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Review: Fitness and Training

A CRITICAL ANALYSIS OF THE SINGLE VERSUS MULTIPLE SET DEBATE

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ABSTRACT

Otto RM, Carpinelli RN. A Critical Analysis Of The Single Versus Multiple Set Debate. ***JEP****online* 2006;9(1):32-57. Several researchers have recently claimed that a series of meta-analyses *unequivocally* support the superiority of multiple sets for resistance training, and that they have ended the single versus multiple set debate. However, our critical analysis of these meta-analyses revealed numerous mathematical and statistical errors. In addition, their conclusions are illogical, inconsistent, and have no practical application to resistance training.

Key Words: Resistance Training, Sets, Effect Size, Meta-analysis, Critical Analysis

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INTRODUCTION

The National Strength and Conditioning Association (NSCA) recently declared the end of the single versus multiple set debate (1). The NSCA based its conclusion on a presentation by Dr. Matthew Rhea at the NSCA 2004 National Conference entitled *The Dose-Response for Strength Development: Scientific Evidence and Practical Applications*. Dr. Rhea claimed that narrative reviews have resulted in conflict and confusion, and that his three meta-analyses (2-4) were the resolution to the debate of single versus multiple sets. He proclaimed that support for multiple set programs is stronger than the correlation of smoking and lung cancer. According to the NSCA, Dr. Rhea's presentation was a part of strength and conditioning history (1).

Despite the NSCA's claim that the topic was no longer a debatable issue, Dr. Brent Alvar, who is Dr. Rhea's mentor and a co-author of all three meta-analyses (2-4), gave a similar presentation—using these three meta-analyses and a training study (5)—in a point/counterpoint debate on single versus multiple sets one year later at the NSCA 2005 National Conference. He claimed that narrative reviews were unscientific, highly susceptible to errors, and fundamentally invalid evaluations of the literature. Dr. Alvar proclaimed that his research (2-4) *unequivocally* showed that the difference in the magnitude of strength gains is quite large as a result of multiple set training. Such profound proclamations of certitude demand extraordinary evidence. Because Dr. Alvar stated that each of the three meta-analyses required two years to complete, the expectation is that each meta-analysis should be accurately and meticulously reported. Therefore, a close examination of the meta-analyses in question and some of their included studies is warranted.

A meta-analysis is a statistical procedure that combines the results from independent studies. Each of the studies should address related research questions such as the effects of single and multiple sets of resistance training on muscular strength. The meta-analysis requires the conversion of each study's outcome into an effect size. In an attempt to produce a single estimate of the effectiveness of a specific intervention, the effect sizes for each study are combined in a meta-analysis. An effect size is the result of a statistical procedure to calculate a standardized estimate of how large or small the difference is between a pre- and post-training variable (such as strength gains) or the difference between two or more groups for a specific variable. In statistical terms, the effect size (ES) represents the difference in the number of standard deviations (SD) between the means; in other words, the ratio of the difference between the means to the standard deviation (ES = Mean 2 – Mean 1 / SD). Statisticians classify an effect size of 0.2 a small effect, 0.5 a medium effect, and 0.8 a large effect (6).

Statisticians also consider the meta-analysis controversial. That is, a meta-analysis inherits all the flaws of the included studies, as well as all the problems inherent with the peer review, editing and publication of those studies. For example, if a similar bias is present across a range of studies, the only effect of a meta-analysis is to reinforce that bias (7). Combining poor-quality data or overly biased data from the included studies produces unreliable results (8). A meta-analysis can also be selectively biased (the file drawer effect). That is, studies that report positive findings (i.e., an advantage of one training protocol over another) are more likely to be published than those that find no significant difference between protocols (9); for example, similar outcomes as a result of single and multiple set training. Consequently, the studies included or excluded from the meta-analysis have a profound effect on the conclusions.

A reliable meta-analysis requires consistent, high-quality reporting of the primary data from the individual studies (8). A meta-analysis of poorly designed studies will result in poor statistics. Meta-analysts are often careless in summarizing included studies and fail to understand the basic issues

(9). Reviewers of meta-analyses rarely—if ever—take the time to review the original studies because this tedious, time-consuming task exceeds the capacity and tolerance of most peer reviewers (10). Most importantly, a meta-analysis does not necessarily exclude the possibility that the results from included studies could be due to methodological flaws or fraud.

Spectacular claims such as those by Rhea and colleagues require spectacular evidence. The preponderance of studies suggest that there is no significant difference in strength gains as a result of performing a single set or multiple sets of each exercise (11-12). Therefore, the entire burden of proof rests solely on those who claim that multiple sets unequivocally elicit superior strength gains.

This critical analysis reveals that the meta-analyses in question (2-4) and their included studies failed to fulfill the burden of proof. We also focus attention on a much more grievous concern than the debate over single versus multiple sets for resistance training. That is, the strength and conditioning community should question how these flawed meta-analyses survived the peer-review process.

META-ANALYSIS #1

In Meta-analysis #1, Rhea and colleagues (2) claimed that narrative reviews such as the review by Carpinelli and Otto (11) rely solely on the reviewers' jurisdiction, and that a meta-analysis provides a more accurate picture of treatment effects. In fact, we did not possess any power of authority or jurisdiction, so we simply described the methodology, data, results, and conclusions that were reported by the authors of all the comparative single and multiple-set studies published at that time. We did not make any unequivocal conclusions. That is, we trusted readers to draw their own conclusions on single-set or multiple-set training based on the available scientific evidence. When they referred to our review, Rhea and colleagues (2) incorrectly referenced our response (13) to a letter-to-the editor of Sports Medicine (14), and not our review article (11).

Rhea and colleagues (2) used the means and pooled standard deviations to calculate effect sizes, and designated single-set training as the control group. The labeling of a treatment group as a control group is confusing. A true control group is a non-treatment group (without exposure to any intervention) that is compared with an intervention group at the beginning and end of the study. Perhaps the authors were referring to a *comparison* group rather than a control group. They claimed that the studies in their meta-analysis included single and triple-set training groups, and that to be classified as a *controlled* study, both groups must have trained at equal intensity with similar variation in protocols. Their operational definition of training *intensity* was the average percent of the 1RM used throughout the training program. If the multiple-set group used a specific variation in the training protocol, the single-set group had to follow a similar variation in protocol.

Rhea and colleagues (2) classified participants as trained or untrained. They claimed that to be classified as trained, participants must have been weight training for at least one year prior to the study. Their standard for establishing training status is ambiguous. Actually, the length of time that some people have been weight training may be irrelevant to their level of muscular fitness. Some trainees may become very fit and strong after only 3-6 months of resistance training; other trainees remain relatively unfit after several years of resistance training. Therefore, the claim by Rhea and colleagues that the effect size in trained participants (ES = 0.55) was significantly greater than previously untrained participants (ES = 0.23) is confusing, and has no practical application to resistance training. Perhaps some comparative measurement of muscular fitness, rather than just the length of time training, would be a better indication of training status.

Rhea and colleagues included 16 studies (5,15-29) in Meta-analysis #1 (2). We assume that they reported the effects of single and multiple sets per exercise—and not muscle groups—because they did not clarify what was reported in their meta-analysis. Some of the studies (e.g., Berger, 15) reported data and described the training and testing protocols for only one exercise (free-weight bench press). Berger compared the results of groups that trained using the same number of repetitions, and according to Rhea and colleagues, a similar intensity or percent 1RM. However, Berger reported no significant difference between 1 set vs. 3 sets of 2RM; 1 set vs. 3 sets of 6RM; or 1 set vs. 3 sets of 10RM. Capen (16) reported on eight groups of subjects who used different combinations of four training programs, which consisted of various loads (RMs), sets, and frequency of training. However, in his comparison of 1 versus 3 sets (groups C and E), the *intensity* was 8-15RM for group C and 1RM for group E. Therefore, Capen's study did not meet the intensity criterion established by Rhea and colleagues for inclusion in their meta-analysis. In fact, only six (5,17-20,29) of the 16 included studies actually met the criteria for similar intensity (percent 1RM) and variation established by Rhea and colleagues (2), and five out of six of those studies reported no significant difference in strength gains between 1-set and 3-set protocols (17-20,29).

Rhea and colleagues claimed that a few studies reported significant differences between 1-set and 3-set strength gains, citing the studies by Berger (15), Kraemer (22) and Kramer and colleagues (23), which they listed twice (p.487). In fact, as we previously noted, there was no significant difference in the Berger study when he compared similar training intensity (RMs); and the studies by Kraemer and Kramer and colleagues did not meet Rhea and colleagues' variation criteria for controlled studies. The only study that met their inclusion criteria for intensity and variation (Figure 1) was by Rhea and colleagues (5). Nonetheless, they failed to identify their own study as meeting the aforementioned criteria, and they also cited their study incorrectly (2). The study by Rhea and colleagues (5) showed a significant difference in one variable as a result of 1-set or 3-set training. However, the following critical analysis of their study reveals numerous statistical errors.

