

CHALLENGING THE AMERICAN COLLEGE OF SPORTS MEDICINE 2009 POSITION STAND ON RESISTANCE TRAINING

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Abstract

The new ACSM Position Stand on resistance training is very similar to the 2002 Position Stand, which based the majority of its claims and recommendations on misinterpretation of resistance training studies and selective referencing. The addition of a few new references published since the previous Position Stand was supposed to enhance the credibility of the ACSM's recommendations for resistance training. Unfortunately, the ACSM's new Position Stand contains all the flaws that were pervasive in their previous Position Stand; that is, the authors cited references that failed to support their opinions and recommendations.

Key words: *muscular strength, load, volume, exercises*

Introduction

The American College of Sports Medicine (ACSM) released a new Position Stand entitled *Progression Models in Resistance Training for Healthy Adults* (1). The 2009 Position Stand replaces the 2002 ACSM Position Stand on resistance training (2) and has some additional references that allegedly serve "...to bolster the scientific integrity of the RT [resistance training] knowledge base" (p.687). However, the failure of the ACSM to support their claims and recommendations with resistance training studies is pervasive throughout the 2009 Position Stand, as it was in the 2002 Position Stand (3).

The comments below are several specific examples from six sub-sections (Loading, Volume, Exercise Selection, Free Weights and Machines, Exercise Order, and Rest Periods) of one primary section entitled *Muscular Strength*. There are ten primary sections in the new Position Stand (1). The rationale for choosing these six sub-sections is that they contain a few new references that were not cited in the 2002 Position Stand (2). Nevertheless, these examples are typical of what is systemic throughout the Position Stand; that is, many of the references cited failed to support the ACSM's claims or recommendations.

Loading

The authors of the Position Stand (1) claimed that at least 80% 1RM is required to produce neural adaptations in experienced lifters (p.690). A 1RM is the amount of resistance that can be lifted for only one repetition. They cited one reference (4) to support that claim. Hakkinen and colleagues (4) trained the knee

extensors with the barbell squat exercise in 11 young males three times a week for 24 weeks. The amount of resistance varied every four weeks (70-80%, 80-90%, 80-110%, 70-90%, 80-115%, and 85-120% 1RM, respectively). Electromyographic activity was measured during maximal knee extensor muscle actions on a dynamometer. Although maximal electromyographic activity significantly increased when the resistance was greater than 80% 1RM (weeks 4-12), there was no significant difference between the pre-training and post-training mean maximal electromyographic activity in the three muscles tested (rectus femoris, vastus medialis and vastus lateralis).

Hakkinen and colleagues (4) speculated that strength gains (~27% isometric knee extension force) were accompanied by an increase in neural activation during very intense training. However, there was no data showing the strength gains during each four-week cycle and, most importantly, no data for the 1RM squat, which was measured every four weeks. There was only one training group and consequently no random assignment of subjects to train at different percentages of the 1RM squat (e.g., 70% 1RM versus 80% 1RM) or a specific range of RM (e.g., 3-6RM versus 7-10RM). Hakkinen and colleagues' Figure 2 (p.577) depicted a progressive increase in maximal isometric force during the first 20 weeks of training. However, this reported increase does not rule out the confounding variable of a typical increase in force production with the duration (20 weeks) of training. Therefore, the increase in force during the first 20 weeks of training may have no relationship to the amount of resistance used in training.

Hakkinen and colleagues (4) noted that the number of repetitions varied from one to 10 per set but they did not report the number of repetitions at each percent 1RM. This was the only information regarding the number of repetitions that Hakkinen and colleagues provided. Because motor unit activation is dependent on the degree of effort (5) and was not reported by Hakkinen and colleagues, the level of motor unit activation during the 24 weeks of squat training is unknown. Consequently, Hakkinen and colleagues did not report enough information in this study to support the claim in the Position Stand that at least 80% 1RM is required to produce neural adaptations in experienced trainees.

