moderate or lighter resistance for enhancing muscle hypertrophy.

Exercise Volume

The volume of exercise is usually calculated as the product of a number of variables: the amount of resistance (load) x number of repetitions x number of sets per session or week, month, etc. Based on a previous meta-analysis by Schoenfeld and colleagues (76) and another by one of their co-authors (54), Schoenfeld and colleagues (77) conducted a training study with experienced trainees. They hypothesized that there would be a graded increase in muscle hypertrophy with low, moderate and high volume (more sets/exercise) resistance training.

Schoenfeld and colleagues (2019)

Schoenfeld and colleagues (77) randomly assigned young adult males with an average 4.4 years of resistance training experience (~3x/week) to follow a 1, 3 or 5 sets per exercise protocol 3x/week for 8 weeks. The participants performed four upper body and three lower body exercises for 8-12 repetitions to *momentary concentric failure*, which they defined as the inability to perform another concentric repetition in good form. The researchers assessed muscle thickness with ultrasound pre- and post-training in 4 major muscle groups (elbow flexors and extensors, mid- and lateral thigh) and used an analysis of covariance to test their null-hypothesis. The only statistically significant difference in muscle hypertrophy was between the 1-set and 5-set groups for 3 out of the 4 muscle groups. There was no significant difference between 1-set and 3-set groups or between the 3-set and 5-set groups for any of the four assessed muscle groups. Their statement that only the triceps did not show statistically greater increases among the 1-, 3- and 5-set protocols was misleading because there was no significant increase in triceps thickness in the 1-, 3- or 5-set groups. Nevertheless, Schoenfeld and colleagues claimed that their results showed increasingly greater muscle hypertrophy with higher training volume; that is, a dose-response relationship.

Schoenfeld and colleagues (77) also performed a Bayesian factor analysis (BF_{10}), which is comparative in nature; that is, the Bayes factor is the ratio of the null hypothesis to some alternative hypothesis (78). It estimates the likelihood of probability that the evidence for one hypothesis is superior to the evidence for another hypothesis (79). Schoenfeld and colleagues did not give any explanation or rationale for why they used a Bayesian statistical procedure in this study. They interpreted the BF_{10} as *no evidence, weak, positive, strong, or very strong decisive evidence*.

Their results ranged from *no evidence* to *positive* for most of their comparisons (77). The BF_{10} rating for 3 sets compared with 1 set, and 5 sets versus 3 sets were all less than 3 and were categorized as *weak and barely worth mentioning*. Elbow flexors and mid-thigh (rectus femoris) muscle thickness were *positive* for 5 sets compared with 1 set. The only *strong* difference reported was for the comparison of 5 sets versus 1 set of exercise on lateral thigh (vastus lateralis) thickness; that is, only two of the four assessed muscle groups showed a *strong* difference and with only one comparison (1 vs. 5 sets). It is important to recognize that none of the ratings even approached a *very strong and decisive* level and therefore questions whether the results support their claim for a dose-response relationship between greater training volume and muscle hypertrophy. It appears that when the first statistical analysis failed to support their claim for a dose-response relationship, the researchers tried a more esoteric statistical analysis.

Schoenfeld and colleagues (77) reported large variability (standard deviations) in all the assessed muscle groups. Perhaps genetics (interindividual heterogeneity) was a much greater influence on the hypertrophy outcomes than the volume of exercise (4). Schoenfeld and colleagues noted also that it was not clear if the average increase in muscle thickness had any significant impact on aesthetic appearance.

Schoenfeld and colleagues (77) noted that the rep duration was 1s concentric and 2s eccentric for all the exercises but did not state if or how the duration was controlled or if failure to maintain that duration, either shorter or longer, constituted the inability to maintain proper form. It is not known if some subjects increased rep duration on the final repetitions (e.g., struggled with a 3s concentric rep) or decreased rep duration (e.g., <1s concentric rep duration) to generate more momentum. They did not state if either of these deviations from the prescribed rep duration constituted not maintaining proper form. Either of these scenarios during the final reps would not be considered a common stimulus between groups for muscle hypertrophy—as discussed in the previously mentioned article by Dankel and colleagues (1).

Schoenfeld and colleagues (77) stated that all the trainees were performing multi-set routines prior to entering the study. One could infer that they performed multiple sets of each exercise because they believed that multiple sets were required for an optimal hypertrophic response and perhaps the study restrictions for the 1-set and 3-set groups affected their training motivation and hence their level of effort for some exercises. They also pointed out that although they instructed the participants not to perform any additional resistance training during the study, the researchers were not sure if the participants complied with those restrictions.

Schoenfeld and colleagues (77) noted that most previous research on the effects of different volumes of exercise (sets/muscle group) recruited previously untrained subjects. Only one other study (80) used site specific measures of muscle hypertrophy (ultrasound) on previously resistance trained (1-4 years) young adult males as a result of low, moderate or high volume exercise (1, 2 or 4 sets per muscle group) 4x/week for 10 weeks. There was a significant increase in cross-sectional area of the rectus femoris and triceps muscles in the groups combined, with no significant difference among the 1, 2 or 4-set groups. Ostrowski and colleagues (80) noted that ultrasound assessments of muscle thickness are inherently subjective, but did not give any indication that their ultrasound operator was blinded during the assessments. They concluded that the low volume program resulted in increased muscle size and function similar to programs with two times or four times the volume.

The major concern with the study by Schoenfeld and colleagues (77) is that the lead author performed all the ultrasound assessments for muscle hypertrophy. To their credit, the authors noted this in their narrative but they did not acknowledge it in their list of study limitations. Simply revealing a potential conflict of interest without any attempt to eliminate it, does not necessarily prevent it from being a questionable practice (81).

Although failure to blind participants and research personnel is categorized as *high risk of bias* in the Cochrane Handbook for Systematic Reviews of Interventions (82), it is understood that it may not be possible to blind participants or instructors to whether they are performing 1, 3 or 5 sets of each exercise. However, the Cochrane Handbook has mandated that if those persons who determine outcome measures such as a change in muscle thickness are aware of specific intervention assignments, bias may be introduced into those assessments (82, Section 8.12.1). Smart and colleagues (83) also noted that although it may be difficult to blind trainers and trainees, it is a reasonable expectation that the assessors will be blinded to the assigned training protocols.

When the people who are measuring the outcome of a study are blinded, readers can be more confident that the reported results were not influenced by any assessor bias. The risk of bias in the outcome assessment is dependent on the degree of subjectivity (e.g., highly subjective ultrasound interpretation). With any mode of assessment for muscle hypertrophy (ultrasound, CT, MRI, and muscle biopsy), there is always error because of the assessor, biological variability, and the interpretation of those measures (84). In studies with highly subjective outcomes (e.g., ultrasound estimates of changes in muscle thickness), observer bias should be suspected (85). Kahan and colleagues (86) highly recommended that assessors be blinded to the treatment allocation because failure to blind the assessors can result in biased estimates of the treatment effect.

Schoenfeld and colleagues (77) stated that in addition to performing the ultrasound imaging, the lead author also obtained muscle thickness dimensions by measuring the distance from the adipose-muscle tissue interface to the muscle-bone interface. Consequently, their outcomes were exposed to the potential for confirmation bias during both the ultrasound procedure and the dimensional measuring. They noted that a 12-week resistance training study by Franchi and colleagues (87) reported a high correlation between changes in ultrasound measured muscle thickness and muscle cross-sectional area assessed with MRI. However, the changes in muscle thickness were less than 2 mm with standard deviations that were twice as large as the mean, which were similar to the study by Schoenfeld and colleagues.

If the lead author (Schoenfeld) had recused himself from the assessments (77) and had an independent ultrasound technician assess the outcomes, it could have minimized any question of assessor bias. Perhaps blinding outcome assessors to the allocated interventions—rarely noted in the resistance training literature—should be mandatory. In addition, it should also be recognized that Schoenfeld and colleagues did not indicate if their statistician (Krieger) was blinded to the interventions, which may have introduced another opportunity for bias.

Readers do not have to infer a bias toward a higher volume of training for muscle hypertrophy because statements by Schoenfeld and colleagues are ample evidence that the lead author and at least one of his co-authors have a bias favoring higher volume training for muscle hypertrophy (77). In the previously mentioned meta-analysis (13), one of the co-authors (Krieger) was the statistician for the meta-analysis as well as for this current training study (77). In the meta-analysis (13), Schoenfeld and colleagues compared 15 training studies that measured muscle hypertrophy as a result of performing different numbers of weekly sets per muscle group. The authors claimed that each additional set corresponded with an additional gain in muscle hypertrophy. That statement was misleading because each additional set each week elicited an additional gain of only 0.37% and that difference was not statistically significant ($p = 0.074$).

The meta-analysis (13) had only two studies that recruited previously trained subjects; the previously discussed study by Ostrowski and colleagues (80) and a study by Rhea and colleagues (88). Rhea and colleagues randomly assigned 16 young adult males with at least two years of resistance training experience to perform either 1 set or 3 sets of bench press and leg press exercises 3x/week for 12 weeks. They did not measure site-specific muscle hypertrophy but indirectly estimated body composition with whole-body plethysmography (BodPod). In addition, they assessed chest and mid-thigh circumference with a tape measure. Rhea and colleagues concluded that neither the 1-set nor 3-set groups showed a significant change in lean body mass or circumference measures.

Arruda and colleagues (89) wrote a Letter to the Editor challenging the reliability of the meta-analysis by Schoenfeld and colleagues (13). One of their strong criticisms was that resistance training outcomes can be influenced by many different variables that interact with each other. Consequently, any attempt to estimate the impact of one specific variable such as the number of sets per muscle group when all other variables are not tightly controlled, has a high risk of bias. Arruda and colleagues believed that the conclusion drawn by Schoenfeld and colleagues was contaminated by all the methodological differences in the studies included in the meta-analysis. Schoenfeld and colleagues (90) responded with a point by point rebuttal to all the criticisms and specifically argued that other variables were controlled in each inclusive study. Readers can decide which group of researchers made the stronger arguments in support of their claims.

As related to the question of bias and as a prelude to their training study (77), the title of the response by Schoenfeld and colleagues (90) was revealing: *The dose-response relationship between resistance training volume and muscle hypertrophy: are there really still any doubts?* Based on the paucity of training studies with previously resistance trained subjects, the answer is that there is really still some doubt—or perhaps should be—in the minds of those without any bias.

After Schoenfeld and colleagues (77) acknowledged more than two dozen research assistants, they apparently believed that it was unusually necessary to declare that their results were reported clearly, honestly and without fabrication, falsification, or inappropriate data manipulation. Readers would expect those qualities to be self-evident in all peer-reviewed scientific publications and therefore raises the question of why the authors believed that a detailed declaration was required for this study.