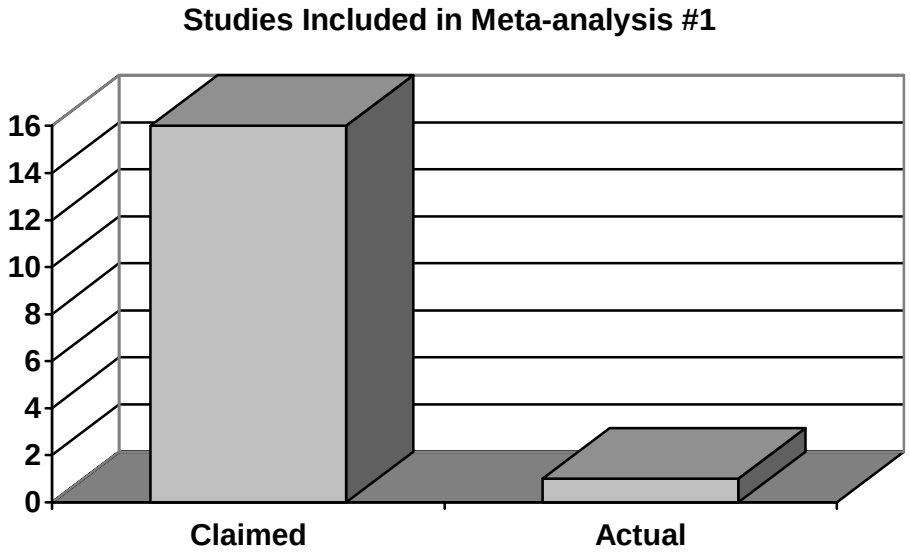

Figure 1. Number of controlled studies that actually met the criteria for inclusion designated by Rhea et al (2) and showed a significant difference in strength gains.

Rhea and colleagues (5) assigned 16 recreationally trained subjects (mean age = 21 years) with a minimum of two years resistance training experience to perform either 1 set or 3 sets of the leg press and bench press exercises 3 times a week for 12 weeks. There was no indication of how many sets

the subjects were completing before they were recruited for the study. If the participants were previously training with multiple sets or really believed that multiple sets were superior, their study design failed to account for this bias. Rhea and colleagues (2) claimed that advanced trainees and competitive athletes require multiple sets for maximal strength gains. Ironically, the potential bias is greatest in this demographic.

Starting strength for the bench press was similar for the 1-set and 3-set groups and both groups showed a significant increase in 1RM (5). There was no significant difference in strength gains between the 1-set and 3-set groups. The baseline 1RM leg press was 19% greater in the 1-set group (269.04 kg) compared with the 3-set group (225.85 kg); however, it was noted that this difference was not statistically significant. Both groups significantly increased 1RM leg press, and the gain in the 3-set group was significantly greater than the 1-set group. The 1RM leg press in the 3-set group increased over 50% (from 225.9 to 343.5 kg) in 12 weeks. There was no significant change in lean body mass or thigh circumference to justify the extraordinary strength gains in these experienced, resistance trained subjects. Rhea and colleagues' classification of training status (two years of resistance training) is apparently a very poor indicator of actual muscular fitness. Both groups attained similar levels of strength at the end of the 12-week study (337.2 and 343.5 kg, 1-set and 3-set groups, respectively). Figure 2 shows the actual pre-, mid-, and post-training absolute 1RM leg press for both groups.

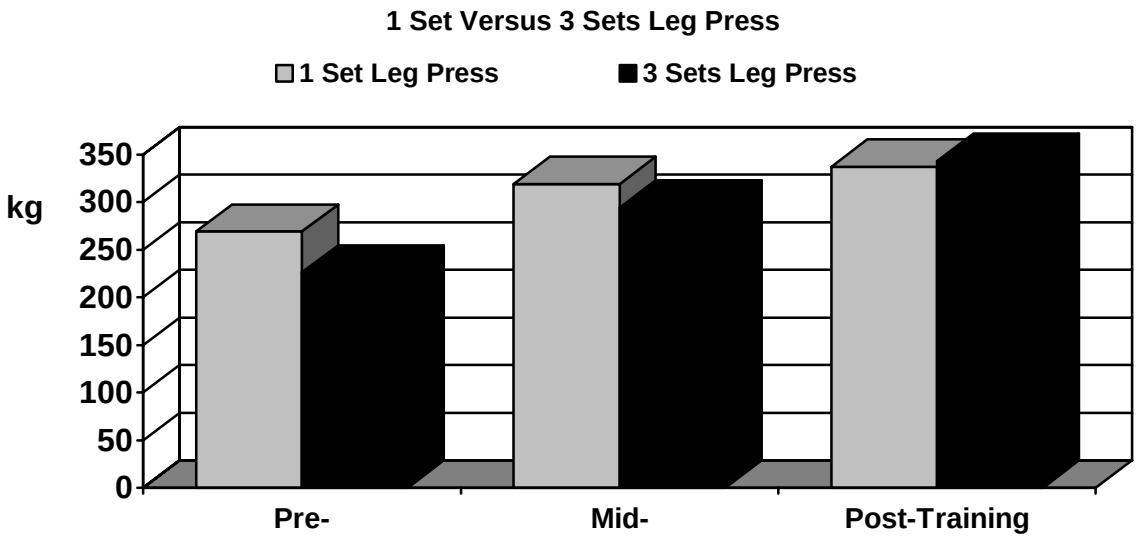

Figure 2. Actual pre-, mid-, and post-training 1RM leg press (Rhea et al, 5).

Rhea and colleagues (5) reported a 3.6-fold increase in the standard deviation for the 3-set group (from 25.0 to 89.9), and a 4-fold increase for the 1-set group (from 16.8 to 69.0) pre-to post-training. This extremely large pre- to post-training increase in standard deviations certainly questions the controls in the study's methodology and whether Rhea and colleagues should have even considered using these data to generate effect sizes.

Rhea and colleagues (2) have noted that an effect size is a measure of treatment effectiveness. Using the 1-set group as a control (comparison) group, they reported an effect size of 2.3 for the bench press and 6.5 for the leg press (5). The measurement of the effectiveness of the 3-set over the 1-set protocol for the bench press (ES = 2.3) was almost 3 times what statisticians (6) define as large (ES = 0.8). They reported an effect size for the leg press (ES = 6.5) that was more than 8 times greater than what is considered a large effect. That is, the purported difference in strength gains

between groups was 6.5 standard deviations. Based on the data reported by Rhea and colleagues (Table 2, p.527), an effect size of 6.5 would require a pooled standard deviation (denominator) of approximately 7.6 (the difference in post-training mean increase in 1RM for the 3-set and 1-set groups divided by the pooled standard deviation: 117.65 - 68.16 = 49.49 / 7.6 = 6.5). We calculated a pooled standard deviation of 80.1. There is more than a tenfold difference between the actual standard deviation (SD = 80.1) and the standard deviation required (SD = 7.6) to yield the effect size reported by Rhea and colleagues (ES = 6.5).

Rhea (30) has proposed elevating the scale for interpreting effect sizes. That is, contrary to contemporary statistical standards, he suggested that 1.5 should be considered a large effect for recreationally trained participants in strength training studies. However, Rhea did not report any rationale supported by data or logic for his proposed new standards. Furthermore, even if his unsupported elevated standards were validated, the extraordinarily large effect sizes (ES = 2.3 and 6.5) claimed by Rhea and colleagues (5) are rarely—if ever—reported in the scientific literature. Figure 3 shows the effect size (ES = 6.5) reported by Rhea and colleagues compared with our estimation of the actual effect size (ES = 0.62) for the leg press exercise.

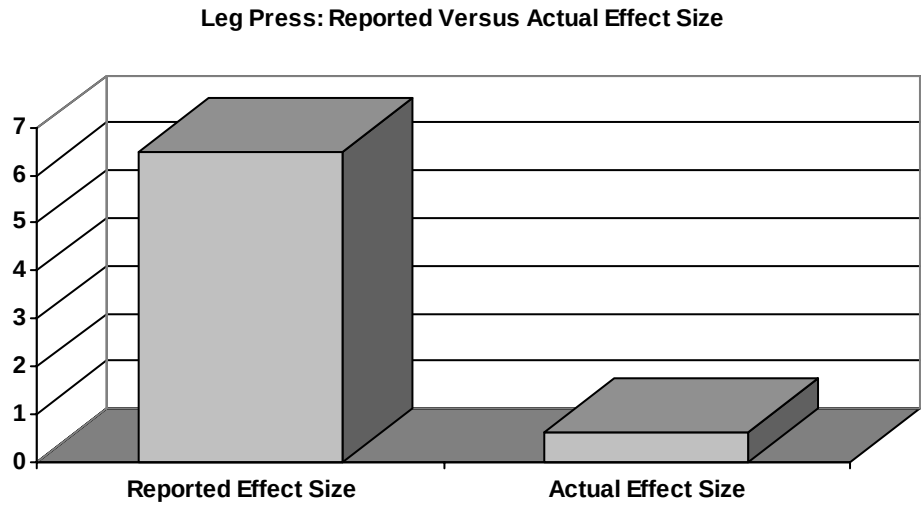

Figure 3. Effect size reported by Rhea and colleagues (5) versus our estimation of the effect size comparing percent increase in leg press 1RM between the 1-set and 3-set groups.