The Position Stand (1) also claimed that 1-6RM loads were most conducive to increasing maximal strength (p.690). The authors cited two training studies (6-7) in an attempt to support their claim. Berger (6) trained 199 male college students three times a week for 12 weeks. They performed one set of the barbell bench press exercise for 2RM, 4RM, 6RM, 8RM, 10RM, or 12RM. Berger's Table 2 (p.337) showed that the increase in 1RM bench press was significantly greater for the 4RM, 6RM, and 8RM groups compared with the 2RM group, and significantly greater in the 8RM group compared with the 2RM, 10RM and 12RM groups. There was no significant difference in strength gains among the 4RM, 6RM and 8RM groups, nor between the 2RM group (heaviest resistance) and the 10RM or 12RM groups (lightest resistance).

O'Shea (7) trained 30 previously untrained male college students who performed barbell squats three times a week for six weeks. They used one of three repetition protocols: 2-3RM, 5-6RM or 9-10RM. There was a significant increase in 1RM barbell squat, static strength on a lower-body dynamometer, and thigh girth. However, there was no significant difference among the groups for any of the outcomes as a result of training with 2-3RM, 5-6RM, or 9-10RM.

Heavier resistance did not produce greater strength gains in either of these studies (6-7). Therefore, these resistance training studies, which were cited by the authors of the Position Stand (1), failed to support their claim that 1-6RM loads produce superior strength gains.

The Position Stand (1) claimed that 80% 1RM produced the largest effect size for strength gains in trained subjects. They also claimed that 85% 1RM was most effective for athletes (p.690). The authors did not explain how they classified *trained* subjects, which should have been a requisite when attempting to differentiate from those subjects whom they classified as *athletes*. The Position Stand cited two meta-analyses (8-9) in an attempt to support those claims. The meta-analysis by Rhea and colleagues (8) reported that training with 80% 1RM resulted in an effect size

of 1.8 (E.S. = 1.8), which was almost three times larger than training with 85% 1RM (E.S. = 0.65). The reason that such a small difference in resistance (80% 1RM compared with 85% 1RM) resulted in such a large difference in strength gains was not addressed by Rhea and colleagues. In other words, why would training with a slightly lighter resistance for a couple of extra repetitions produce such markedly superior strength gains? There is no known physiological hypothesis to explain such large differences in outcomes as a result of such small differences in resistance. Consequently, the conclusions of Rhea and colleagues and the Position Stand have no logical foundation and have no practical application to resistance training.

The meta-analysis by Peterson and colleagues (9) reported that the effect size for strength gains in athletes was almost double as a result of training with 85% 1RM (E.S. = 1.12) compared with training with 80% 1RM (E.S. = 0.57). They also claimed that training with 75% 1RM (E.S. = 0.73) was 10 times as effective as training with 70% 1RM (E.S. = 0.07). For example, if the 1RM bench press is 100kg and an individual trains to muscular fatigue with a 75 kg barbell, the strength gains would be (according to Peterson and colleagues) 10 times greater than training to muscular fatigue with a 70kg barbell. In addition, their data (E.S. = 0.07) erroneously suggest that training with 70% 1RM to muscular fatigue has basically no effect on strength gains.

Most of the studies included in the meta-analysis by Peterson and colleagues (9) did not have a control group. The lack of a control group required the use of a pooled standard deviation as opposed to the pre-training standard deviation employed by Peterson and colleagues. This statistical error apparently was not questioned by the reviewers of the publishing journal or the authors and reviewers of the Position Stand (1).

As with the aforementioned meta-analysis by Rhea and colleagues (8), Peterson and colleagues (9) did not hypothesize how such a small difference in resistance could elicit such a large difference in strength gains. Readers should question how these data were simply accepted as valid evidence to support a concept that is bereft of any physiological explanation. Shouldn't it have occurred to the reviewers of the respective journals (*Medicine & Science in Sports & Exercise*, and the *Journal of Strength & Conditioning Research*) to challenge these data? And equally importantly, shouldn't the reviewers of the Position Stand (1) have challenged the claims?

Neither Rhea and colleagues (8) nor Peterson and colleagues (9) distinguished between *trained* subjects and *athletes*. They provided no explanation for differentiating between highly-motivated advanced trainees, whose goals are to attain the greatest strength

gains and muscular hypertrophy, and competitive collegiate or professional athletes—who may have very limited resistance training experience. For example, are competitive collegiate or professional basketball players *trained* individuals if they do not perform resistance training? As *athletes*, would they be classified differently in a meta-analysis if they regularly performed resistance training? Why would a 5% difference in resistance result in such dramatic differences in strength gains, regardless of whether the trainees were classified as *trained* or as *athletes*? The absence of an objective physiological difference between these arbitrarily defined groups precludes any conclusion about different responses to a specific intensity or volume of resistance training. The credibility of both these highly-flawed meta-analyses (8-9) has been previously refuted (10). Nevertheless, the authors of the Position Stand (1) cited Rhea and colleagues at least 12 times and Peterson and colleagues at least six times.