Barbalho and colleagues (2019)

In two separate but similar 24-week studies in young adult females (91) with at least 3 years of uninterrupted resistance training experience (~3.4 years) and in young adult males (92) with similar training experience (~5.4 years), Barbalho and colleagues randomly allocated their participants (counterbalanced with baseline muscle thickness measurements) to perform 5, 10, 15 or 20 sets of resistance exercise per muscle group per week (G5, G10, G15 and G20, respectively). They trained 3x/week but used a split routine to train each muscle group (3 exercises per muscle group) 1x/week. A nonlinear periodization protocol varied the repetitions from 4-6RM to 12-15RM, with all sets completed to *momentary failure*. Muscle thickness was measured in the biceps brachii, triceps brachii, pectoralis major, quadriceps femoris and gluteus maximus. The ultrasound technician was not involved in the training and was blinded to group allocation in both studies.

The 4 groups of females (91) produced a significant increase in muscle thickness but there were significant differences as a result of the 4 training protocols for some muscle groups. There was no significant difference in muscle thickness between G5 and G10, and G5 was significantly greater than G15 and G20 for the 5 assessed muscle groups; G10 was significantly greater than G15 and G20, and G15 was not significantly different from G20 for the biceps or quadriceps; G15 was significantly greater than G20 for the triceps, pectoralis major and gluteus maximus.

The males (92) significantly increased muscle thickness for the five assessed muscle groups in G5, G10, G15 and G20 after 24 weeks and there was no significant difference among the groups for any measures of muscle thickness. At 24 weeks however, muscle thickness for G5 was several times greater than G20, (~5x, 4x, 6x, 4x and 5x greater, for the biceps, triceps, pectoralis major, quadriceps femoris and gluteus maximus, respectively). Although those differences were not statistically significant, Barbalho and colleagues noted that the results suggested that there was an *inverse* dose-response relationship between training volume (sets per week) and muscle hypertrophy.

Barbalho and colleagues (92) commented that it was not clear if any of their measureable changes in muscle thickness actually translated into noticeable aesthetic improvements. For example, the smallest assessed muscle group (biceps brachii) in females (91) revealed a difference between 5 and 10 sets/week of 0.2 mm and the difference between 15 and 20 sets/week was 1.0 mm. In the largest assessed muscle group (quadriceps femoris), the difference between 5 and 10 sets/week was 0.7 mm and between 15 and 20 sets/week was 1.6 mm. Similarly in males (92), the difference between 5 and 10 sets/week was 0.6 mm and the difference between 15 and 20 sets/week was 1.2 mm for the biceps; the difference between 5 and 10 sets/week for quadriceps femoris was 1.4 mm, and between 15 and 20 sets/week was 1.8 mm.

Barbalho and colleagues (91-92) concluded that their results in both females and males indicated that 10 sets per muscle group per week represented an *upper threshold* for optimizing muscle hypertrophy and lower volumes (5 sets per week for each muscle group) produced a similar hypertrophic response.

Fragala and colleagues (2019)

In a recent Position Statement on resistance training in older adults from the National Strength and Conditioning Association (NSCA), with its 34 pages of unsupported windbaggery, Fragala and colleagues (93) claimed that a greater number of sets per session were associated with increases in lean body mass, but cited only a meta-analysis by Peterson and colleagues (94) and a review/meta-analysis by Borde and colleagues (95).

The meta-analysis by Peterson and colleagues (94) reported on 47 studies that included 1,079 adults ≥50 years old; however, they reported on strength changes only and did not report any measures of muscle hypertrophy or lean body mass. Peterson is one of the authors of the aforementioned Position Statement (93) and he should have known that his own meta-analysis (94) did not report any lean body mass data. The International Committee of Medical Journal Editors has stated that every author of a published manuscript is responsible for its entire content (96).

The review/meta-analysis by Borde and colleagues (95) included 25 studies comprised of 819 older adults (60-90 years). Nine of those studies reported the results of resistance training on muscle morphology (cross-sectional area, volume or thickness) measured with MRI, CT, DEXA, ultrasound, or air displacement plethysmography. The authors noted that because of the small number of studies, they were only able to calculate mega-regression for training volume. Borde and colleagues stated that no single training volume variable (e.g., frequency, the number of sets or repetitions) had a significant effect on muscle morphology. They noted also that on a PEDro scale of 0 (low quality) to 10 (high quality), their inclusive study scores averaged 4.6 (range 2-7) and admitted that those scores were indicative of low methodological quality. Readers may presume that any attempt to fact check the entire Position Statement from the NSCA (93) would be even more laborious than fact checking the American College of Sports Medicine Position Stands on resistance training (97-98).

Meta-analysis

Meta-analysis is a highly controversial statistical procedure where statisticians combine the data from several independent studies, sometimes with dissimilar methodology and from very different demographics, in an attempt to produce an estimate for the effectiveness of a specific resistance training intervention (e.g., the volume of exercise). The validity of a meta-analysis, also known by its critics as *numerological abracadabra*, is entirely dependent on the arbitrarily defined criteria and discrimination of the statistician, and most importantly on the quality of the inclusive studies (99).

There is much controversary concerning the validity of meta-analyses in general (100-106) specifically in the resistance training literature (89, 107-108). The decisions regarding the inclusion or exclusion of studies are dependant entirely on the assumably unbiased discretion of the person conducting the meta-analysis and that descretion can result in a wide range of subjective opinions (99, 103). The meta-analysis by Krieger (54) has been critically challenged in great detail by Fisher (108) but several additional key points are noteworthy.

Krieger (2010)

An effect size (ES) is a dimensionless number that represents the difference in the number of standard deviations between the pre- and post-test means, or between two or among several groups. It is the ratio of the difference between the means to the standard deviation (SD): ES = mean #2 (post-training mean) minus mean #1 (pre-training mean) divided by the SD. An effect size represents how many standard deviations the groups or pre- to post-training means differ in outcomes. A half-century ago, Cohen (109) arbitrarily and subjectively proposed an index for rating effect sizes that are still used today: small (0.2), medium (0.5), and large (0.8). It is important to recognize that Krieger classified all the effect sizes in his meta-analysis for low, moderate and high volume resistance training (ES = 0.24. 0.34 and 0.44, for 1 set, 2-3 sets and 4-6 sets, respectively) as *small* (54).

Krieger (54) reported that only two of the eight studies he included in his meta-analysis reported a significant difference in muscle hypertrophy between single set and multiple set groups (110-111). Those two studies involved only previously untrained subjects but are briefly described below.

Ronnestad and colleagues (2007)

Ronnestad and colleagues (110) randomly assigned untrained young adult males to perform either 1 set for each of the five upper body exercises and 3 sets for each of the three lower body exercises, or another group who followed a 3 sets upper body and 1 set lower body protocol 3x/week for 11 weeks. Although the authors did not indicae if the MRI assessors were blinded to the training protocols, they claimed that the increase in thigh muscle CSA was significantly greater the the group that performed 3 sets of each lower body exercise (11%) compared with the 1-set group (7%). There was no significant difference between groups for upper body hypertrophy. The researchers used DEXA, with the scans blinded, to report that lean body mass significantly increased in both groups, with no significant difference between the 1-set and 3-set protocols. Ronnestad and colleagues speculated that the lack of significance in upper body changes between groups may be that the upper body requires more than 3 sets per exercise. Perhaps their unsubstantiated implication inadvertently revealed their bias in favor of high volume resistance training; that is, their implication was that if 3 sets are not better than 1 set, perhaps more than 3 sets are necessary.

Marzolini and colleagues (2008)

The older male and female adults (~62 years) in the study by Marzolini and colleagues (111) were also previously untrained participants and with documented coronary artery disease. It is noteworthy that Marzolini and colleagues blinded their DEXA operator to the group assignments. After training 2x/week for 29 weeks, there was a significantly greater increase for only the thigh muscle mass in the 3-set group compare with the 1-set group ($p<0.05$). However, both the 1-set and 3-set groups significantly increased total lean body mass, lean arm mass, and lean trunk mass, and there was no significant difference between the 1-set and 3-set groups in any of those changes in total or segmental lean body mass. Neither Ronnestad and colleagues (110) nor Marzolini and colleagues reported the absolute chances in total or segmental lean body mass or muscle thickness—only the percent changes.

Rhea and colleagues (2002)

Only two (80, 112) of the eight studies included in Krieger's meta-analysis (54) recruited previously trained participants. Rhea and colleagues (112) trained 16 young adult males who they described as recreationally experienced weight trainees with at least 2 years of training 2x/week. However, with a body fat of ~20%, these young males apparently had poor genetic potential for muscle hypertrophy or they may have participated in more recreational activities than resistance training. Note that Krieger designated participants with <6 months resistance training as *untrained* and those with ≥6 months as *trained*. However, a visit to any fitness center with resistance training equipment would reveal trainees who have been training for >6 months—or even 6 years—with little or no significant muscular hypertrophy, which could challenge the very common practice of classifying participants as untrained or trained based solely on their duration of training.

The subjects trained 3x/week for 12 weeks performing so-called *daily undulating periodization* (8-10RM, 6-8RM and 4-6RM, weekly sessions 1, 2 and 3, respectively) for either 1 set or 3 sets of leg press and bench press exercises (112). There was no control group. Rhea and colleagues measured chest and thigh circumference and estimated body composition with whole-body plethysmography (BodPod). They did not indicate if the assessors were blinded to the training protocool. Although Rhea and colleagues did not report effect sizes, Krieger (54) claimed that their study effect size was 0.44, incorrectly labeled thigh circumference as *leg* circumference, and failed to report the lack of any significant change in body composition (Table 1, p. 1152-3). Rhea and colleagues noted briefly in their Results section that neither the 1-set or 3-set groups showed any significant change in body composition or circumference measures. However, they failed to mention anything about body composition or muscle hypertrophy in their Discussion or Practical Applications sections.

Ostrowski and colleagues (1997)

In the previously mentioned study by Ostrowski and colleagues (80), they randomly assigned 27 young adult males who had been resistance training for ~2.8 years to one of 3 training groups: low, moderate and high volume (3 sets/muscle group/week, 6 sets or 12 sets, respectively). Ostrowski and colleagues stated that because the majority of subjects had been following a training program very similar to the high volume protocol (12 sets/muscle group/week) prior to the study, they designated their high volume group as an *active control group*. The researchers used ultrasonography to quantify cross-sectional area and circumference of the rectus femoris and triceps brachia thickness. Although they acknowledged that ultrasound assessments are inherently subjective, they did not indicate if the ultrasound technician was blinded to the training protocols.

Participants trained 4x/week for 10 weeks performing 6 exercises at each of the 4 weekly sessions (80). It should be recognized that some muscles were exercised with a higher volume than others; for example in the high volume group, the triceps were involved in 28 sets/week. Each set of all the exercises was executed to *muscular failure* for 12RM, 7RM and 9RM, weeks 1-4, 5-7 and 8-10, respectively. Ostrowski and colleagues reported that pre and post-test assessments of the rectus femoris circumference and cross-sectional area and triceps thickness did not differ significantly among the 3 groups but there was a significant increase for the 3 groups combined. For example, triceps thickness increased 1 mm in the low volume group and 2 mm in the high volume group, which although it was 100% greater in the high volume group, it was only a 1 mm difference and was not significantly different between groups. Ostrowski and colleagues concluded that the low volume program produced changes in muscle size that were similar to the moderate and high volume programs.