Interestingly, one of the studies that Rhea and colleagues (2) included in Meta-analysis #1, but actually did not meet their criteria for similar variation in training between 1-set and 3-set groups, was a series of *Experiments* reported by Kraemer (22). The data for this series of *Experiments* were from a database that Kraemer accumulated as a coach at least 15 years prior to publication. The remarkable differences in outcomes for one training protocol were 3 to 14 times greater than another. The subtitle of this series of *Experiments* is *Fact over Philosophy*; however, there was no logical physiological explanation for these extraordinary differences. Because these unusual data (outliers) would critically skew the statistical results, they should be excluded from a meta-analysis.

Nonetheless, a comparison of our estimated effect sizes from Kraemer's *Experiments* (22) with those reported by Rhea and colleagues (5) is noteworthy. For example, in *Experiment* #2, Kraemer reported the results of strength gains for the leg press exercise as a result of 1-set (2.8%) and 3-set (18.9%) resistance training. The increase in leg press strength was significantly greater in the 3-set group. Because the initial leg press strength was similar between the 1-set (176 kg) and 3-set (175 kg) groups, we used the post-training means and pooled standard deviations to estimate the effect

size between the groups. Although the strength gain was more than six times greater in the 3-set group (33 kg) compared with the 1-set group (5 kg), our estimated effect size was 0.68. From a practical application, a legitimate effect size of 0.68 in highly trained athletes would be extremely meaningful. According to Rhea (30), the difference in strength gains in these highly trained Division I American football players would be classified as a moderate effect (ES = 0.50-1.0, Table 1, p.919). The 3-set group was initially 19% weaker in the study by Rhea and colleagues (5), improved the leg press 1.7 times greater than the 1-set group, and both groups had standard deviations twice as large as those reported by Kraemer (22). Rhea and colleagues claimed that their effect size was 6.5. This unusually high effect size was obviously unchallenged throughout the entire peer-review process in the *Journal of Strength and Conditioning Research*.

Rhea and colleagues (5) noted that the rest time between sets in their study was 1-2 minutes for the 3-set group. They claimed that the amount of rest between sets is an important variable for eliciting an overload in multiple-set training. They cited a book by Zatsiorsky (31), which has no resistance-training studies to support his *corridor theory of strength training.* Rhea and colleagues' theory is that if the time between sets is not sufficient for the recruited motor units to recover from exhaustion, then different motor units are recruited and overloaded for the next set of the exercise. This scenario presumably overloads multiple sets of motor units and elicits greater adaptations than a single-set protocol (5). Contrary to the *corridor theory,* the size principle - one of the most supported principles in neurophysiology (32-50) - states that if a set of repetitions is carried to a point of maximal effort, the motor units in the specific pool (those motor units available for a specific exercise) are recruited from the most easily excitable smaller motor units to the most difficult to excite larger motor units. Therefore, all the motor units in the pool are stimulated during the single maximal set and additional sets are apparently not required to stimulate that specific pool of motor units. Claims that different rest time between sets or exercises affect specific outcomes such as increased muscular strength, power, hypertrophy, or endurance lack scientific support (12).

Although another study by Rhea and colleagues (51) compared three specific so-called *periodization* protocols and not single versus multiple sets, a critical analysis of that study clearly challenges their ability to accurately report results and conclusions. They randomly assigned 60 recreationally trained males and females (mean age = 21 years) to one of three *muscular endurance* treatment groups: linear periodization (LP), daily undulating periodization (DUP), or reverse linear periodization (RLP). Subjects performed 3 sets of knee extension exercise with 15, 20, or 25RM (the specific variation in protocol depended on the group assignment) 2 times a week for 15 weeks. All groups significantly increased muscular endurance (repetitions performed to fatigue with 50% body mass). Rhea and colleagues claimed that although the RLP group increased the most, there was no statistical difference between groups.

There was no explanation why Rhea and colleagues (51) decided to use the LP group as the *control* group. They claimed that the effect size for the RLP group (ES = 0.27) was significantly different from the LP group (*comparison* group) and the DUP group (ES = -0.02). They concluded that if maximal improvement in muscular endurance is the goal, reverse linear periodization (RLP) should be prescribed. However, the data from their Table 3 (p.85) are replete with mathematical and statistical errors, and fail to support their conclusion that RLP is superior to LP and DUP.

According to the data in their Table 3 (p.85), the groups increased the number of repetitions from pre- to post-training by 12.6 (LP), 12.2 (DUP), and 11.8 (RLP). Rhea and colleagues (51) claimed that the increase was 55.9, 54.5, and 72.8%, respectively (72.8% was not a typographical error because it also appeared in the text on p.85). However, their own data (Table 3, p.85) revealed that the increase for the RLP group was 55.4%—an increase in muscular endurance that was almost identical

to the other two groups. Using the same statistical methodology that Rhea and colleagues (51) used to calculate effect size with the correct increase in muscular endurance for the RLP group (55.4% instead or 72.8%), we estimated an effect size of -0.008. Based on their reported data, the three groups actually showed similar gains in muscular endurance.

In his proposal to change the scale for evaluating effect sizes in the resistance training literature, Rhea (30) claimed that an effect size less than 0.35 for recreationally trained subjects is trivial. Therefore, according to their own standards, their incorrectly calculated and reported effect size of 0.27 for the RLP group would be considered trivial. There is no real description for the actual effect size of -0.008 - other than completely meaningless. Figure 4 shows the absolute increase in muscular endurance (repetitions), and compares the reported percent change with the actual percent change for the three groups. Rhea and colleagues (51) should have stood by their original results; that is, stating that there was no significant difference among the groups.

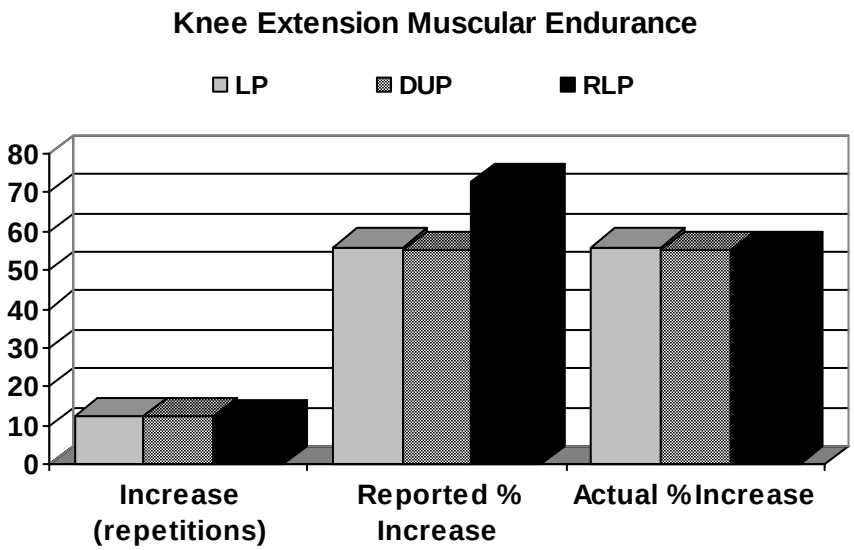

Figure 4. Absolute increase in repetitions; reported and actual percent increase (Rhea et al, 51).

Our examination of these two training studies by Rhea and colleagues (5,51) is what we call a critical analysis. That is, we perused the study, performed some simple mathematical calculations, such as the percent change in variables, and determined whether their reported data and results actually supported what they claimed in their discussion. To prevent a meta-analysis from inheriting the flaws of the included studies, a critical analysis of those studies should always precede any attempt at a meta-analysis - a task that becomes more challenging when attempting to critically analyze your own studies.