None of these training studies (4, 6-7) or meta-analyses (8-9) supports the claim in the Position Stand (1) that a heavier resistance produces superior strength gains. That claim is based on a misinterpretation of the size principle. Motor unit activation is dependent of the level of effort at the end of a set of repetitions—not the amount of resistance or percent 1RM (5).

Volume

The Position Stand (1) claimed that a meta-analysis of 37 studies reported that eight sets per muscle group produced the greatest effect size in athletes (p.690). They cited two references (9, 11) in attempt to support that claim. As previously discussed, the meta-analysis by Peterson and colleagues (9) does not meet scientific standards and has previously been refuted in detail (10).

Specifically related to the volume of exercise, Peterson and colleagues (9) claimed that the dose-response for resistance training in athletes differs from lesser-trained populations. One of the criteria for inclusion of studies in their meta-analysis was that study participants must have been competitive collegiate or professional athletes. However, at least nine of the studies included by Peterson and colleagues involved subjects who had not performed resistance training prior to the specific study, had no prior resistance training experience, or there was no indication of prior resistance training (see reference 10 for list of specific studies).

Peterson and colleagues (9) claimed that maximal strength gains were elicited as a result of performing eight sets per muscle group during each training session. However, they did not indicate how they coded the number of sets per muscle group and they did not indicate which muscles they were coding or explain the rationale for their choice.

Peterson and colleagues (9) also claimed that their data unequivocally demonstrate the added strength benefits of higher training volumes. However, their Table 2 (p.379) failed to support any pattern or continuum for the effectiveness of the number of sets per muscle group. The effect sizes for four, five, six and eight sets per muscle group were 0.90, 0.64, 0.68, and 1.22, respectively. In their Methods section (p.378) Peterson and colleagues claimed that an analysis of variance was used to compare differences in effect sizes. However, they did not report any statistical differences between effect sizes. In addition, their Table 2 (p.379) revealed that the mean effect size was 1.22 for eight sets per muscle group but that mean was generated from only six effect sizes. Unfortunately, they did not specify the source of those effect sizes or how many studies produced those data. They did not attempt to explain their reported pattern of a fluctuating mean effect size (high, low, high) as training volume increased. Peterson and colleagues concluded that their meta-analytic procedure showed a continuum of quantified strength increases that were elicited by a continuum of training intensities, frequencies, and volumes. They also claimed that their data unequivocally demonstrated the added benefits with higher training volumes compared with lower-volume training. In fact, because they failed to demonstrate a continuum of strength gains related to volume, their own data do not support their conclusions.

In the second reference cited in the Position Stand (1), Peterson and colleagues (11) reported no new data. It was merely a rehash of their previous meta-analysis (9).

Exercise Selection

The Position Stand (1) claimed that multiple-joint exercises are more effective than single-joint exercises for increasing muscular strength because a greater amount of resistance can be lifted (p.691). A similar claim was made in the previous Position Stand (2) and the same reference was cited, which was a review by Stone and colleagues (12). Stone and colleagues merely expressed their opinion—without any resistance training studies for support—about the superiority of multiple-joint exercises, and revealed their apparent misinterpretation of the size principle of motor unit activation. Inquisitive readers may find it difficult to find this review because it was incorrectly cited as the *National Strength & Conditioning Association Journal (NSCA J)* in both the 2002 and 2009 Position Stand. The name of the *NSCA J* was changed to *Strength & Conditioning* in 1994 and since 1999 has been known as *Strength & Conditioning Journal*.

The authors of the Position Stand (1) recommended that there should be an emphasis on multiple-joint exercises to maximize muscular strength in

novice, intermediate and advanced trainees (p.691). They cited 28 references to support that recommendation. However, none of those references reported that strength gains were greater with multiple-joint exercises nor did they compare single-joint and multiple-joint resistance training. Consequently, the claim for superiority of multiple-joint exercises in the Position Stand is not substantiated with any science.