Krieger (54) stated that the mean effect size in the study by Ostrowski and colleagues (80) was 0.24 for 1 set/exercise, 0.34 for 2-3 sets/exercise, and 0.44 for 4-6 sets/exercise. He noted that these differences in effect size between 1 set and 2-3 sets (0.1), and between 2-3 sets and 4-6 sets (0.1) were not statistically significant. However, after he performed a Hochberg-adjusted permutation test (113), Krieger claimed that those differences were significant. Readers may be curious to know how a trainer or coach would respond to a trainee who asks for an explanation of how the Hochberg-adjusted permutation test magically converted the minuscule non-significant difference between groups to statistically significant differences. Perhaps more *statistical abracadabra* would be a justified response.

Krieger (54) claimed several times (p. 1150, 1156, 1158) that multiple sets were associated with 40% greater hypertrophy-related effect sizes compared with 1 set in both previously untrained and trained subjects. Note that he did not state that muscle hypertrophy would be 40% greater with multiple sets. He carefully used the words *hypertrophy-related effect sizes*. For example, an effect size of 0.34 (2-3 sets) is 40% greater than an effect size of 0.24 (1 set). Perhaps it would be unfair to criticize motivated, inquisitive trainees for incorrectly inferring that performing several times the volume of exercise would result in 40% greater muscle hypertrophy. Would trainers and coaches be embarrassed to admit that they did not fully understand the concept of effect sizes or the Hochberg-adjusted permutation test? As previously noted in the study by Ostrowski and colleagues (80), triceps thickness increased 1 mm in the low volume group and 2 mm in the high volume group, which although it was 100% greater in the high volume group, it was only a 1 mm difference and was not significantly different between groups. Readers may wonder if a 1 mm difference is really worth performing 4-6 times the volume of training?

Dankel and colleagues (114) have noted that effect sizes are arbitrary units subject to arbitrarily designated set points (small, moderate or large). They argued that effect sizes and percent changes should only be reported in addition to the raw data and that conclusions based only on percent change or effect size can produce misleading results (114). It is important to recognize that Krieger (54) did not report any raw data (e.g., muscle thickness in mm), only the percent changes and his estimated effect sizes.

Krieger (54) defined a *trend* as a *p* value of ≤10, and subsequently claimed that there were *trends* for an increase in effect size with an increased number of sets. He reported that there was a *trend* for 2-3 sets to be better than 1 set (p = 0.09), a *trend* indicating that 4-6 sets were better that 1 set (p = 0.096), and that there was no trend between 4-6 sets and 2-3 sets (p = 0.29). However, Gibbs and Gibbs (115) noted that describing an almost but non-significant difference as a *trend* is not simply a trivial error. They argued that the outcome of an inferential test is to either reject the null hypothesis or fail to reject it, and that there is *no almost rejected category*. They noted also that claiming a *trend* for a *p* value that is close but not quite statistically significant (e.g., p = 0.06) reveals a similar—although antithetical—deceptive logic as describing a *p* value that only just achieved statistical significance (e.g., p = 0.04) as a *trend* toward non-significance. Gibbs and Gibbs emphasized that implying the existence of an almost rejected category is a serious statistical error.

Furthermore, Wood and colleagues (116) commented that describing a nearly significant *p* value as a *trend* is not just inappropriate but misleading, and undermines the principle of accurately reporting the data. Sometimes when authors fail to get the results they were hoping for, they resort to statistics that are misleading—at best.

Many of the studies by Schoenfeld and colleagues cited in this Critical Commentary, which included Krieger as their statistician, referred to an almost significant *p* value as a *trend*. For example in their 2016 meta-analysis, Schoenfeld and colleagues (76) reported a 0.023 increase in effect size (0.37%) for each additional weekly set and a 9.8% greater gain for >10 additional sets per week. As previously noted in the study by Ostrowski and colleagues (80), the difference in triceps thickness (100%) between low and high volume training was 1 mm. Is the difference (9.8%) with 10+ additional sets reported by Schoenfeld and colleagues comparable to 0.1 mm greater muscle thickness? Are Schoenfeld and colleagues actually recommending an additional 10+ weekly sets for each muscle group to produce a greater hypertrophic increase of one tenth of a millimeter? Readers can decide if their reported minuscule differences justify the conclusion that their meta-analysis actually indicated a graded dose-response relationship between a greater volume of exercise (additional weekly sets) and greater muscle hypertrophy.

Krieger Sub-Section Summary: There is a lack of robust scientific evidence in Krieger's meta-analysis (54) to support a significant dose-response relationship between the volume of exercise (multiple sets) and muscle hypertrophy—in fact, the evidence is nonexistant. However, for the last decade Schoenfeld and colleagues (including Krieger as a co-author and Schoenfeld's statistician)—along with other purveyors of nitwittery—continue to cite Krieger's meta-analysis ad nauseam as their *compelling* evidence.

Schoenfeld (2010)

Approximately one decade ago, Schoenfeld (31) claimed that higher volume training, which he defined as the product of the number of repetitions, sets, and amount of resistance, had *consistently* proven to be superior to lower volume training for increasing muscle hypertrophy. In in an attempt to support his belief for higher volume, he cited only a review by Wolfe and colleagues (117) who did not assess or report any measure of muscle hypertrophy, and the previously discussed meta-analysis by Krieger (54). The only consistency is the lack of substantial supporting evidence for the superiority of higher volume training.

Just a few years later (2013) on a website belonging to Bret Contreras (The Glute Guy) https://bretcontreras.com/the-b-b-connection-episode-2-hit-training-vs-higher-volume-training/, Schoenfeld noted that as both a researcher and practitioner, he focused on optimizing his hypertrophic response to resistance training. Schoenfeld claimed that after lifting weights for 30 years, he gained more muscle mass with higher volume training but always reverted to HIT (high intensity low volume training) because of overtraining or injuries. Schoenfeld stated that any of his additional gains from the higher volume training were temporary and then he actually began to lose muscle mass, suffer injuries, or he just felt sick. For these reasons he then claimed that in the long run, the lower volume training was much safer and resulted in an overall greater muscle mass. Readers can decide whether these comments by Schoenfeld actually support or contradict what he previously claimed about higher volume training (31).

***Exercise Volume Section Summary*:** There is no compelling evidence to support the claim that higher volume resistance training (3, 4, 5, 6, 10, 15, or 20 additional sets) will elicit superior muscle hypertrophy.

Interset Rest Intervals

Henselmans and Schoenfeld (2014)

Henselmans and Schoenfeld (118) correctly concluded in their review of interset rest intervals that the existing resistance training literature did not support the superiority of either shorter or longer interset rest to stimulate muscle hypertrophy. The three longitudinal training studies that they cited (Schoenfeld and colleagues (53), Buresh and colleagues (119), and Ahtiainen and colleagues (120) are worth mentioning.

Buresh and colleagues (2009)

The study by Schoenfeld and colleagues (53) was previously mentioned in this Critical Commentary and as reported by Henselmans and Schoenfeld (118) there was no significant difference in muscle hypertrophy in groups of well trained young adult males after training with 90 s or 3 min interset rest. The similar hypertrophic responses occurred despite all the differing training variables between groups (resistance, reps, sets, frequency). Buresh and colleagues (119) recruited previously untrained young adult males to compare 1 min and 2.5 min interset rest after 2-3 sets for each of 9 exercises in one session and 7 exercises in another session, 4 days/week (split routine) for 10 weeks. Thigh and arm cross-sectional area was estimated with formulas that included limb circumference and skinfold thickness, but they did not indicate if the assessors were blinded to the training protocols. Both groups significantly increased muscle CSA, and the arm CSA was significantly greater in the 2.5 min group. There was no significant difference between groups for thigh CSA in these previously untrained males.

Ahtiainen and colleagues (2005)

Ahtiainen and colleagues (120) recruited 13 resistance trained young adult males (~6.6 years of continuous resistance training) who they allocated to a crossover design study for two 3-month training periods using 2 min or 5 min interset rest intervals. The trainees performed multiple sets of 10RM knee extension, leg press and squat exercises. The researchers wanted to equate the total volume of exercise (resistance x reps x sets) in both the training periods. When the trainees rested for 2 min (1st 3 months), they completed a greater number of sets with a lighter resistance than when they rested 5 min between sets. With 5 min rest (2nd 3 months), they used a resistance that was 14% greater in the leg press and 30% greater for squats. The researchers used MRI to measure thigh muscle cross-sectional area but they did not indicate if the assessors were blinded to the specific training protocols.

Henselmans and Schoenfeld (118) stated correctly that there was no significant difference between the two 3-month training protocols (2 min or 5 min interset rest) for muscle hypertrophy (120). However, they neglected to state that the trainees did not significantly increase thigh muscle mass during either of the two 3-month periods (1.8 and 1.8%, respectively). They also failed to state that the study began with 20 volunteers whose primary goal was to gain maximal muscle mass and—as described by Ahtiainen and colleagues—7 out of the 20 dropped out of the study because of training-induced knee and back pain. Readers may want to consider the number of dropout injuries (35%) in these experienced young adult males (age ~28 years) while performing their high volume training for the knee extensors (4-5 sets of leg presses and 3-4 sets of squats), which did not elicit a significant increase in muscle mass, and question the practical application of these high volume protocols in any demographic.

Ahtiainen and colleagues (120) concluded that shorter or longer interset rest intervals did not influence the magnitude of muscle hypertrophy, and that the different number of sets or the amount of resistance during each 3-month period also had no significant effect on the degree of muscle hypertrophy.

In a systematic review, Grgic and colleagues (121) claimed that their results suggested that longer inter-set rest intervals may have an advantage over shorter rest intervals for eliciting muscular hypertrophy in previously trained subjects. However, the only study in their review that involved previously trained participants was by Schoenfeld and colleagues (122).

Schoenfeld and colleagues (2016)

Schoenfeld and colleagues (122) compared the effect of 1 min and 3 min interset rest on muscle hypertrophy in 21 previously trained young adult males. All the trainees completed 3 sets of 8-12RM to *momentary concentric muscular failure* 3x/week for 8 weeks. The researchers used ultrasound to measure muscle thickness of the elbow flexors, triceps, anterior quadriceps, and vastus lateralis, but they did not indicate if the assessors or the statistician were blinded to the specific training protocols. The only significant difference between groups in muscle thickness after 8 weeks of training was the anterior quadriceps that was 3.5 mm thicker in the 3-min group. There was no significant difference between groups for the other three muscle groups; that is, 3 out of 4 muscle groups showed no significant difference in muscle hypertrophy as a result of training with 1-min or 3-min interset rest intervals.