Only one (5) of the 16 studies that met their criteria for a *controlled* study in Meta-analysis #1 (2) reported a significant difference in relative strength gains for one exercise; however, both the 1-set and 3-set groups had similar absolute levels of strength at the end of the study. The mean effect size in Meta-analysis #1 was 0.28, with a standard deviation of 0.56 for 1-set versus 3-set resistance training. A standard deviation twice the size of the mean represents data from a huge variation of responses (strength gains) - perhaps because of differences in genetic potential or poorly controlled methodological factors. It may also indicate a contradiction of the standard premise of parametric statistics, which assumes a homogenous population, and questions the validity of using effect size to estimate differences in outcomes.

Rhea and colleagues (2) concluded that as people become more accustomed to resistance training, more volume (a greater number of sets) is required for maximal strength gains. Their conclusion is not supported by the studies in Meta-analysis #1. Consequently, Meta-analysis #1 failed to meet the required burden of proof to support the *unequivocal* superiority of multiple-set training.

META-ANALYSIS #2

In Meta-analysis #2, Rhea and colleagues (3) calculated effect size using the difference between the post-training and pre-training means divided by the pre-training standard deviation, which is different from their use of the pooled standard deviation in Meta-analysis #1 (2) and their two training studies (5,51). We assume that when Rhea and colleagues used the terms *pretest mean* and *pretest standard deviation* they were referring to the *pre-training* mean and standard deviation. There is no mean or standard deviation until after the pre-training strength test.

Rhea and colleagues have claimed that narrative reviews such as ours (11) have created *confusion* (presentations at the NSCA 2004 and 2005 National Conference). However, we would argue that Rhea and colleagues created the *confusion* because they shifted the topic of discussion from sets per exercise - as it was in our review (11), their training study (5), and Meta-analysis #1 (2) - to sets per muscle group in Meta-analysis #2 (3). In fact, even the ACSM's Position Stand on resistance training (52) specifically recommended the volume of exercise for increased strength (p.367), hypertrophy (p.370), power (p.371), and endurance (p.372) as sets per exercise - not sets per muscle group.

Rhea and colleagues (3) did not explain how they were able to determine the specific muscle groups that accounted for the strength gains reported in each study. More importantly, they did not specify how they coded sets per muscle group; that is, studies that employed multiple exercises for the muscles involved in the exercise tested and reported. For example, Berger (15) reported the results for the free-weight bench press in nine groups of males enrolled in a college weight-training program. The students executed different combinations of sets and repetitions for the bench press exercise. However, these participants also performed numerous other exercises in the weight-training class, and there was no control for the number of sets or repetitions for any of the other exercises (53). Consequently, some individuals who executed 3 sets of 6 repetitions in the bench press may have performed any number of sets and repetitions with other exercises such as the military press, triceps press, and dipping exercises - all stressing the triceps muscles. Indeed, some individuals in any of the three 3-set groups may have performed 3 sets or more for each of the other three exercises involving the triceps; thereby stressing the triceps muscle group - a prime mover in the bench press - 12 times or more. Other individuals in that same group may have performed 1 set for each of the other three triceps exercises, for a total of 6 sets for the triceps. Furthermore, the protocol for these other exercises may have varied from session to session. The point is that Rhea and colleagues did not explain how they were able to code Berger's study, or any of the studies included in their meta-analysis, for the number of sets per muscle group.

Rhea and colleagues (3) described their interpretation of *intensity* as the average percent of the 1RM, which was the same as in Meta-analysis #1 (2). *Frequency* was described as the number of times each week that a particular muscle group was trained. Participants must have been weight training for at least one year prior to enrolling in a study to be classified as *trained*. As with Meta-analysis #1 we find this classification of training status (length of time) ambiguous.

Rhea (54) has noted that a potential source of bias in research is an over-reliance on published research. He recommended that both published and unpublished studies should be included in a

meta-analysis. Rhea and colleagues (3) claimed that they performed a literature search for published and unpublished strength training intervention studies. However, there is no indication in their narrative or reference section of any unpublished studies, their inclusion and exclusion criteria for these studies, or how many unpublished studies were found in their search.

Rhea and colleagues (3) cited the aforementioned study by Berger (15), which was also included in Meta-analysis #1 (2). They claimed that it was one of the most notable scientific studies, and that the study demonstrated how different training volumes and intensities elicited different increases in strength. However, Berger's results did not support that claim. Berger reported nine comparisons between groups that performed the same number of repetitions (two, six, or ten) for 1, 2 or 3 sets. Seven of his comparisons showed no significant difference in strength gains. Curiously, 3 sets of 6 reps were significantly better than 2 sets of 6 reps, but not significantly better than 1 set of 6 reps; and there was no significant difference between 1 set and 2 sets. That is, there was no logical pattern of results (53). Perhaps the two out of nine small but statistically significant differences reported by Berger were simply due to chance. We estimated the effect size for each of the three groups in Berger's study who were performing a similar number of repetitions (two, six, or ten), which denotes a similar training *intensity* according to Rhea and colleagues. The estimated effect sizes for 3 sets versus 1 set of 2, 6, and 10 repetitions were 0.23, 0.21, and 0.12, respectively, for Berger's previously untrained male subjects. Statisticians (6) classify these effect sizes as *small*. According to the classification proposed by Rhea (30), these effect sizes are *trivial*.

Berger (15) and Rhea and colleagues (3) claimed that 3 sets of 6 repetitions resulted in the greatest strength gains. In fact, Berger reported using an analysis of covariance to test for any significant interaction between sets and repetitions. He reported that the results for this interaction were not significant for any period of training. That is, 1, 2, or 3 sets was not better systematically with 2, 6, or 10 repetitions for increasing strength. Berger's Conclusion #6 states that training with 1, 2 or 3 sets in discrete combination with 2, 6, or 10 repetitions per set (interaction) was not systematically more effective in improving strength than other combinations of sets and repetitions. Berger's statement is supported by his statistical analysis. However, Berger's Conclusion #7 (the last sentence in his study, p.181) claims that a combination of 6RM performed for 3 sets was more effective in improving strength than any other combination of sets and repetitions. Berger's Conclusion #7 is not supported by his own statistical analysis. It actually contradicts his Conclusion #6 and the claim by Rhea and colleagues.

As previously mentioned, Berger's study (15) was replete with potentially confounding variables such as no control for sets, repetitions, or effort for any of the other exercises involving the triceps, pectorals and deltoids, which the novice trainees employed in their college weight training class (53). These methodological flaws should have provided enough evidence for Rhea and colleagues (3) to have classified Berger's study as exclusionary rather than including it in their meta-analysis. Interestingly, in a meta-analysis by a different group of investigators (55), Berger's study did not meet the criteria for inclusion because the authors also noted that the study was not well controlled. Rhea and colleagues (3) included one of Berger's follow-up studies (56) in Meta-analysis #2, but did not include another study by Berger (57). Both of Berger's follow-up studies reported no significant difference in strength gains as a result of training with 3 sets or 6 sets of free weight bench press (56) or between 1, 2, or 3 sets of the bench press exercise (57).

There were eight strength-training studies from Dr. Hurley's group (58-65) that were included in Meta-analysis #2 (3). These studies reported on various health-related benefits of resistance training. With the exception of one study (59) that used a 5-set program (9-week lower-body only), the training protocol was similar in all these studies: 1 set for each upper-body exercise, 2 sets for each lower

body exercise. All the studies were well controlled, supervised, and of longer durations (4-6 months) than most strength training studies. The results were reported for 1-set and 2-set exercises within the whole study group, rather than a comparison between different groups of trainees. Strength gains were similar as a result of training with 1 set or 2 sets. However, there are at least 12 other studies (66-77) from Dr. Hurley's group that followed the same training protocol. The average strength gain in the 19 studies (58,60-77) was 41.7% for 1-set training (upper body) and 38.6% for 2-set training (lower body). The lower-body strength gain was 31.5% for the subjects in the 5-set study (59). Rhea and colleagues (3) did not address the exclusion of these additional 12 studies (66-77) from their meta-analysis.

There are several very important, very simple examples to show that Meta-analysis #2 is illogical and impractical. In Table 1 (p.458), Rhea and colleagues (3) reported an effect size of 2.8 as a result of training with an average 60% 1RM and an effect size of 1.2 as a result of 70% 1RM training. That is, assuming that the effort of training in previously untrained subjects was similar in both situations (e.g., muscular fatigue), the effect size as a result of training with 60% 1RM was 2.3 times greater than training with 70% 1RM (a difference of 1.6 standard deviations). Furthermore, the variation in the percent 1RM may be from 45-75% in an average 60% 1RM group, with some periods of training employing 45% 1RM and some overlapping the next category (70% 1RM). There is no known physiological mechanism that would explain this reported pattern of outcomes. Rhea and colleagues did not address this issue.