Free Weights and Machines

The Position Stand (1) claimed: "...machine exercises have demonstrated less neural activation when matched for intensity for most comparisons to free weight exercises" (p.691). The authors cited one study (13) to support their claim. McCaw and Friday (13) tested five young resistance trained males who performed the bench press exercise with free weights (barbell) and a machine. Subjects performed several trials on both modalities with 60% and 80% of the respective 1RMs. Surface electromyographic activity was recorded for the triceps brachii, anterior deltoid, medial deltoid, pectoralis major, and biceps brachii. The only significant difference in electromyographic activity was greater neural activation in the anterior and medial deltoid during lifting and lowering the barbell with 60% 1RM. There was no significant difference in electromyographic activity for any of the five muscle groups when lifting or lowering 80% 1RM. McCaw and Friday concluded that the high individual variability in electromyographic activity in their subjects suggests that factors other than the mode of exercise (free weights or machines)—such as joint and muscle mechanics specific to the individual—are responsible for neural activity (muscle involvement) during a bench press. This study does not support the claim in the Position Stand that there is lower neural activation in most comparisons of machines and free weights. The authors of the Position Stand apparently selected one piece of data from this study, cited the study in an attempt to support their opinion regarding free weights, and ignored the overall results and conclusions of McCaw and Friday.

The Position Stand (1) also claimed: "unlike machines, free weights may result in a pattern of intra- and intermuscular coordination that mimics the movement requirements of a specific task" (p.691). There is no reference cited to support that opinion.

Exercise Order

The authors of the Position Stand (1) claimed that it is necessary to perform multiple-joint exercises early in a workout session in order to produce optimal strength gains (p.692). They cited one study (14) to support their claim. Spreuwenberg and colleagues recruited nine healthy young males who had approximately seven years of resistance training experience

to perform four sets of free weight squats with 85% 1RM during one visit to the laboratory. The same squat protocol (4 sets with 85% 1RM) was performed during another session but was executed at the end of a resistance training workout that consisted of three sets of 8-10RM for seven other lower-body and upper-body exercises. During the first set of the four sets of squats, there were significantly fewer repetitions performed when the squat was preceded by the other exercises compared with performing only the squat exercise (5.4 and 8.0 repetitions, respectively). However, there was no significant difference in the number of repetitions during the second, third, and fourth sets of squats. The difference in the number of repetitions was only during the first set of squats. Most importantly, the rating of perceived exertion (RPE) was not significantly different between the two experimental protocols. As previously noted, because the activation of motor units is dependent on the degree of effort and not the amount of resistance or number of repetitions (5), and the effort (RPE) was similar for both experimental protocols, one may infer that there was similar activation of motor units. Therefore, the comment by Spreuwenberg and colleagues that trainees should perform multiple-joint large muscle group exercises at the beginning of an exercise session to achieve maximal strength gains is without foundation. Their comment also reveals a misinterpretation of the size principle by Spreuwenberg and colleagues as well as by the authors of the Position Stand.

Rest Periods

The Position Stand (1) claimed that the amount of rest between sets and exercises significantly affects training adaptations (p.692). They cited two references (15-16) in an attempt to support their opinion. Pijnappels et al. (15) reported the association between lower body strength and the prevention of falls in elderly participants. They did not attempt to compare different rest intervals—probably because this was not a training study. Consequently, this reference does not support the claim in the Position Stand.

In the other reference cited, Robinson and colleagues (16) compared inter-set rest intervals of 180 seconds, 90 seconds and 30 seconds in moderately trained young males. There was no random assignment of groups and no control group. The increase in 1RM squat was significantly greater in the 180-second rest group (7%) compared with the 30-second rest group (2%). With the exception of circuit weight training, there are very few—if any—resistance training protocols that recommend rest intervals be limited to 30 seconds. In addition, the accuracy of determining the differences in the miniscule strength gains (5%) after performing five sets of 10RM squats two times a week for five weeks is questionable at best. More impor-

tantly, there was no significant difference in strength gains between the 180-second (7%) and 90-second rest groups (6%); that is, when comparing reasonable rest intervals, longer inter-set rest intervals did not produce superior strength gains.