It is noteworthy also that Grgic and colleagues (121) stated that they categorized the methodology in their inclusive studies as either good or excellent. Blinding means that the people involved in the study (subjects, trainers or the assessors) do not know which training protocol the subjects were assigned. The PEDro scale (Physiotherapy Evidence Database) was designed to evaluate the quality of randomized controlled trials. If the assessors are blinded, readers could be confident that the effect (or lack of effect) of the training protocol was not because of any assessor conscious or subconscious bias (123). Grgic and colleagues noted that the blinding of assessors was important; however, they eliminated numbers 5, 6, and 7 from the PEDro scale of 0-10 (blinding of subjects, trainers and assessors, respectively). They rated the study by Schoenfeld and colleagues a quality score of 5, which Grgic and colleagues considered a *good* quality score. In a Disclosure statement they declared no potential conflict of interest; however at least two of those authors (Schoenfeld and Krieger) have websites where they promote themselves as experts on muscle hypertrophy and sell products such as online coaching, books, audio and video discs, nutritional supplements, apparel, etc. The topic of conflict of interest is discussed in another section of this Critical Commentary.

Interset Rest Intervals Section Summary: Most of the aforementioned studies showed no significant difference in muscle growth as a result of training with shorter or longer interset rest. One study that recruited experienced trainees reported a significantly greater increase in one muscle group but no significant difference in 3 out of the 4 muscle groups.

Self-Described Bias

It must be emphasized that although the following paragraphs clearly reveal overwhelming evidence for Schoenfeld's self described bias in favor of higher volume training for maximal hypertrophic adaptations, there is no evidence that the bias influenced his objectively while performing the ultrasound assessments or calculations in his previously discussed training study (77) or any of his other studies or reviews.

For at least several years, Schoenfeld has demonstrated a bias favoring higher volume resistance training. On his personal website (http://www.lookgreatnaked.com/blog/my-journey-to-a-doctoral degree/), where he has labeled himself as *the hypertrophy specialist*, Schoenfeld stated that it was important for him to choose a doctoral program with a director whose views were similar to his. His choice for doctoral mentor was Brent Alvar. His other doctoral committee members were Mark Peterson and Nicholas Ratamess, all of whom are advocates of high volume resistance training. Alvar, Peterson and Rhea (another high volume advocate) co-authored a so-called *symposia* (124) where they listed several articles that challenged their claims regarding high volume training (125-127) as well as a critical analysis of the American College of Sports Medicine position stand on resistance training (97). Alvar and colleagues specifically cited and quoted several of the criticisms from the aforementioned articles but offered no defense of those criticisms, only a snide comment that the criticisms were *familiar biased dialog*. Ratamess was the lead author, along with Alvar and others in a follow-up ACSM position stand on resistance training (128).

Ratamess and colleagues (128) claimed that greater hypertrophy is associated with high volume training and recommended higher volume (more sets per exercise and higher frequency) for advanced resistance trained individuals. They cited three references in an attempt to support their recommendation (71, 129-130). However, two of those studies recruited previously untrained participants (71, 130). The third reference (129) was from a database (5 *experiments*) that the author (Kraemer) resurrected from his coaching days 15 years prior to publication. Most importantly, Kraemer did not report any measure of muscle hypertrophy in any of his five *experiments*. Consequently, not only did the references cited by Ratamess and colleagues in the ACSM position stand (128) fail to support their recommendations for optimal muscle hypertrophy in advanced trainees, but there was very little credible evidence to support many of their recommendations (98, 131). Nevertheless, Schoenfeld chose to be associated with people who also believed—without any compelling evidence—that higher volume resistance training was superior to lower volume training.

The Max Muscle Plan (2013)

Schoenfeld made several dogmatic claims and recommendations in his book entitled The M.A.X. Muscle Plan (132). MAX is an acronym for mitogen-activated extreme training and the goal is to enhance mitogenic training responses that allegedly promote *optimal* musclar development. Schoenfeld claimed that trainees should not worry about genetic potential because genetics is responsible for only 25-50% of one's potential to build muscle and that anyone could *unquestionably* develop an impressive muscular physique with The MAX Muscle Plan.

Schoenfeld (132) made several claims in his book: 1. 65-85% 1RM is the *optimal* load for muscle hypertrophy; 2. dismissing the muscle pump as a significant factor that affects muscle hypertrophy is short sighted; 3. trainees should lift the resistance as quickly as possible (explosively) during the concentric muscle action; 4. super slow training cannot compare with traditional training for improving muscle hypertrophy; 5. eccentric muscle actions *preferentially* recruit fast twitch motor units; 6. *at least* three sessions per week are required to maximize muscular hypertrophy; 7. periodized training will *optimize* results; 8. greater muscle tension means greater muscle hypertrophy.

However, Schoenfeld failed to cite a single reference to support any of his claims and recommendations, which are shown throughout this Critical Commentary to be devoid of any robust support. Schoenfeld believed, and apparently still believes, that higher volume training has *consistently* been shown to be superior to lower volume training and that multiple sets of exercise are required to maximizing muscle hypertrophy. The only reference he cited was the aforementioned meta-analysis by Krieger (54), and as previously discussed in this Critical Commentary, is without merit.

Schoenfeld (132) also claimed that studies showed that heavy eccentric muscle actions (accentuated negative resistance) can enhance the hypertrophic response and he cited his previously discussed article on specialized resistance training techniques (14). In that article he discussed in detail the specifics of eccentric training but stated only that heavy negatives (accentuated eccentric muscle actions combined with assisted concentric muscle actions) *may* produce an additional hypertrophic stimulus. In the Conclusion section of that article, Schoenfeld claimed that the evidence suggested a beneficial effect for heavy negative training, recommended the amount of eccentric resistance (105-125% of the concentric1RM), and a specific rep duration of 2-3s. However, the only reference for those recommendations was his previously discussed article on the mechanisms of muscle hypertrophy (31). He failed to cite any resistance training studies.

In that review article on the mechanisms of muscular hypertrophy (31), Schoenfeld discussed the neurophysiology of eccentric muscle actions, the tension developed by the contractile elements, and how they *may* enhance the hypertrophic response. He claimed that the load (% 1RM) is the most important exercise variable for stimulating muscle hypertrophy and that each specific range of repetitions (described as low (1-5 reps), moderate (6-12 reps) and high (>15 reps) stimulates the neuromuscular system in different ways. According to Schoenfeld, high rep training does not provide an adequate load to recruit and fatigue the larger high threshold motor units. He failed to support those statements with any resistance training studies.

In his discussion of eccentric resistance training, Schoenfeld (31) cited a study by Shepstone and colleagues (133) who compared the results of training with eccentric-only muscle actions at two different rep durations in contralateral arms on an isokinetic device 3x/week or 8 weeks. Even Schoenfeld acknowledged that this study had very limited practical application to training with gravity-dependant resistance such as free weights, plate loading and selectorized weight stack machines and pulleys. Most importantly, Shepstone and colleagues did not incorporate any form of accentuated eccentric resistance training as it was described by Schoenfeld.

Science and Development of Muscle Hypertrophy (2016)

In his book entitled Science and Development of Muscle Hypertrophy (134), Schoenfeld stated that there was *compelling* evidence for a clear and positive dose-response relationship between training volume and muscle hypertrophy; and that more advanced lifters may require double the volume of previously untrained subjects. He cited one training study by Radaelli and colleagues (135) and the previously discussed meta-analysis by Krieger (54).

Radaelli and colleagues (2015)

It is worth noting that Radaelli and colleagues (135) randomly assigned 48 young adult males with no previous weight training experience to a 1-set, 3-set, 5-set, or control group. The trainees performed 9 upper and lower body exercises to the point of *concentric failure* (8-12RM) 3x/week for 6 months. The authors did not indicate if the ultrasound technician was blinded to the training protocols. There was no significant increase in either the elbow flexors or extensors in the 1-set group or the control group. The 3-set and 5-set groups significantly increased elbow flexor thickness and the increase in the 5-set group was significantly greater than the 3-set group. Elbow extensor thickness significantly increased only in the 5-set group. Curiously, 3 sets for each of these three elbow extensor exercises (bench press, shoulder press and triceps [elbow] extension) 3x/week for 6 months in previously untrained young adult males failed to produce any significant increase in elbow extensor muscle thickness. Radaelli and colleagues failed to address this point anywhere in their narrative.

The way Radaelli and colleagues (135) reported their results for muscle thickness was highly unusual and certainly questionable. They detailed the pre- and post-training means, standard deviations and confidence intervals for volume (reps x sets x resistance), 5RM bench press, lat pull-down, shoulder press and leg press, 20RM bench press and leg press, countermovement jump height, percent body fat and absolute fat-free mass in their Tables 1-5. However, they failed to report any specific absolute data or percent change for muscle thickness in a table or their narrative other than one very small bar graph labeled in cm (Figure 3, p.1354), which made it very difficult to interpret the actual increases in muscle thickness or any possible differences among the groups.

Radaelli and colleagues (135) reported a significant increase in fat-free mass pre- to post-training in all the groups, including the control group, with no significant difference among the groups (~4.7, 0.7, 4.7 and 4.7%, control, 1-, 3- and 5-set groups, respectively). The control group, who performed traditional military body weight calisthenics for 1 hour/day, 3x/week, significantly increased fat-free mass (2.9 kg, 4.7%), which was identical to the 3-set group (2.9 kg, 4.7%) and very similar to the 5-set group (3.3 kg, 4.7%). Their data indicated that there was ~5 times greater increase in fat-free mass for the control group compared with the 1-set group, although that difference was not statistically significant. The authors also failed to address these unusual results. Their only comment was that skinfold measurements may not be sensitive enough to measure changes in body fat. However, they apparently believed that they were sensitive enough to use for their estimate of percent body fat and subsequently calculate and report significant changes in fat-free mass. The conclusion by Radaelli and colleagues (135) in their Discussion and Practical Applications sections that their results supported a dose-response relationship between multiple sets and muscle hypertrophy was not supported by their own reported data, and it was misleading at best.

***Bias Section Summary*:** Schoenfeld failed to cite any *compelling* evidence to support the claim in his book regarding regarding training volume and muscle hypertrophy in advanced trainees. Although it has been documented that Schoenfeld has repeatedly expressed his bias for higher volume training, it must be emphasized that there is no evidence that it influenced his reporting of results.

Frequency of Training

Brigatto and colleagues (2019)

For the purpose of this Critical Commentary, frequency of training is defined as the number of times a muscle group is trained per week—not necessarily the number of exercise sessions per week. The frequency of training may also contribute significantly to the weekly volume of exercise per muscle group; e.g., 3 sets per muscle group 2x/week would yield a greater weekly volume of exercise than 3 sets 1x/week.

Brigatto and colleagues (136) randomly assigned 20 young adult males, with ~5 years resistance training experience, to train each muscle group either in 2 weekly sessions (5 exercises on Mon, 4 exercises on Thurs) or 4 weekly sessions (5 exercises on Mon and Thurs, 4 exercises on Tues and Fri). Both groups completed the same number of weekly sets with 8-12RM for each exercise during the 11 week study. The researchers used ultrasound to measure thickness of the triceps, elbow flexors, vastus lateralis, and anterior quadriceps pre- and post-training, but did not indicate if the ultrasound technician was blinded to the training protocol.