Rhea and colleagues (3) reported effect sizes of 1.75, 1.94 and 2.28 for 2, 3, and 4 sets per muscle group in previously untrained populations, but failed to indicate if there was any significant difference in effect size between these protocols. Five sets per muscle group elicited an effect size of 1.34, almost one standard deviation below 4 sets (ES = 2.28). The implication is that a 1-set protocol of the bench press, military press, triceps press, and dipping exercises in a previously untrained population elicits the greatest strength gains in the triceps; but adding only one more set of any one of these exercises or the addition of just one more triceps exercise would reduce the training effect by almost one standard deviation. These conclusions of Rhea and colleagues are illogical and fail to have any practical application to resistance training.

The data reported by Rhea and colleagues (3) for advanced trainees are also illogical from a practical aspect. They claimed that training with an average 80% 1RM resulted in an effect size (ES = 1.8) that was almost three times greater than using 85% 1RM (ES = 0.65). Their data apparently suggest that if the average 1RM is 100 kg for a specific exercise, and training involves an 8RM with 80 kg, which was estimated by Rhea and colleagues to be 80% 1RM (e.g., with a variation of 70-90% 1RM), the effect on strength gains is 2.8 times greater (more than 1 standard deviation difference) than performing that specific exercise with an average 85 kg (85% 1RM or approximately 6-7RM). There is no known physiological mechanism that would explain this large difference in outcomes as a result of a relatively small change (5%) in resistance (Figure 5). Rhea and colleagues did not address this issue. In addition, the practical application of an *average percent 1RM* is also questionable and not addressed by Rhea and colleagues. For example, training three times a week with an average 80% 1RM could refer to 1 set for a specific muscle group at each of the three sessions using 78, 80 and 82% 1RM (average = 80% 1RM); or 3 sets for the muscle group with 65, 80, and 95% 1RM (average = 80% 1RM); or some combination of these examples. Perhaps Rhea and colleagues are implying that because they claim designated outcomes are dependent on a specific percent 1RM, trainees should evaluate their 1RM every week or perhaps at each session for every muscle group. They did not explain how the average percent 1RM was determined or its practical application to sensible resistance training.

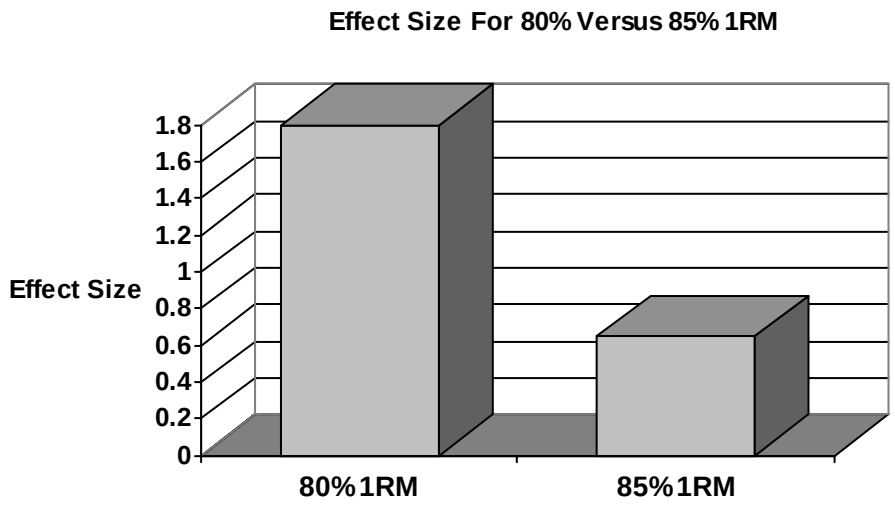

Figure 5. Rhea et al. (3) claimed that the effect size for increased strength was 2.8 times greater (>1 standard deviation) as a result of training with an average 80% 1RM versus 85% 1RM.

Rhea and colleagues (3) reported that training each muscle group 2 times a week elicited an effect size of 1.4 for strength gains, which was two times greater than training 3 times a week (ES = 0.70). Their rationale for this difference was that the higher training volume, which was common among the training interventions for trained populations, is more strenuous than lower volume programs and requires longer recovery time between training sessions. However, we disagree with their rationale that previously untrained populations should perform three sessions a week simply because the sessions are less strenuous. We are not aware of any evidence to suggest that 4 sets of 12RM (60% 1RM) 3 times a week are any less strenuous than 4 sets of 8RM (80% 1RM) 2 times a week. Each set of both protocols is equally strenuous; that is, the effort at the completion of each set is maximal - as noted in the designation of *repetition maximum* (RM) by Rhea and colleagues. The volume at each session - for those who actually believe that total volume is meaningful - is also similar in both protocols. For example, if the 1RM for a specific exercise is 100 kg, 4 sets of 8 repetitions with 80 kg (80% 1RM) yields a session volume of 2,560 units; and 4 sets of 12 repetitions with 60 kg (60% 1RM) produces a session volume of 2,880 units. Rhea and colleagues did not cite any evidence to support their claim that one protocol was more or less strenuous than the other.

Rhea and colleagues (3) also claimed that additional strength increases accompany training beyond single-set protocols. Their data for advanced trainees (Table 3, p.458) show effect sizes of 0.92, 1.0 and 1.17 for 2, 3 and 4 sets per muscle group. There is no reported statistical analysis of differences between these effect sizes; hence, any conclusions from these data are questionable.

Interestingly, Rhea and colleagues (3) concluded that exercise prescription for strength gains is a complex process involving the manipulation of each of the resistance training variables. In contrast, we have previously reported that the complex manipulation of any resistance-training variable in an attempt to enhance gains in muscular strength, hypertrophy, power, or endurance in novice, intermediate or advanced trainees is primarily based on unsubstantiated opinions, and lacks substantial scientific evidence for support. We supported our conclusion with a critical analysis of the available strength training studies (12).

Rhea and colleagues (3) concluded that progression (continued adaptations over time) is dependent on variation (so-called *periodization*), additional work (volume), and higher intensity (greater percent

1RM). These conclusions reported by Rhea and colleagues are illogical, without foundation, and have no reasonable practical application to resistance training.

META-ANALYSIS #3

Peterson and colleagues (4) claimed that the 2002 ACSM Position Stand on resistance training (52) examined, affirmed and reinforced the research that established the principles required for optimal strength gains. They alleged that chronic alteration of resistance, repetitions, sets, rest periods, and exercise selection are prerequisites for progressive resistance training. They claimed that as training experience increases, there must be an increase in training volume and intensity; as well as a continuum of training adaptations that correspond to specific populations. These adaptations are allegedly based on training status (experience). The ACSM's Position Stand was the only reference cited in an attempt to support all these claims by Peterson and colleagues.

Peterson and colleagues (4) claimed that Meta-analysis #2 (3) is critical to the body of literature because it identified objective data for the optimal dose of training to produce maximal strength gains in previously untrained and trained populations; essentially eliminated the ambiguity that surrounds fundamental training variables in specific populations, thereby maximizing potential adaptations; and that Meta-analysis #2 strongly supported the ACSM's Position Stand (52). Our critical analysis (12) of the ACSM's Position Stand revealed that all the aforementioned claims were primarily unsubstantiated. That is, the ACSM failed to meet the burden of proof required for a scientific document. In fact, the recently published (2005) ACSM's Guidelines for Exercise Testing and Prescription (78) are inconsistent with what was published in the 2002 ACSM Position Stand. The new ACSM Guidelines recommend one set for each of 8-10 exercises (p.158); and state that the preponderance of evidence reports similar gains in muscular strength, hypertrophy, power, and endurance as a result of single or multiple-set programs (p.157). The ACSM Guidelines also note that for any commonly performed range of repetitions such as 3-6, 6-10, 10-12 (i.e., percent 1RM) which was defined as *intensity* by Rhea and colleagues (2-4), there is a lack of scientific evidence to suggest that a specific number of repetitions (*intensity*) will provide superior responses for a specific adaptation such as muscular strength, hypertrophy or endurance (p.156). Contrary to the definition of *intensity* by Rhea and colleagues, the new ACSM Guidelines define intensity as the degree of effort or difficulty in performing an exercise. A 3RM, 10RM, and 15RM all result in a similar intensity as designated by repetition maximum (RM), and the use of percent 1RM does not portray a true measure of intensity (p.156). Most importantly, the Guidelines (78) note that there is very little scientific evidence to suggest that the stimulus for improving muscular strength in resistance-trained populations is different from previously untrained populations (p.158).