The Position Stand (1) concluded: "However, most longitudinal training studies have shown greater strength increases with long versus short rest periods (e.g., 2-5 min vs. 30-40s)" (p.692). The authors cited three references (16-18). The study by Robinson et al. (16) has been previously discussed. Pincivero and colleagues (17) compared 40-second and 160-second inter-set rest intervals in previously untrained young participants who trained three times a week for four weeks. Pincivero and colleagues reported no significant difference between groups for 12 out of the 14 variables measured on a dynamometer and no significant difference in the functional performance measure. In the Results section (p. 231) they claimed that quadriceps average power and peak torque showed a significantly greater improvement in the longer rest group. However, the claim in their Results section regarding quadriceps torque is antithetical to the claim in their Conclusions section: "*It was also evident that isokinetic quadriceps torque improved after training, as did functional performance. These improvements however, do not appear to be affected by rest interval manipulation*" (p.234). Both of these studies (16-17) were the only references cited in an attempt to support the same opinion regarding rest intervals in the 2002 Position Stand on resistance training (2). They failed to support that opinion in 2002 and again in 2009.

The new reference in the 2009 Position Stand (1) is a training study by Ahtiainen and colleagues (18) that compared two-minute and five-minute inter-set rest intervals in 13 young males with 6.6 years of continuous resistance training. Ahtiainen and colleagues concluded: "*The present study shows that, in hypertrophic heavy-resistance exercise, the 2- vs. 5-minute length of rest periods between sets did not lead to systematic differences in the acute exercise-induced metabolic, hormonal, or neuromuscular responses. Furthermore, training-induced adaptations over the 3-month period in muscle mass and strength were similar in magnitude in both the short- and long-rest protocols*" (p.581). In addition, one important clinically significant aspect of this study was that seven out of the original 20 participants in this study had to drop out during the experimental period because of training-induced aches in the knees and back. It is highly questionable if 4-5 sets of squats and leg presses constitute a safe, effective resistance training protocol (*First Do No Harm*).

Because William Kraemer is a co-author of the study by Ahtiainen and colleagues (18) and a co-author of

the 2009 Position Stand (1), one should question how his study—which reported results that are antithetical to the claim in the Position Stand—was incorrectly cited by the authors for support, and why it was accepted as supporting evidence by the reviewers, the ACSM Pronouncements Committee, and the Editor-in-Chief of *Medicine & Science in Sports & Exercise*.

The Position Stand (1) also claimed (p.692) that it is important to note that inter-set rest is dependent on the complexity of the exercise. For example, the authors claimed that Olympic lifts require longer inter-set rest. There is no reference cited to support this opinion.

In a study published in 2008, Williardson & Burkett (19) trained young males who were consistently performing the squat exercise for a minimum of four years prior to the investigation with the primary purpose of increasing maximal strength and muscle mass. The participants were randomly assigned to a 2-minute or 4-minute inter-set rest interval, with both groups performing the same squat training protocol two times a week for 12 weeks. The 2-minute and 4-minute groups showed significant increases in squat strength. Williardson and Burkett concluded: "*The primary finding of this study was that squat strength gains were not significantly different between groups that rested 2 minutes or 4 minutes between sets*" (p.149). This study is curiously missing from the new Position Stand (1). One should question why one of the co-authors of the Position Stand (William Kraemer), who is the Editor-in-Chief of the *Journal of Strength and Conditioning Research* where the study by Williardson & Burkett was published, neglected to cite this study in the Position Stand. If this failure to cite contradictory evidence was intentional, it exposes a condition known as selective referencing.

Discussion

There were 139 references examined in the Critical Analysis of the 2002 ACSM Position Stand on resistance training (2). Only eight of these studies actually supported the claims in the Position Stand (3) and 16 other studies contained serious flaws in methodology or data. More importantly, 59 studies failed to support the claims and 56 studies that were not cited in the Position Stand actually refuted the claims or recommendations. This was not only a failure in the ACSM's writing and peer-review processes but was a misrepresentation of the studies conducted by dedicated researchers who devoted countless hours performing resistance training research; that is, their studies were being used—incorrectly—by the ACSM in an attempt to support their own opinions.