Both groups significantly increased thickness of all the assessed muscle groups and there was no significant difference between groups for any of those changes (136). Brigatto and colleagues concluded that both training frequencies produced a similar increase in muscle thickness.

Brigatto and colleagues (136) noted that they used this high volume protocol because of the *clear* dose-response relationship between the volume of exercise and muscle hypertrophy. They cited only the previously discussed meta-analyses by Schoenfeld and colleagues (76) and Krieger (54). Brigatto and colleagues failed to indicate which author was the trained ultrasound technician, whether that person was blinded to the training protocol, or what Schoenfeld's contribution was to this study from a different country (Brazil).

Gomes and colleagues (2019)

Gomes and colleagues (137) randomly allocated 23 young adult males with ~6.5 years of resistance training experience to a low frequency group (trained specific muscle groups 1x/week) or high frequency group (trained all muscle groups at each of 5 sessions/week). High and low frequency referred to the number of times each week that the participants trained a specific muscle group. Both groups trained 5x/week and performed the same number of weekly sets using an 8-12RM with equated exercise volume and session time for 8 weeks. Gomes and colleagues stated in their Methods section that both groups performed 10 sets of each exercise except 5 sets each for barbell curls and triceps [elbow] extensions (p. S136). Because the training protocol in their poorly labeled and perhaps confusing Table 1 (p. S132) lists 10 sets for each of 6 exercises and 5 sets for each of 5 other exercises, it is not clear how many sets were performed for each exercise. Researchers used DEXA to estimate total fat-free mass, and what they labeled as trunk, android, gynoid, upper trunk, leg, and arm fat-free mass. They did not indicate if the DEXA assessor was blinded to the training allocation.

Both groups significantly increased (p<.05) total, trunk, gynoid and thigh fat-free mass (137). There was no significant difference between groups for any measured outcome. The authors speculated that because of the lower number of sets per session in the high frequency group, there may have been less session fatigue that could result in a greater total training volume. Although the researchers attempted to equate the training volume, the high frequency group performed ~14% more total training volume. However, the greater training volume did not result in greater gains in fat-free mass. Gomes and colleagues concluded that both of the aforementioned resistance training strategies promoted similar gains in fat-free body mass in well trained young adult males.

Yue and colleagues (20018)

Yue and colleagues (138) randomly allocated 18 young adult males who were resistance training 2-3x/week for ~3 years to a low volume-high frequency group (routine 1 on Mon and Thurs, routine 2 on Tues and Fri) or a high volume-low frequency group (routines 1 & 2 on Mon and Thurs). It should be recognized that the terms high and low volume refers to the exercise volume per session but the weekly volume was similar for both groups. They performed 9 different exercises per session (18 and 36 sets/session, LV-HF and HV-LF, respectively). All sets were performed to what the authors described as *8-12 self-determined maximum repetitions* for 6 weeks. An independent blinded researcher assessed muscle thickness of the elbow flexors, anterior deltoids and vastus medialis with ultrasound measurements.

Elbow flexor thickness significantly increased only in the HV-LF group, vastus medialis in both groups, and no significant increase in anterior deltoids for either group (138). It is noteworthy that the difference between the significant increase (HV-LF) and non-significant increase (LV-HF) in elbow flexors was only ~0.8 mm between groups. The conclusion by Yue and colleagues that only HV-LV training was effective for enhancing upper body muscle thickness was not supported by their own results, and was misleading at best.

Schoenfeld and colleagues (2016)

Schoenfeld and colleagues (139) reported the results of seven studies that compared frequencies of training muscle groups from 1-3 times a week and changes in muscle hypertrophy. Although the difference in effect size was deemed *modest* (ES = 0.19) and subjective, they claimed that the current body of evidence indicated higher frequencies were consistently superior to lower frequencies for increasing muscle mass and that muscle groups should be trained at least twice a week to maximize muscle hypertrophy. Their rationale was that the higher frequencies allowed for higher volumes of training. They claimed that there was a dose-response relationship between volume and muscle hypertrophy. However, the only reference they cited was the previously discussed meta-analysis by Krieger (54). Only two of the inclusive studies involved previously trained young adult males (140-141).

The study by Ribeiro and colleagues (140) used DEXA to compare changes muscle hypertrophy after training 4 versus 6 days/week (a similar volume of exercise just distributed differently). There was no significant difference between groups in lean body mass. It is also worth mentioning that Schoenfeld and colleagues (139) claimed that their results raised the *possibility* that very high frequencies may be beneficial for enhancing muscle hypertrophy in experienced trainees; however, there is also the *possibility* that Earth has had visitors from ancient extraterrestrial astronauts who used anti-gravity or teletransporter technology to travel from different star systems and galaxies. Neither possibility has any substantial evidence for support. The other study by Schoenfeld and colleagues (141) is discussed below.

Schoenfeld and colleagues (2015)

Schoenfeld and colleagues (141) recruited 20 resistance trained young adult males who they assigned 2-3 sets for each of 21 exercises using either a SPLIT (2-3 muscle groups trained at each of 3 weekly sessions) or a TOTAL (all muscle groups trained at each of 3 weekly sessions) training protocol for 8 weeks. Ultrasound imaging revealed a significant increase in elbow flexors, elbow extensors and vastus lateralis muscle thickness. They did not indicate if the ultrasound technician was blinded to the resistance training protocol. Only the changes in elbow flexor muscle thickness showed a small but significantly greater increase (1.1 mm) in the TOTAL group (3.2 mm vs. 2.1 mm).

Schoenfeld and colleagues (141) commented on the large interindividual variability of responses among subjects but they reported their standard deviations only in a figure, not a table. Based on their 1.1 mm difference in elbow flexor thickness, which was the only muscle group to show a significant difference, they claimed that their results suggested that there is a *superior* hypertrophic benefit to a higher frequency of training. Their rational was that higher frequency allows a greater volume of exercise and that there is a dose-response relationship between the volume of exercise and muscular adaptations. They cited two references: the previously discussed meta-analysis by Krieger (54) and another meta-analysis by Krieger (142). However, Krieger reported only on strength gains—not muscle hypertrophy.

Carpinelli (99) critically challenged that meta-analysis and he showed that it lacked any credible evidence for support.

Schoenfeld and colleagues (2019)
A most recent meta-analysis by Schoenfeld and colleagues (143) reported the results of 13 studies that compared different frequencies of training a muscle group per week (e.g., 1 vs. 2, 2 vs. 3, 2 vs. 3 vs. 5, and 3 vs. 6x/week, etc.). On a volume equated basis, there was no significant difference in muscle hypertrophy between lower and higher frequencies of training in upper or lower body muscle groups, using direct or indirect hypertrophy assessments. The percent increase in muscle hypertrophy for volume equated studies that used direct measurements were 5.4±1.9, 5.4±1.7, 7.6±1.5, and 6.3±2.3, for 1, 2, 3 and 4-6 days/week, respectively. Ten of their studies involved resistance trained subjects and also showed no significant difference between lower and higher frequencies of training.

Schoenfeld and colleagues (143) concluded that there was strong evidence that training frequency did not significantly or meaningfully impact muscle hypertrophy. Based on their previous claim for a dose-response relationship between training volume and muscle hypertrophy, Schoenfeld and colleagues (139) claimed that frequency can be used as a tool to increase training volume. However, they antithetically noted that because the difference in hypertrophy in non-equated volume studies revealed a trivial effect size of only 0.18 between the different volumes of exercise, the small difference in hypertrophic outcome would challenge any practical benefit of a greater volume of exercise to produce significantly greater muscle hypertrophy.

Saric and colleagues (2019)
Saric and colleagues (144) randomly assigned 27 previously trained young males to train each muscle group 3x/week (RT3) or 6x/week (RT6) for 6 weeks. The groups were volume equated (load x reps x sets) and they completed 4 sets of each exercise in RT3 and 2 sets each in RT6 for 6-12 repetitions to *muscular failure*. The authors did not indicate who performed the ultrasound assessments of the elbow flexors and extensors, rectus femoris and vastus lateralis, or if the assessor was blinded to the specific training protocol.

Ultrasound assessments revealed a significant increase in muscle thickness in the four muscle groups measured (144). There was no significant difference between the RT3 and RT6 groups in the changes in muscle thickness except for the elbow flexors, which significantly increased only in the RT3 group. The authors did not discuss or even speculate why, despite a 20% greater training volume in the RT6 group, there was an almost identical increase in rectus femoris thickness in the RT3 (1.2 mm) and RT6 (1.4 mm) groups. As previously noted in this Critical Commentary, two of the study's co-authors (Krieger and Schoenfeld) have repeatedly claimed over the years that there is a dose-response relationship between training volume and muscle hypertrophy but they failed to address this apparent conflicting outcome.

Saric and colleagues (144) noted the equal contribution by most of the co-authors to the study but they did not indicate how Schoenfeld or Krieger—both from the USA—contributed to this study that was done in Croatia. There is a detailed discussion on authorship is the Author Contribution section of this Critical Commentary.

Grgic and colleagues (2019)
Grgic and colleagues (145) reviewed 26 studies that compared frequency of training with a frequency range of 1-4x/week. Their overall conclusion was their results strongly suggested that when the volume of exercise is equated, resistance training frequency does not appear to have a significant or meaningful effect on muscle hypertrophy. Ten of those studies used what the authors defined as a *direct measurement* of muscle hypertrophy (ultrasound, MRI or muscle biopsies). The authors reported that resistance training 1x/week produced similar gains in muscle hypertrophy compared with training 2-3x/week. Only two of those studies involved resistance trained participants (115, 136). As previously discussed, Schoenfeld and colleagues (115) reported a significant increase in elbow flexor, elbow extensor, and vastus lateralis muscle thickness, with the increase in elbow flexor muscle thickness significantly greater (1.1 mm) in the TOTAL group compared with the SPLIT group. Brigatto and colleagues (136) concluded that there was no significant difference in the changes in muscle thickness in any of the muscles they assessed.

It is worth mentioning that Grgic and colleagues (145) speculated also that greater frequencies may introduce more *variety* into the workout, thus augmenting muscle hypertrophy because of its *novelty* effect. They failed to cite any references to support their speculation. In 2012, Schoenfeld co-authored an internet article with Bret Contreas entitled *6 Lessons Learned from the Master Blaste*r (https://t-nation.com/training/6-lessons-learned-from-the-master-blaster) where they discussed six of their favorite Weider Training Principles, which included *The Master Blaster's Muscle Confusion Principle* (42). This principle suggests that thousands of tweaks for an exercise can be used to *confuse* the muscles. Perhaps Grgic and colleagues gleaned their recommendation for *variety* from another very high volume training advocate–The Master Blaster.