Peterson and colleagues (4) calculated effect size using the post-training mean minus the pre-training mean divided by the pre-training standard deviation. According to Thomas and colleagues (79), a pooled standard deviation is recommended when there is no control group. Dr. Alvar noted in his 2005 NSCA presentation that Dr. Thomas is the research methodological professor who guided his group through the three meta-analyses (2-4), and claimed that Dr. Landers—one of Dr. Thomas' co-authors—is the guru of meta-analytical research. However, many of the studies in Meta-analysis #2 (3) and #3 (4), such as the previously discussed studies by Berger (15) and Kraemer (22), had no control group. According to their own statistical advisors, the absence of a control group in these studies necessitated the use of a pooled standard deviation rather than the pre-training standard deviation used by Peterson and colleagues (4).

Peterson and colleagues (4) claimed that because of the progression designated in the ACSM's Position Stand (52), the dose-response for resistance training in athletes differs from lesser-trained

populations. They defined the demographics for Meta-analysis #3 as competitive athletes at the collegiate or professional level. However, they provided no explanation for differentiating between serious highly-motivated advanced trainees, whose goals are to attain the greatest strength gains and muscular hypertrophy within the limitations of their genetic potential, and competitive collegiate or professional athletes - who may have very limited resistance training experience, or none at all. The absence of an identified objective physiological difference between these arbitrarily delineated groups precludes any conclusion about a differential response to a specific volume or intensity of resistance training.

One of the criteria for inclusion of studies in Meta-analysis #3 (4) was that study participants must have been competitive collegiate or professional athletes. However, at least nine (80-88) of the studies included by Peterson and colleagues involved subjects who had not performed resistance training for 3 (83), 6 (88), or 12 months (85) prior to the specific study, subjects that had no prior experience with resistance training (80,82,84,86), or there was no indication of prior resistance training (81,87). Peterson and colleagues did not offer any physiological hypothesis to explain how these competitive male and female athletes from varied sports (soccer, skiing, running, tennis, baseball, track and field, basketball, lacrosse), who were not performing any resistance training prior to the study intervention, would respond differently from other novice resistance trainees.

Most of the studies included in Meta-analysis #3 (4) did not compare different training protocols (e.g., 1 set versus multiple sets). They simply reported the effects of one specific protocol, or the effect of a specific training protocol with or without dietary supplementation on different outcomes. One of the references Peterson and colleagues listed as a training study, which was included in their meta-analysis, is actually a book (89). Another of their included studies (23) did not involve competitive collegiate or professional athletes. Kramer and colleagues classified the male subjects as moderately trained recreational weight trainees because they were able to parallel squat with at least their body mass. The increase in strength for the squat exercise was significantly greater for the two multiple-set groups compared with the single-set group. Although this study was included in Meta-analysis #3, the participants did not fulfill the specific criterion (competitive athletes) established for inclusion by Peterson and colleagues.

In contrast, a noteworthy study by Ostrowski and colleagues (90) was not included in Meta-analysis #3 (4). The participants were not described as competitive collegiate or professional athletes, but were just as qualified for inclusion in Meta-analysis #3 as the subjects in the study by Kramer and colleagues (23). That is, the participants in the study by Ostrowski and colleagues could bench press at least 100% of their body mass and squat with 130% of their body mass. They were randomly assigned to perform 1, 2 or 4 sets to muscular fatigue for each free-weight and machine exercise (2, 6, and 12 sets per muscle group per week). All the subjects followed a split routine (2 days upper body and 2 days lower body each week) for 10 weeks, with a similar variation in RM and rest between sets for the three groups. The only difference in training variables among the three groups was the number of sets. There was a significant increase in 1RM squat and bench press, muscle hypertrophy and body mass, with no significant difference in outcomes among groups who were performing 1, 2 or 3 sets per exercise (3, 6 or 12 sets per muscle group). Similar to the study by Kramer and colleagues, the study by Ostrowski and colleagues fulfilled the other criteria for inclusion in Meta-analysis #3: a strength training intervention and the data required to calculate effect sizes. Yet, the study by Ostrowski and colleagues is curiously missing from Meta-analysis #3. It appears that Peterson and colleagues included studies that supported their pre-determined beliefs and intentionally excluded studies that conflicted with their opinions regarding resistance training. Peterson and colleagues (4) claimed that maximal strength gains were elicited as a result of performing 8 sets per muscle group during each training session, which is twice the number of sets

that Rhea and colleagues reported in Meta-analysis #2 (3). Similar to Meta-analysis #2, they did not indicate how they coded the number of sets per muscle group. For example, one of their inclusive studies was the previously discussed series of *Experiments* by Kraemer (22). In *Experiment* #2, Kraemer reported strength increases in the bench press exercise. If the muscle group coded by Peterson and colleagues was the pectoralis, then that muscle group was a major mover in two training exercises (the bench press and chest fly) involved in testing the bench press, with a total of 2 sets per muscle group per session for the 1-set group. If their focus was on the deltoid muscles, then the bench press, chest fly, lateral raise, and military press training exercises were all contributing to the bench press test, with a total of 4 sets per session for the 1-set group. For the 3-set group, the session volume would be 6 sets for the pectoralis muscles and 12 sets for the deltoids. Peterson and colleagues did not indicate which muscles they were coding or explain the rationale for their choice.

Peterson and colleagues (4) also claimed that their data *unequivocally* demonstrate the added strength benefits of higher training volumes. However, their Table 2 (p.379) revealed a lack of any pattern for the effectiveness of the number of sets per muscle group. The effect sizes for 4, 5, 6, and 8 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 by variable and training protocol, with the level of significance set at $p<0.05$. However, they did not report any statistical differences between effect sizes.

In addition, their Table 2 (p.379) revealed a mean effect size of 1.22 for 8 sets per muscle group, which was generated from only six effect sizes. Peterson and colleagues (4) did not indicate the source of those effect sizes or how many studies produced those data. They may have calculated six effect sizes from only one of the several previously discussed training studies (80-88) that involved competitive athletes with little or no prior resistance training (novice trainees). They also reported a lower mean effect for 12 sets per muscle group (ES = 0.69) that was determined from 46 effect sizes, followed by a higher mean effect for 14 sets per muscle group, which was derived from eight effect sizes (Table 2, p.379). There was no report of any statistical comparison between the mean effect of 8 sets per muscle group (ES = 1.22) and 14 sets per muscle group (ES = 1.06). More importantly, they did not attempt to explain their reported pattern of a lower—and then a higher—mean effect with increased training volume. This unexplainable pattern of responses was also illustrated in their Figure 3 (p.379) and incorrectly referred to as Figure 1 by Peterson and colleagues.

In another attempt to support their high-volume training philosophy, Peterson and colleagues (4) claimed that experienced trainees adapt to lower training volume and need to increase their training volume to elicit a continued overload on the neuromuscular system (p.380). They cited the previously discussed and discredited ACSM Position Stand (52) and a training study by Hakkinen and colleagues (91). After training the knee extensors in 11 physically active males for 24 weeks, Hakkinen and colleagues speculated that during very *intense* strength training, neural factors are primarily responsible for strength gains, with some contribution from training-induced muscular hypertrophy. There was a significant decrease in neural activation during low intensity training and with complete cessation of training. They emphasized the importance of training *intensity* for maximal neural activation in subjects with previous strength training experience. There was only one training group; that is, there was no comparison of low and high volume strength training, and no suggestion or speculation by Hakkinen and colleagues that high-volume training is required for experienced athletes.

Contrary to the claim by Peterson and colleagues (4) that there was a trend for increased strength development with a greater percent 1RM up to 85% 1RM, their data (Table 3, p.379) failed to support that claim. The effect sizes for training with 70, 75, 80, and 85% 1RM were 0.07, 0.73, 0.57, and

1.12, respectively. That is, the effect size was 10 times greater for training with 75% 1RM compared with 70% 1RM, decreased for 80% 1RM, and then doubled for 85% 1RM. Their data also implied the unlikely scenario that training with 70% 1RM to muscular fatigue had no effect on strength gains (Figure 6). There is no known physiological hypothesis to explain why a 5% difference in resistance, performed for one or two fewer or greater repetitions with a similar effort, would result in such large differences in outcomes. Peterson and colleagues did not address this issue.