The complex resistance training recommendations in the 2009 Position Stand's (1) Table 2 (p.700) are based on the unsubstantiated opinion that the obsessive

manipulation and specific combinations of training variables such as loading (amount of resistance), the number of repetitions, number of sets, inter-set rest intervals, repetition duration, time under load, frequency of exercise, modality of exercise, order of exercise, and exercise selection (single or multiple joint) results in significantly different specific outcomes. Most resistance training studies do not support that opinion (3).

If people were to assume that the ACSM's recommendations in the Position Stand (1) have any validity (scientific support), they can actually calculate how many hours are required in the gym to attain or maintain the essential components of muscular fitness (strength, hypertrophy, power and endurance). Trainees would be required to spend a *minimum* of 20 hours per week performing resistance exercise (according to Table 2 in the Position Stand); that is, approximately five hours a day four times per week. This does not include the time required to improve or maintain aerobic capacity or engage in other forms of physical activity. Competitive athletes would have little time to practice their specific sport activity. Furthermore, the ACSM's recommendations in Table 2 should have been challenged by the reviewers of the Position Stand, the ACSM's Pronouncements Committee, and the editorial staff of *Medicine & Science in Sports & Exercise*.

Conclusions

It is important for readers to understand what is required for a clear, succinct specific refutation of an unsubstantiated opinion versus what is simply required to state an opinion. For example, the aforementioned claims in the Position Stand (1) regarding the Loading sub-section required only three sentences and approximately 80 words. To refute those claims required eight paragraphs consisting of over 1100 words. The time and effort involved to retrieve and peruse the references are much more difficult to estimate. It is beyond the scope of this review to address every reference in the Position Stand. That task was the obligation of the authors and reviewers, which was apparently unfulfilled. However, many of the same studies were cited in the 2002 Position Stand (2) and the validity of the ACSM's attempt to use these studies to support their opinions and recommendations has been previously challenged and refuted (3).

Science dictates that the burden of proof is on the writers of the Position Stand (1) to support their claims and recommendations with peer-reviewed resistance training studies. The challenge for the reviewers, members of the ACSM's Pronouncements Committee, and editorial staff was actually to read the Position Stand and see if any of the references cited support

the claims and recommendations. They all failed to meet these obligations.

An editorial by the current Editor-in-Chief (Andrew Young) of *Medicine & Science in Sports & Exercise* (20) emphatically stated that he will not consider letters criticizing the ACSM's process for deriving a *pronouncement* (e.g., a Position Stand). Because the ACSM Position Stands (1-2) are so bereft of any science (resistance training studies that actually support their claims and recommendations) and apparently not open to criticism (according to the Editor-in-Chief), there is very little expectation that the ACSM or its Position Stands will gain any respect from those who carefully read the studies and evaluate all the evidence.

The ACSM claims: "*Position Stands are based on solid research and scientific data and serve as a valued resource for professional organizations and governmental agencies*" (21). The ACSM also claims: "*A 'Position Stand' is developed when enough research has been completed to support the position on scientific grounds. An 'Opinion Statement' is developed when available scientific data do not permit the development of a formal position stand, but provide support for a given position on a crucial issue*" (22). Readers can decide on the validity of the ACSM's claims and recommendations and whether those claims and recommendations belong in a Position Stand supported by science or perhaps in an Opinion Statement supported by opinions.

Disclosure

In the interest of full disclosure, I was one of eight reviewers for the 2002 Position Stand (2). None of the other reviewers challenged a single reference. Two colleagues and I were designated as Reviewer #5 and we were removed from the review process after challenging many of the references. This remains a highly questionable ethical maneuver by the ACSM. In addition, I am the primary author of the critical analysis (3) of that Position Stand.

I sent similar comments and questions to the 26 ACSM members responsible for this highly-flawed 2009 Position Stand (1): The ACSM President and Executive Vice-President, Editor-in-Chief of *Medicine & Science in Sports & Exercise*, the seven Position Stand authors and five reviewers, and the 11 members of ACSM's Pronouncements Committee. The only response that I received was from the ACSM President (Mindy Millard-Stafford, Ph.D.): "The Position Stand represents a broad scientifically validated consensus that the College has determined will represent its current position on an issue." She failed to address any of the specific aforementioned issues.

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