Barcelos and colleagues (2018)
Although a recent study by Barcelos and colleagues (146) recruited previously untrained young adult males, their results and follow-up analysis of individual muscle hypertrophy (147) deserve mention. Barcelos and colleagues (146) compared contralateral thigh hypertrophy after high frequency (5x/week) or low frequesncy (2x or 3x/week) resistance training. All the participants perfromed 3 sets of 9-12RM knee extension exercise to *muscular failure* for 8 weeks. The researchers measured vastus lateralis cross-sectional area with ultrasound and blinded the assessors to the training protocol allocations. They designated the volume of exercise as sets x reps x load, which was significantly greater in the high frequency limb (5x/week) compared with the contralateral low frequency limb (2x or 3x/week). There was a significant increase in cross-sectional area after training 2, 3 or 5 times a week (~11, 11, and 12%, respectively), with no significant difference in muscle hypertrophy among the training frequencies. Barcelos and colleagues concluded that higher training frequencies did not result in greater gains in muscle hypertrophy—despite producing a significantly higher ($p<0.0001$) training volume throughout the 8 weeks of training.

Damas and colleagues (2019)

In a follow-up to the study by Barcelos and colleagues (146), Damas and colleagues (147) reported the individual muscle hypertrophy responses. If an individual showed a difference in cross-sectional area for high frequency versus low frequency training within two typical errors (1.38%), the authors declared no significant difference in hypertrophy as a result of high or low frequencies. As previously noted, the training volume was significantly greater in the high frequency limb (5x/week) compared with the contralateral low frequency limb (2x or 3x/week). They reported that 31.6% of the subjects had a better hypertrophic response with higher frequency training; 36.8% responded better with lower frequency; and 31.6% showed no difference in muscle hypertrophy between higher or lower frequencies.

Damas and colleagues (147) noted that these individual responses occurred in previously untrained subjects, which challenged the notion that almost any training protocol will elicit a maximal response in this novice demographic. The authors concluded that interindividual hypertrophic responses to resistance training primarily depend on the individual genetic pre-disposition to resistance training.

Gentil and colleagues (2018)

Gentil and colleagues (148) allocated 16 resistance trained (at least 1 year) young adult males to perform 8 upper body exercises (4 exercises involved the elbow flexors) either in 1 session a week (G1) or 2 sessions per week (G2). All the trainees performed 3 sets of 8-12 repetitions to *momentary concentric failure* for 10 weeks. The G2 group performed 4 of the 8 exercises on Monday (2 involved the elbow flexors) and the other 4 exercises (2 for the elbow flexors) on Thursday; thus, the weekly volume of exercise for G2 was similar to G1 but G2 stimulated the elbow flexors 2x/week. The researchers assessed elbow flexor thickness with ultrasound but did not indicate if the technician was blinded to the training protocol.

G1 significantly increased muscle thickness (3.1%) but there was no significant increase in G2 (148). The authors reported the individual absolute pre- to post-training changes for the 16 trainees only in a figure, but it appeared that the mean increase in G1, although statistically significant, was only approximately 1 mm.

Gentil and colleagues (148) concluded that experienced trainees who have been training at greater frequencies (2-3x/week) may overcome hypertrophic plateaus by keeping the weekly number of sets per muscle group constant and decreasing the frequency of training. Because of the obvious interindividual variability in baseline muscle thickness and the individual responses to the two training frequencies shown in their Figure 1 (p. 6), the ability to assess and interpret such a small difference and the relevance of a mean increase of only 1 mm in G1 may be questionable. The differences in muscle thickness—pre- and post-training—support the original hypothesis of Gentil and colleagues that training 1 or 2 days a week would result in similar gains in muscle hypertrophy.

Lasevicius and colleagues (2019)

Lasevicius and colleagues (149) pair matched 36 previously trained (~3.2 years experience) young adult males from their baseline strength and muscle thickness and then randomly allocated them to either SPLIT or TOTAL body volume-equated resistance training. They performed 4 upper body and 3 lower body exercises either in a SPLIT routine (each muscle group was trained 2x/week in 4 weekly sessions on Mon, Tues, Thurs, Fri or a TOTAL group who trained all the muscle groups 3x/week in 3 weekly sessions (Mon, Wed, Fri). The SPLIT group performed 6 sets of each exercise while the TOTAL group completed 4 sets of each exercise. Both groups completed 8-12 repetitions to the point of *momentary concentric muscular failure* for each exercise. An ultrasound technician measured muscle thickness of the elbow flexors and extensors, rectus femoris and vastus lateralis but did not indicate if the technician was blinded to the training protocols. After 10 weeks of training, there was a significant increase rectus femoris, vastus lateralis and elbow extensor muscle thickness with no significant difference between the SPLIT and TOTAL groups for any of these measurements.

Lasevicius and colleagues (149) concluded that their data showed that training a muscle group 2 or 3 times a week resulted in similar increases in muscular strength and hypertrophy in young adult resistance trained males. Nonetheless, they claimed that the small but potentially meaningful difference in effect sizes (0.31 to 0.39), which favored the SPLIT group, indicated a potential benefit of training muscle groups 2x/week versus 3x/week. However, the absolute differences in muscle thickness between groups in all the assessed muscle groups showed a statistical improvement that ranged from only 1 to 2 mm.

Bartolomei and colleagues (2020)

Bartolomei and colleagues (150) randomly assigned 21 young adult males with at least 3 years resistance training experience to perform 26 upper and lower body resistance exercises either in a total body routine (TB: all the major muscle groups at each of 4 sessions/week) or a split routine (SR: different muscle groups at each of the 4 sessions/week) for 10 weeks. The total volume of exercise was similar for the SR and TB protocols. Both groups performed 5 sets of 6 reps before reaching *volitional failure*, which the authors described as one repetition in reserve (1 RIR). There was no control group. An investigator, who was blinded to the allocated protocols, used ultrasonography to assess muscle thickness of the pectoralis major, trapezius and vastus lateralis.

The TB and SR groups significantly increased muscle thickness and there was no significant difference in 2 (pectoralis and trapezius) out of the 3 muscle groups (150). Only the vastus lateralis showed a significantly greater increase with the SR protocol, but the difference between SR and TB routines was only 1.1 mm. Bartolomei and colleagues concluded that SR training was more conducive in simulating muscle growth; however for the majority of outcomes, their own data failed to support that claim.

A sidebar to the study by Bartolomei and colleagues (150) is that they noted the set end point in their training protocol was one repetition in reserve (1RIR) and to justify their protocol they cited a reference by Zourdos and colleagues (151). It is beyond the focus of this Critical Commentary to describe in detail how Zourdos and colleagues concluded that there was an inverse relationship between average repetition velocity and the rating of perceived exertion/RIR for the squat exercise (e.g., RPE = 10, RIR = 0; RPE = 9, RIR = 1, etc.). They believed that this was a practical way to regulate daily training loads and that appropriate daily loads are paramount for optimal adaptations. It should be recognized however, that their conclusions were based on the mythical concepts of so-called *periodization, autoregulation training,* the *Tendo* device they used to estimate average rep velocity, and the highly subjective rating of *perceived exertion*, which relies on subjective feedback from the trainer and trainee. These absurd training concepts are beyond the scope of any rational interpretation.

Thomas and Burns (2016)
Thomas and Burns (152) assigned 19 middle-aged males and females, who had been resistance training ~3x/week for ~4 years, to a high frequency training group (3 sets per muscle group, 3x/week) or a low frequency split routine group (9 sets per muscle group at each of 3 sessions per week). Both groups performed a total of 9 sets per muscle group each week with an 8-12RM to *momentary muscle failure*. The researchers used DEXA to determine lean body mass but did not indicate if the assessors were blinded to the training frequency assignments.

The authors claimed that there was no significant difference between the low (0.99 kg) and high frequency (1.06 kg) training groups for the gains in lean body mass (152). Interestingly, Thomas and Burns stated this 2 times in their Abstract, 2 times in their Results section, and 6 times in their Discussion section, using the phrases *similar improvements*, and *almost identical increases* (1.9% and 2.0% for high and low frequency training, respectively. However, they noted only once in their Results section that neither group significantly improved lean body mass (p>.05) and referred to their Figure 1 (p. 164). Statistically, neither group increased lean body mass. The authors' misleading statements somehow survived the so-called rigors of the peer-review and editorial processes.

Zaroni and colleagues (2019)
Zaroni and colleagues (153) randomly assigned 18 young adult males (~6 years resistance training experienced) to train either with a SPLIT routine (multiple exercises for a specific muscle group each session) or a TOTAL routine (1 exercise for each muscle group each session). Both groups trained 5x/week for 8 weeks and performed 3 sets of 10-12RM (*momentary concentric failure*) for each of 5 exercises/session.

Ultrasound assessments (not blinded) revealed a significant increase in elbow flexor, triceps and vastus lateralis thickness in both groups and the increase was significantly greater in the TOTAL group for the elbow flexors (p = 0.009) and vastus lateralis (p = 0.021). The increase in triceps thickness was not significantly different (p = .227) between the TOTAL and SPLIT routines.

***Frequency of Training Section Summary*:** Most of the studies and extensive reviews reported no significant difference in muscle thickness as a result of different resistance training frequencies. In a very few exceptions, the differences were reported as statistically significant but ranged from only 0.2 mm to 2.0 mm. One study that equated high volume training between groups reported a significantly greater hypertrophic response with a total body routine compared with a split routine in 2 out of 3 muscle groups.

Text Recycling (Self-plagiarism)
When authors reproduce their own verbatim text from a previous publication into a new publication without putting it in quotations and referencing the original source, it is known as text recycling (154-158). Although there are some academics who find text recycling acceptable (159), others believe that text recycling is simply a euphemism for self-plagiarism (154) and is indicative of the author's intellectual laziness (157, 160-161). Recycling text is misleading, unacceptable, deceives readers, is unethical (154, 157) and should not be tolerated by the academic community (161).

Because most publishers have copyright infringement laws, text recycling is perhaps fraudulent as well (159). For example, the *Journal of Strength & Conditioning Research* specifies in their Assignment of Copyright that all the authors convey ownership of their manuscript to the Journal and shall not be published elsewhere in whole or in part. Nevertheless, self-plagiarism is rampart in many of the studies cited in this Critical Commentary.

Schoenfeld and colleagues copied several sentences verbatim from the methodology section of one study (141, p. 1823-4), to another study (122, p. 1807), to another study (73, p. 443-4) and so on. Schoenfeld and colleagues also recycled approximately three-quarters of the section on muscle thickness methodology from one study (58, p. 717) to another (163, p. 708-9. Even as a co-author, Schoenfeld recycled—or interchanged—text from Brigatto and colleagues (136, p. 2109) to Zaroni and colleagues (152, p. S145-6) without quoting or referencing the source. Schoenfeld or his co-authors could have clearly noted that portions of their methodology sections had been previously described and they should have properly referenced those publications (162). Each of the authors on those studies is equally responsible for these ethical violations. Readers can decide if all that blatant ethical misbehavior raises suspicion about other ethical concerns when those authors and journals report their data and results.