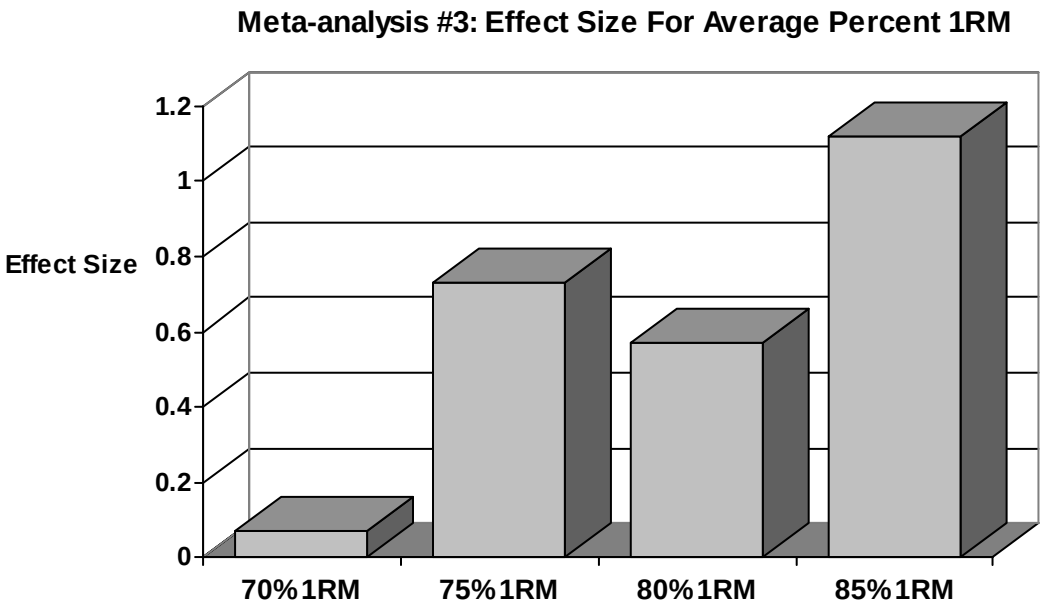

Figure 6. Effect sizes reported by Peterson et al (4) as a result of training with different percent of the 1RM.

More importantly, when comparing these effect sizes with those reported in Meta-analysis #2 (3), Peterson and colleagues (4) reported an effect size 10 times greater using 70% 1RM (ES = 0.70 compared with 0.07), similar effect size at 75% 1RM (ES = 0.74 and 0.73), and 3 times greater at 80% 1RM (ES = 1.8 compared with 0.57). At 85% 1RM the effect size in Meta-analysis #3 (ES = 1.12) was 1.7 times greater than in Meta-analysis #2 (ES = 0.65). All of these contradictory effect sizes were allegedly gleaned from similar populations of advanced trainees by the same investigators (Figure 7). Because Peterson and colleagues did not explain how advanced trainees differ from competitive athletes, these incompatible conclusions challenge the credibility of their Meta-analyses.

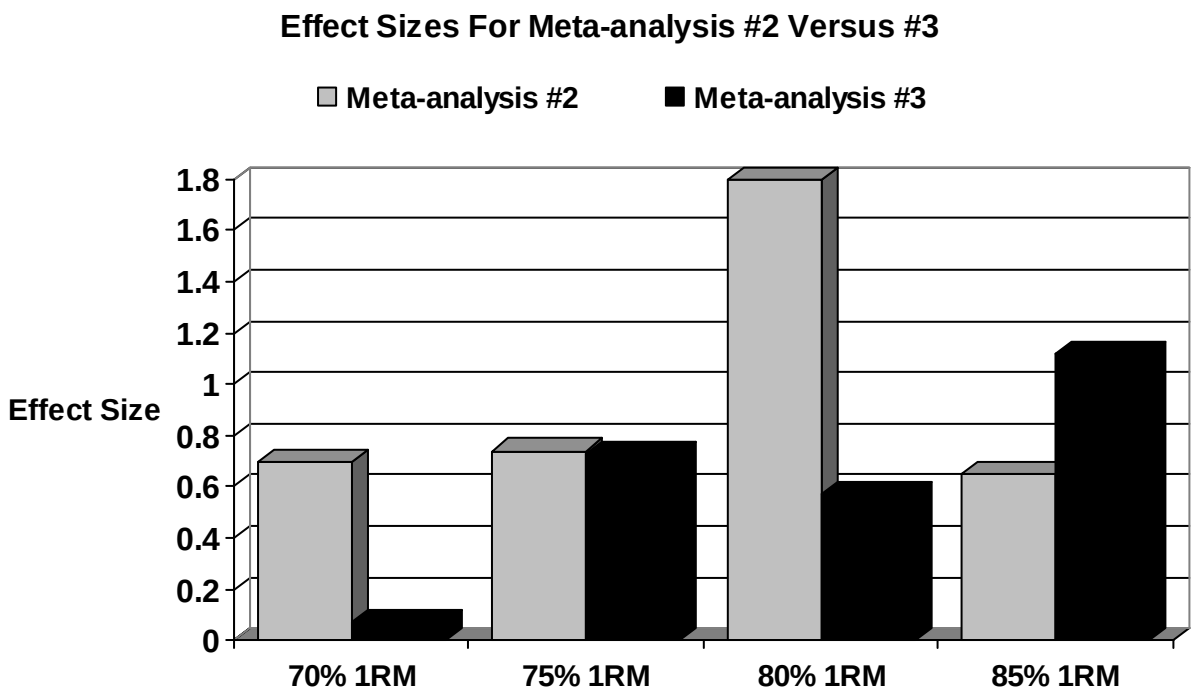

Figure 7. Comparison of effect sizes reported in Meta-analysis #2 (3) and Meta-analysis #3 (4) for strength gains as a result of training with a different percent of the 1RM.

Without any logical rationale for their claim that highly motivated advanced trainees responded differently from highly motivated competitive athletes, Peterson and colleagues reported similar effect sizes for training muscle groups 2 times a week (ES = 0.70) and 3 times a week (ES = 0.69). This was contrary to the results reported in Meta-analysis #2 (3) where advanced trainees produced twice the effect size as a result of training each muscle group 2 times a week compared with 3 times a week. Peterson and colleagues did not address this confounding issue.

Cohen (92) recommended reporting a confidence interval with each effect size because it shows the range of values of the effect-size index that includes a population value with a specific probability. Rhea and colleagues (3) and Peterson and colleagues (4) did not report confidence intervals. However, other groups of researchers have reported confidence intervals in their meta-analyses for the effects of resistance training on resting blood pressure (93-95), bone mineral density (96), and dietary supplements (97). Several of these meta-analyses reported on specific training variables and their relationship to health-related outcomes. For example, Cornelissen and Fagard (93) and Kelley and Kelley (95) reported that their health-related outcomes did not significantly differ among studies with different training intensities (% 1RM), or between conventional weight training and circuit weight training. Kelley (94) reported no statistical relationship between the decrease in pre- to post-training resting blood pressure and any of the previously discussed training variables such as the number of sets (1 to 5 sets), repetitions (5 to 25 reps), frequency (2-6 days a week), or rest between sets (15 to 216 seconds). That is, health-related outcomes were not significantly different among the various training protocols.

Peterson and colleagues (4) 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. Ironically, their own data do not support their conclusions.

CONCLUSIONS

Rhea (54) claimed that narrative reviews such as those by Carpinelli and Otto (11) have made incorrect conclusions, and they are less reliable and detailed than a meta-analysis. Rhea claimed that a meta-analysis provides a more objective, quantitative evaluation of the research, resolves controversies, provides the required clarification for scientific advancement, and greatly enhances the body of knowledge. Rhea also declared that his meta-analyses (2-4) reject the conclusions of narrative reviews (11), demonstrate the risk of error in the narrative review procedures, and provide much greater detail about the body of knowledge in the area of strength and conditioning. We would advise strength and conditioning practitioners to perform their own critical analysis of his meta-analyses and the included studies before accepting any of the aforementioned claims by Rhea (54).

Rhea (54) credits Gene Glass for popularizing the use of meta-analysis in the last 30 years. The genesis of that popularity is noteworthy - and revealing. Glass (98) claimed that he developed a major league neurosis while earning his doctorate in statistics. Although he believed that psychotherapy intervention helped him cope with his mental problems, the scientific studies showed no significant difference between psychotherapy and placebo. He claimed that the science was personally threatening and it challenged his own preoccupation with psychotherapy. Because he was obligated to give a speech at the 1976 National meeting of the American Educational Research Association, he stated that he used meta-analysis to confirm his personal belief in psychotherapy (98). If researchers in the field of resistance training use meta-analysis to confirm a preoccupation with a specific training protocol, despite the preponderance of resistance training studies reporting results that are threatening to their personal beliefs, it would be a disservice to the strength and conditioning community.

Genetic factors apparently have a very strong influence on how people respond to the exact resistance training protocol. That is, responses are primarily determined by genetics, not by specific training protocols. Acknowledgement of this genetic component is curiously missing from the previously discussed meta-analyses (2-4), training studies (5,51) and commentaries (30,54). As we have previously noted (12), Van Etten and colleagues (99) classified their resistance trainees (based on a fat-free mass index) as *solid* or *slender*. They reported a significant increase in fat-free mass in the *solid* group (1.6 kg); but there was no significant change in the *slender* group (0.3 kg). Van Etten and colleagues concluded that because all the participants followed the same resistance training protocol, the potential to increase fat-free mass was genetically determined.