Author Contribution
Tarkang and colleagues (164) have stated that authorship confers career benefits for tenure or promotion and consequently each author's contribution to an article should be specified. For example, authors should specify if they participated in the writing or technical editing of the manuscript and/or contributed to playing an active role in carrying out the study. This raises the question of how ethical is it to put your name on a training study that was carried out in another country without disclosing what your possible contribution was to that research. Readers can access several of the previously discussed studies and easily find many examples of the authors' failure to disclose each author's contribution to the study.

Authorship implies responsibility for the published manuscript, that each author will be held accountable for the contents of that publication, and that the corresponding author is responsible for identifying the contribution of each author, which could eliminate some of the ambiguity involving contributions (165). Those who provide only writing assistance, language and technical editing, or proofreading do not qualify as a contributing author (165).

In the previously discussed study by Schoenfeld and colleagues (122), the authors stated that all the training sessions were supervised by the research team. Ten out of the

13 authors were from Oklahoma; Schoenfeld, Henselmans and Krieger are from New York, the Netherlands and Washington, respectively. Readers should be curiously questioning if those three authors (including Schoenfeld as the lead author and Krieger as the last author) traveled to Oklahoma to participate in the supervision of the training, or did 12 of the co-authors travel to the Netherlands for 8 weeks to supervise the study. Schoenfeld and colleagues did not indicate who performed the ultrasound or if that assessor was blinded to the specific training protocol. Schoenfeld and colleagues failed to specify the contributions of each author.

Contreas is the 2nd author listed on the current previously discussed study by Schoenfeld and colleagues (77). In an a YouTube video interview with Schoenfeld entitled the B&B Connection, episode 9 (https://www.youtube.com/watch?v=UHpRMNddFEc), Contreras revealed that after Schoenfeld does all the work, he (Contreas) looks over an article, gives Schoenfeld his *two cents*, and then stated that Schoenfeld lists him as an author on the publication. Because Contreas is listed as one of Schoenfeld's co-authors on several publications in this Critical Commentary, his statement raises the question of what exactly is his definition of *two cents* and how many times has this practice occurred? Schoenfeld obviously finds this practice acceptable and that may consequently raise the question concerning the extent of Schoenfeld's contribution to studies that were performed in other countries.

The 3rd author listed in the study by Schoenfeld and colleagues (77) is Krieger, who was the statistician for many of Schoenfeld's reviews and studies. In a YouTube video interview with Jacob Schepis that was ironically entitled *The Science of Size* (https://www.youtube.com/watch?v=bpnoTL6IsG8&feature=youtube), Krieger claimed that the existing data *definitively* showed that volume is the primary driver for muscle hypertrophy and that every time a trainee doubles the number of weekly sets per exercise, the hypertrophic response increases by 50%. He repeatedly emphasized his opinion regarding the very strong relationship between volume and hypertrophy, and gave an example of how 30-40 sets/week will yield better results than 20 sets per week. However, Krieger failed to cite any evidence (science) to support his opinion and again publicly revealed his bias for higher volume training. Despite all the impressive robust muscularity displayed in that video, Krieger sacrificed his status as an unbiased objective researcher and statistician. These issues with his high volume opinions raise the question of blinding a statistician who has an inherent bias in favor of one training protocol over another such as high volume (multiple sets) and muscle hypertrophy. Krieger was the statistician for many of Schoenfelds publications and Schoenfeld has never indicated that Krieger was blinded to the training intervention in any of their studies or meta-analyses.

Terminology

Many researchers failed to use the correct terminology when describing human body segments and movements. There are no such words as *bicep* or *tricep*; the muscles are the biceps and triceps. And they are not muscles of the *upper arm*; they are arm muscles. The biceps muscle consists of the short and long heads; the triceps have three heads: long, lateral and medial heads (166). The *arm* is the body segment of the upper extremity between the shoulder and elbow joint, and the forearm—not the *lower arm*—is the segment from the elbow to the wrist joint. Similarly, the segment of the lower extremity between the hip and knee joint is the thigh, and the leg is the segment from the knee to the ankle joint (167). With blood flow restricted knee extension exercise, a tourniquet is placed around the upper thigh—not the upper leg. The aforementioned study by Franchi and colleagues (87) used the word *leg* instead of the correct word *thigh* six times in one short paragraph (p. 847).

Muscles and limbs cannot be flexed or extended—only joints can be flexed or extended (167); that is, shortening muscle actions of the biceps or triceps do not flex or extend the forearm, they flex or extend the elbow. Free weights, plate loading and selectorized machines use a mass to provide resistance (discs or plates), which remains *constant* throughout the range of motion; and they all provide an external resistive torque, which is *variable* throughout the range of motion (168-169).

These are common errors in the uninformed general population but should be unacceptable by the self-proclaimed experts in resistance training. It may appear to be nitpicking but if researchers are not able to correctly describe body segments, muscle actions, joint movements and exercise equipment, they could hardly be expected to standardize training protocols and control all potential confounding variables.

Self-Plagiarism / Author Contribution / Terminology Sections Summary: Failure to fully reveal author contributions, continuing to self-plagiarize sections of a narrative, or simply being incapable of correctly describing muscles and joint movements may not necessarily negate an entire study. However, those transgressions certainly question the author's other ethical and educational standards within their resistance training studies or reviews.

Pseudoscience

Howe and colleagues (2017)

There is a considerable downside to all the pseudoscience in the so-called peer-reviewed resistance training literature. Coaches, trainers, trainees, and other researchers often use the misinformation in these publications as a guideline for their own recommendations and training protocols. For example, Howe and colleagues (170) recently wrote a narrative review on the training principles for enhancing muscle hypertrophy. These presumably well-intentioned authors stated that the purpose of their review was to present an overview of the current, effective, evidence-based resistance training literature. Many of the references they cited, which have been discussed in this Critical Commentary, were probably an attempt to confirm their beliefs and opinions. However, many of their references are devoid of any validity or merit. Therefore, although it is beyond the scope of this Critical Commentary to rehash all the references that Howe and colleagues cited, a few points are worth noting.

When Howe and colleagues (170) cited a study that did not support their opinion about a specific training variable, they claimed that the study was probably underpowered. For example, in their Load section they suggested that type II muscle fibers are stimulated to a greater degree when exposed to a heavier load and cited a training study (171). Mitchell and colleagues (171) randomly assigned the quadriceps in a counterbalanced design to 2 of out 3 training protocols: 1 x 80% 1RM, 3 x 80% 1RM or 3 x 30% 1RM. All the trainees (18 previously untrained young adult males) performed each unilateral set of knee extensions (12 thighs in each protocol) to *voluntary failure* 3x/week for 10 weeks. The researchers assessed quadriceps muscle volume with MRI and quantified biopsied muscle fiber area with a myofibrillar ATPase histochemistry procedure. The authors did not indicate if any of their assessors were blinded to the training protocols.

Quadriceps muscle volume significantly increased in the 3 x 30% 1RM group (6.0%) and in the 3 x 80% 1RM group (6.8%), 95 cm^3 and 104 cm^3, respectively, with no significant difference in muscle volume as a result of training with a lighter or heavier load (171). More specifically, type II vastus lateralis fiber area increased 16% with the heavier load training and 18% with the lighter load. Mitchell and colleagues concluded that both type I and II muscle fibers increased significantly with heavier and lighter load training, which they noted was suggestive that both fiber types were recruited to a similar extent. Howe and colleagues suggested that in their opinion the study was underpowered, which they claimed was the reason for the lack of any statistical difference between groups. They noted also that in another article, Ogborn and Schoenfeld (172) expressed a similar opinion. However, Mitchell and colleagues, who conducted and reported the study, never mentioned the possibility that their study was underpowered. It was simply an opinion by other authors who perhaps were disappointed that the study failed to support their heavier-is-better belief regarding muscle hypertrophy.

Howe and colleagues (170) cited a meta-analysis by Schoenfeld and colleagues (59) regarding training lighter (<60% 1RM) and heavier loads (>60% 1RM). Note: The definitions by Schoenfeld and colleagues for lighter and heavier loads were actually ≤60% 1RM and ≥65% 1RM, respectively. Howe and colleagues claimed, as did Schoenfeld and colleagues, that there was a *nonsignificant trend* in favor of higher load training (Note: Readers can refer to the discussion of *trends* in a previous critique of the meta-analysis in this Critical Commentary). Howe and colleagues noted that the failure to find a significant post-training difference in hypertrophy was because of the low number of studies that reported the effects of different loads on muscle hypertrophy.

In their Frequency section, Howe and colleagues (170) noted that Dankel and colleagues (173) suggested that higher training frequencies may be more beneficial for enhancing muscle hypertrophy in advanced trainees. They cited the article three times in one paragraph. Dankel and colleagues hypothesized that greater frequencies may be more beneficial and used the words *hypothetical, hypothetically, hypothesis or hypothesized* well over a dozen times in their Current Opinion article; and to their credit, stated *in our opinion* more than once in their narrative. However, the only resistance training studies that Dankel and colleagues cited to support a greater frequency of training in advanced trainees were Schoenfeld and colleagues (141) and Hakkinen and Kallinen (174). As previously discussed in this Critical Commentary, Schoenfeld and colleagues (141) claimed that there was a superior hypertrophic benefit to higher frequency training; however, their own study reported only 1.1 mm difference in muscle thickness in just 1 of the 3 muscle groups assessed; that is, 2 out of 3 muscle groups showed no significant difference in muscle thickness.

Hakkinen and Kallinen (174) assigned ten adult females (~29 years) with at least 2-3 years of resistance training experience to perform 10 sets of squats with 70-100 % 1RM (1-3 repetitions/set, ~20 total repetitions) and four sets of leg press or knee extension exercises with 60-70 % 1RM (5-10 repetitions/set, ~30 reps) either in one session a day 3x/week or two sessions a day 3x/week for three weeks. Half the group started with the two sessions/day protocol for the first three weeks followed by the one session/day protocol for the next three weeks; the other half of the group followed the same procedure in reverse order. CT scans revealed a significant increase in quadriceps femoris cross-sectional area (~4 %) when the training protocol was divided into two sessions/day. There was no significant change when all the exercises (sets) were performed in one session, but the researchers did not report those data. They did not indicate what level of effort (e.g., RMs or muscular failure) the athletes required to perform the specified number of squats (1-3 reps/set) with 70-100% 1RM, or if the CT assessors were blinded to the training protocols. However, Hakkinen and Kallinen (174) did comment on their own limitations of assessing these changes.

The practical application of the training protocol used by Hakkinen and Kallinen—regardless of how the exercises are distributed (1 or 2 sessions/day)—is questionable at best, even in advanced trainees (174). For, example, if trainees perform 14 sets with one minute rest between sets and exercises for each of only six muscles groups 3x/week, it would require approximately 3 hours per day and 9 hours per week.

After hypothesizing throughout their opinion article, it is puzzling that this group of credible researchers (173) cited these two studies in an attempt to support their hypothesis; one with questionable results (141) and the other from more than a quarter-century ago that adopted an absurd training protocol (174), which Hakkinen and Kallinen ironically described as *normal* training. Why not state the logic behind their hypothesis, which Dankel and colleagues did clearly and distinctively, present it as just a hypothesis, and test the hypothesis in advanced trainees?

In their Conclusion section Howe and colleagues (170) claimed that high volumes and greater frequency of training are necessary to maximize muscle hypertrophy in advanced trainees. They failed to support those statements and many of their recommended training protocols with any robust evidence.

Krzysztofik and colleagues (2019)

In furtherance of the pseudoscience that has infiltrated the resistance training literature, Krzysztofik and colleagues (175) recently wrote a review on training techniques and methods to maximize muscle hypertrophy in advanced resistance trainees. A detailed response to most of their unsupported recommendations would be redundant to what has been shown in this Critical Commentary; however, one example is worth mentioning.

Krzysztofik and colleagues (175) claimed that a critical variable that influences training outcomes is what they described as the *evidenced based* dose-response relationship between resistance training volume and the degree of muscle hypertrophy. They claimed specifically that higher training volumes, compared with lower training volumes, are associated with greater hypertrophic responses in previously untrained and experienced trainees. They cited four references (57, 77, 90, 176) in their attempt to support those claims. As previously noted in this Critical Commentary, the three references by Schoenfeld and colleagues (57, 77, 90) failed to demonstrate any valid *evidenced based* dose-response relationship.

The study by Haun and colleagues (176) compared the effects of different nutritional supplements during what the authors accurately described as *extreme-volume* resistance training (4 exercises, 3x/week, 48 sets, 32 sets and 48 sets, Mon, Wed and Fri, respectively). They did not compare different volumes of training. Consequently, none of those references supported the dose-response claim by Krzysztofik and colleagues (175).

Krzysztofik and colleagues (175) noted in their Conclusions section that for maximal muscle growth, the *intensity of effort* should be between 60-80% 1RM with subsequent increases in training volume. They cited only the previously discussed study by Schoenfeld and colleagues (77). However if each set were completed to the point of—or near—muscular failure, wouldn't the trainees all be performing with a similar *intensity of effort* (i.e., maximal effort) with 60, 70, 80% 1RM or any percent 1RM between 60 and 80? Do Krzysztofik and colleagues actually believe that the *intensity of effort* would be different on the last rep of a set with 70 or 80% 1RM?

Damas and colleagues (2019)

In stark contrast to the unsupported opinions of Howe and colleagues (171) and Krzysztofik and colleagues (175), a recent study by Damas and colleagues (177) could put to rest some of the absurd obsessions with constantly manipulating these resistance training variables. Damas and colleagues (177) recruited 20 resistance trained young adult males with an average 2.5 years of experience to perform unilateral leg press and knee extension exercises in a within-subjects unilateral designed study (40 experimental limbs) 2x/week for 8 weeks. One limb was randomly assigned to a standard resistance training protocol of 4 sets of 9-12 repetitions for each exercise with 2 min interset rest (CON protocol). The trainees varied the protocol for their contralateral limb (VAR protocol) at each session with one of four training variables (load, sets, muscle action or interset rest; a, b, c or d, respectively): a) 4 sets of 25-30 reps for each exercise with 2 min interset rest, b) 6 sets of 9-12 reps for each exercise with 2 min interset rest, c) 4 sets of each exercise with 10 eccentric-only muscle actions at 110% of the concentric load with 2 min interset rest, or d) 4 sets of 9-12 reps for each exercise with 4 min interset rest. The participants completed the 4 varied protocols in a counterbalanced randomized manner 4 times during the 8-week program thereby adhering to a systematic and constant manipulation of variables a, b, c and d with the VAR limb. All the exercises for both limbs were executed to *concentric failure*. The researchers used ultrasound to measure bilateral vastus lateralis cross sectional area (CSA) pre- to post training. They collected 4 days of saliva samples (from 48 hours before to 48 hours after the 17th training session) and used bilateral vastus lateralis percutaneous needle biopsies to assess integrated myofibrillar protein synthesis (MyoPS). They did not indicate if the assessors for these evaluations were blinded to the training protocols.

The CSA of the vastus lateralis was similar in the CON and VAR limbs before the study commenced and significantly increased in the CON (~7.6%) and VAR (~7.4%) limbs after the 8-week program (177). There was no significant difference in the hypertrophic response between limbs. The interindividual increase in CSA ranged from 2.9 to 13.7% in the CON limb and from 2.4 to 13.5% in the VAR limb. There was a mean difference of only 0.91% between limbs despite a significantly greater ($P<0.0001$) 8-week total training volume (sets x reps x load) in the VAR limb. Between-subject variability was ~42 times greater than within-subject variability.

The integrated 0-48 hour increase in MyoPS was significantly greater ($P<0.0001$) with a mean interindividual difference in contralateral limbs of only 0.08% (maximal difference of 0.21%), and a between-subject difference of ~3.5% and ~3.1% for CON and VAR, respectively (177). The between-subject variability was ~41 times greater than the intra-subject variability for MyoPS responses to resistance exercise. However as previously noted, these differences did not result in a greater hypertrophic response in the VAR limb.

Damas and colleagues (177) concluded that the results of this study supported their hypothesis that the type of resistance training protocol would not affect the significantly high between-subject variability in hypertrophy for these previously trained males, and that the trainees' individual intrinsic predisposition to hypertrophy determined the main source of variability in the hypertrophic responses. Damas and colleagues stated that the extrinsic manipulation of common resistance training variables such as load, sets, type of muscle action, and interset rest had no influence on the degree of muscle hypertrophy. They believed that as long as exercises are performed to or close to concentric failure, it is not necessary to manipulate the training protocol to avoid a plateau in hypertrophy—even in previously well-trained males. These conclusions by Damas and colleagues have been repeatedly and strongly supported in this Critical Commentary.

***Pseudoscience Section Summary**:* Howe and colleagues and Krzysztofik and colleagues both claimed that the purpose of their review was to present an overview of evidence-based resistance training literature. However, it was actually very similar to most reviews on resistance training—just an overview of unsupported opinions.

Free Weights versus Machines

Schwanbeck and colleagues (2020)

It is commonly—and perhaps almost universally—believed, especially by the *bros*, that free weight training is superior to machine training for increasing muscle mass. In an extensive review reporting the effect of free weights and machines on strength gains, Carpinelli (169) concluded that there was an absence of evidence to support the superiority for either mode of exercise to increase muscular strength, and stated that he was not aware of any resistance training studies to suggest that free weights are superior to machines for enhancing muscle hypertrophy. Recently, Schwanbeck and colleagues (178) hypothesized that experienced trainees who exercised exclusively with free weights would produce a greater increase in muscle hypertrophy compared with training exclusively with machines.

Schwanbeck and colleagues (178) randomly assigned 46 young adult females (n = 26) and males (n = 20), who had greater than 2 years of resistance training experience, to either a free weight or machine training protocol. Both groups trained with a split routine 4 days/week: Day 1; chest, back and triceps; Day 2: legs [and thighs], shoulders and biceps. Both training days were followed by a rest day and they repeated this sequence twice each week for 8 weeks. Free Weights group Day 1: flat bench press, incline bench press, bent over row, chin-ups, supine elbow extension, and dumbbell kickbacks; Day 2: squat, deadlift, lunge, calf-raise, shoulder press, lateral raise, cambered bar curls, and preacher curl exercises. Machine group Day 1: Smith machine bench press and incline bench press, seated row, lat pulldown, triceps press-down, and rope press-down; Day 2: Smith machine squats, seated knee extension and knee flexion, calf-raise, shoulder press, lateral raise, and biceps curl exercises.

All the participants completed 4 sets of 8-10 reps, 4 sets of 6-8 reps, and 3 sets of 4-5 reps, weeks 1-3 (112 sets/week), 4-6 (112 sets/week), and 7-8 (84 sets/week), respectively (178). In order to achieve a progressive overload, they increased the resistance throughout the training program to comply with the designated number of repetitions. Approximately 25% of males and 19% of females dropped out of the study, and the authors noted that the reason for the relatively high dropout was the time commitment required to complete all the sets. The researchers estimated lean body tissue with air displacement plethysmography (Bod Pod) and used ultrasound to measure thickness of the biceps and quadriceps. They did not indicate if the assessors were blinded to the mode of exercise training.

Lean tissue mass did not change significantly in either the Free Weight or Machine training group for males or females (178). Biceps and quadriceps thickness significantly increased in both training groups (males 2-4 mm and females 1-3 mm), with no significant difference between the Free Weight and Machine groups. Schwanbeck and colleagues concluded that significant increases were achieved by training with only with free weights or training only with machines, and the mode of training did not influence the degree of muscle hypertrophy.

Free Weights versus Machines Section Summary

There is no evidence to suggest that either free weight or machine resistance training is superior for increasing muscle mass in any demographic.

Conclusions

Some readers may conclude that this Critical Commentary is argumentative at best. However, they may conclude also that the so-called *peer review system* for resistance training failed miserably to challenge the nonsense in many of these inclusive studies and reviews.

It is not possible to accurately compare groups within a given study when more than one variable is manipulated or all potential confounding variables are not controlled. There is no standardization regarding the stimulus for muscle hypertrophy and consequently there are no valid comparisons of results among training studies. Most importantly, the majority of training studies (with a few rare exceptions) failed to blind the assessors. Obviously, the authors and reviewers of those studies do not believe that assessor blinding should be a requirement for non-bias reporting of outcomes.

Based on all the aforementioned section summaries, this Critical Commentary concludes that there is no credible evidence for any measureable or practical intra-individual difference in muscle hypertrophy as a result of the obsessive manipulation of resistance training variables such as the number of sets, amount of resistance (load), number of repetitions, volume of exercise, interset rest intervals, repetition duration (time under tension), frequency of training, whole body or split routines, periodized and non-periodized routines, free weights and machines, etc. Based on the currently available evidence, it may be logically concluded that the few very small differences in the individual hypertrophic response to resistance training do not constitute any meaningful aesthetic differences. Any intra- or inter-individual differences in muscle hypertrophy in response to manipulating different resistance training protocols probably are genetically dictated.

Disclosure

Conflicts of interest can be solely the result of a specific intellectual belief that might bias or even appear to bias results. It also may influence academic promotion. Failure to disclose an actual conflict of interest or even the perception of a conflict of interest is a form of academic misconduct (179).

The author declares absolutely no conflicts of interest: no commercial or personal websites, podcasts, and no internet videos; no online, teleconference or telephone consultation; has no social media accounts (except the academic media network ResearchGate), and has never sold or endorsed any product such as books, audio or video discs, nutritional supplements, apparel, equipment, workshops, conferences, or certifications. His opinion is that any of these activities present a potential compelling conflict of interest, and although they are rarely disclosed, should always be reported. He has never had an interest in achieving or maintaining tenure in any academic institution and has never considered himself to be a *hypertrophy specialist* or one of the *bros*.

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