Hubal and colleagues (100) trained 585 previously untrained males and females (age 18-40 years) at eight training centers for 12 weeks. The supervised progressive-resistance free-weight training consisted of 3 sets for each of 3 biceps exercises (9 sets) with varying resistance (12RM wk 1-4, 8RM wk 5-9, 6RM wk 10-12) and 2 minutes rest between sets. All their subjects followed the identical supervised protocol using identical equipment. Magnetic resonance imaging revealed an average 18.9% increase in biceps cross-sectional area (CSA); males (20.4 %) significantly greater than females (17.9 %). Of the 585 subjects, 232 increased CSA between 15 and 25 %. However, 10 subjects gained over 40%, and 36 subjects gained less than 5 %. The 1RM biceps curl increased an average 54.1 %; females (64.1 %) significantly greater than males (39.8 %). Of the 585 subjects, 232 increased their 1RM between 40 and 60 %. However, 36 subjects gained over 100 % and 12 subjects gained less than 5 %. The range of 1RM strength gains was from 0 % to 250 %. Hubal and colleagues concluded that males and females exhibit a wide range of responses to resistance

training, with some subjects increasing muscle size by over 10 cm^2 and doubling their strength (p.964).

An interesting aspect of the report by Hubal and colleagues (100) is that because of the differences in strength, some subjects used different training volumes (more weight) than others. However, their results showed that there was no relationship between training volume and increased muscle size for males or females. The 1RM was negatively correlated with training volume, which indicated that there was a bias toward higher relative gains in those with the smallest starting level of strength. This is apparently what happened in the previously discussed study by Rhea and colleagues (5). The initially weaker 3-set group got *relatively* stronger than the 1-set group in the leg press exercise, but the groups finished with similar levels of strength.

Thomis and colleagues (101) trained male twins (age 17-30 years) 3 times a week for 10 weeks. All the twins followed the same training protocol with a resistance (percent 1RM) relative to each person's maximal strength. Therefore, the relative stimulus for strength gains was similar among the participants. The average increase in 1RM was 45.8 %. The variation in strength gains among the 25 monozygotic (identical) twins was 3.5 times greater than within the pairs of identical twins. Thomis and colleagues confirmed the significant contribution of genetic factors to resistance training outcomes.

It is apparent that Rhea, Peterson and Alvar (2-5,30,51,54) are reluctant to report - or are unaware - that resistance-training outcomes are primarily dependent on genetic factors. Reporting the results of these revealing studies (99-101) would certainly undermine their unsupported, highly complex training philosophies.

As previously noted, one of the criticisms of meta-analysis is that the analysts do not devote the time required for the tedious task of performing a critical analysis of the included studies (10). For example, in a recent review of the specific topic of single versus multiple sets, Galvao and Taaffe (102) noted that the meta-analyses by Rhea and colleagues (2-3) found greater strength gains with multiple-set training. Apparently, they never critically analyzed these meta-analyses or the included studies. Galvao and Taaffe also claimed that the training study by Rhea and colleagues (5) reported a significantly greater increase in upper-body strength for the multiple-set group compared with the single-set group. A competent peer review would have revealed that Rhea and colleagues reported no significant difference in upper-body strength, and the peer reviewers could have challenged Galvao and Taaffe. In another publication, Galvao and Taaffe (103) discussed the manipulation of resistance training variables to enhance muscular strength and again they cited the study by Rhea and colleagues (5) as evidence for the superiority of multiple sets.

In a review on the fundamentals of resistance training, Kraemer and Ratamess (104) cited the training study by Rhea and colleagues (5) and their meta-analysis (3) in an attempt to support the superiority of multiple sets. Our critical analysis revealed that their claims were not supported by those references. They also cited the muscular endurance study by Rhea and colleagues (51) to support one type of *periodization* over another. As previously discussed, their results actually showed no difference in *periodization* protocols. In a review of resistance training variables, Bird and colleagues (105) cited the meta-analysis by Rhea and colleagues (3) to substantiate their belief that multiple sets produce superior results. In the President's Council on Physical Fitness and Sports publication entitled Progression and Resistance Training, Kraemer and colleagues (106) cited the study by Rhea and colleagues (5) and the meta-analysis by Peterson and colleagues (4) in an attempt to support their opinion that as one progresses to intermediate and advanced stages, multiple-set programs have been shown to be superior.

In the *ACSM's Health & Fitness Journal*, Sorace and LaFontaine (107) claimed that multiple sets are superior to a single set of each exercise and they cited the meta-analyses by Rhea and colleagues (3) and Peterson and colleagues (4). Interestingly, the Editor-in-Chief of that journal, who is also the Chairman-Elect of the President's Council of Physical Fitness and Sports Science Board, claimed in an editorial (108) that the review by Sorace and LaFontaine provided important references that back up their resistance training recommendations. Not, surprisingly, in their 7-page article, Sorace and LaFontaine cited the ACSM's evidence-deficient Position Stand (52)—42 times.

Galvao and Taaffe (102-3), Kraemer and Ratamess (104), Bird and colleagues (105), Kraemer and colleagues along with the President's Council on Physical Fitness and Sport Science Board (106), Sorace and LaFontaine (107), and the reviewers and editors for the respective publishing journals obviously failed in their obligation to perform a critical analysis of the original studies or the meta-analyses.

Peterson, Rhea and Alvar (109) recently squandered nine pages of the *Journal of Strength and Conditioning Research,* the official peer-reviewed research journal of the National Strength and Conditioning Association, to rehash their highly flawed meta-analyses (3-4) and their support for the discredited 2002 ACSM Position Stand (52). There was no new information except for some unsupported hypothetical narrative and a graph on *diminishing returns* and *marginal strength* (p.954-5). Peterson and colleagues, who described themselves as "ethical investigators" to the readers of the *Journal of Strength and Conditioning Research* (109, p.957), apparently believe that if they repeat themselves often enough, readers will eventually mistake their unsubstantiated opinions for science.

Peterson and colleagues (109) also claimed that training to failure, which they briefly described as repetition maximum (RM) training, does not elicit greater gains in strength than not training to failure - even when their comparisons involved multiple-set training (p.956). However, this is inconsistent with what they reported in Meta-analysis #3 (4). Without presenting any supporting data, they previously claimed that training to failure produced significantly greater magnitudes of strength gains (4, p.379). They also failed to note that their Figure 7 (109, p.955) showed approximately twice the effect size (ES = ~1.0 vs. 0.5 with 3 sets per muscle group, 1.1 vs. 0.6 with 4 sets per muscle group, and 1.45 vs. 0.8 with 6 sets per muscle group) as a result of not training to failure compared with training to failure. Peterson and colleagues did not report any data in Meta-analysis #3 (4) or in their current review (109) to support any of these conflicting claims. In addition, the reviewers, editor and publisher of these articles (4,109) failed to challenge these unsupported antithetical claims by Peterson and colleagues.

Peterson and colleagues (109) claimed that a meta-analysis provides an objective assessment of the effectiveness of a specific intervention through established, meticulous statistical guidelines. Peterson and colleagues also noted that it is imprudent to suggest that there is only one correct way to conduct a meta-analysis. We would argue that there *is* only one correct way to conduct a meta-analysis: critically analyze all the studies considered for inclusion or exclusion, accurately report the data from the included studies, meticulously apply the required statistical procedures, and determine whether the analysis has any logical, theoretical—or even hypothetical—physiological basis for support. Peterson, Rhea and Alvar did not fulfill any of these requirements in their meta-analyses (2-4).

It is disconcerting - but not surprising - that all the mathematical and statistical errors in these previously discussed meta-analyses (2-4) and training studies (5,51), as well as the reviews that cited those references for support (e.g., 102-107,109), continually elude detection by the peer-review

process of the publishing journals. The ethical problems in the peer-review process for exercise physiology journals have been previously noted (110) and specifically regarding resistance training (12,111-113).

Dr. Rhea and Dr. Alvar claimed that their meta-analyses (2-4) and training study (5) *unequivocally* showed an advantage to multiple-set training (NSCA 2004 and 2005 National Conference presentations). However, their data are inconsistent and illogical for any practical application to resistance training. Our critical analysis of their meta-analyses and training study does not substantiate their hypothesis that a greater volume of exercise is required for optimal strength gains.

DISCLOSURE

In the interest of full disclosure, one of us (Robert M. Otto) presented the counterpoint to Dr. Alvar's presentation of his three meta-analyses at the 2005 NSCA National Conference. In his counterpoint presentation, Dr. Otto revealed many of the aforementioned mathematical, methodological and statistical flaws in the three meta-analyses, as well as the absence of any logical practical application of their conclusions to resistance training. In the subsequent 30-minute question and answer period, neither Dr. Alvar nor Dr. Rhea attempted to defend any of the obvious flaws in their studies or meta-analyses.

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