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STRENGTH TRAINING FOR POLICE OFFICERS

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Abstract

A recent peer-reviewed article described resistance training programs for police officers and made several specific training recommendations that the author claimed were evidence-based. This review challenges his claims regarding numerous training variables such as exercise selection, order, volume, load, frequency, interset rest, and periodization. Science places the entire burden of proof on the author who makes specific claims or training recommendations and he must provide sufficient substantiating evidence for support. However, the majority of claims and recommendations in his article are unsubstantiated and consequently failed to meet the burden of proof.

Key words: *Strength training, burden of proof, meta-analysis, recommendations*

Introduction

As a retired police officer, I was especially interested in a recent article entitled Needs Analysis and Program Design for Police Officers by Rhea [1]. Rhea made a valid point that many of the most strenuous tasks performed by police officers may be preceded by relatively long periods of inactivity. Although foot patrol officers have been significantly reduced and perhaps non-existent in some departments, motor patrols usually minimize strenuous physical activity for most of the tour of duty. Rhea provided a good example of a vigorous foot pursuit after sitting several hours in a patrol vehicle and that it represented an extreme change in the officer's physiological state. Although these encounters used to be resolved in a few seconds with little or no harm to the police officer, political pressure has restricted or eliminated a very effective defensive tool –the nightstick – in many law enforcement agencies. Consequently, capturing, neutralizing the hostility, and restraining an uncooperative aggressive perpetrator may require several minutes of very intense physical exertion.

The terms *effect size* and *meta-analysis* are used throughout this review. Briefly, calculating an effect size is a statistical procedure that estimates how small or large the difference is between some pre-training and post-training outcome such as strength gains or the difference in outcomes between two or more experimental groups. A meta-analysis combines the effect sizes from several studies in an attempt to draw a conclusion from the combined results of those independent studies. The validity and practical application of meta-analysis has been repeatedly challenged by statisticians and those criticisms are noted in a later section of this document.

The Burden of Proof

Science places the entire burden of proof on those who make claims regarding specific training recommendations. The claimant has a responsibility to supply supporting evidence for the assertions. If there is a failure to present substantial supporting evidence, those assertions may be worthless. In science, the default position is that the assertion is not valid and that position is referred to as the null hypothesis. The claimant must provide substantial evidence for the null hypothesis to be rejected. This burden of proof is the foundation for the scientific method.

There is no burden of proof on those who challenge or demand evidence for an assertion or recommendation. It would be a daunting task if not impossible to prove a negative; that is, provide evidence for an absence of the efficacy for some training concept or philosophy. Rhea [1] made claims and recommendations that are described below. Unfortunately for police officers, his claims and recommendations have little or no supporting evidence.

Individual Needs Analysis

Rhea [1] listed several steps involved in a fitness program for police officers, which he begins with a complex technically-sounding *individual needs analysis*. Rhea did not present any credible evidence to suggest that a so-called needs analysis must be performed. An individual needs analysis implies that a 60-70 kg female patrol officer would require a training program that is significantly different from an 80-100 kg male patrol officer. Or that if the smaller female officer was relatively stronger (relative to body mass) or more powerful compared with a larger male officer, they should follow different resistance training programs. However, if both trainees chose a similar range of repetitions for a specific exercise (e.g., 8-10 repetitions for the bench press), the only difference in the training program between a smaller female (or a smaller male) and a larger male would probably be the amount of resistance (load) required to stay within the selected repetition range. An individual needs analysis may be required only to determine the exercises to be avoided with an injured muscle or some structural orthopedic limitation, which is simply common sense.

Exercise Selection

Rhea [1] claimed that the selection of exercises for each training session should be matched as closely as possible to the movement patterns during law enforcement tasks. The selection of specific resistance training exercises would allegedly enhance the transfer of fitness gains to specific law enforcement tasks. He claimed that specific modalities such as free weights, free-moving cable machines, medicine balls, kettle bells and elastic bands will transfer to law enforcement demands and that resistance training machines are not preferred because of their inherent artificial stabilization and fixed movement patterns. Rhea did not provide any evidence to support his opinion regarding the superior transfer of strength gains from any resistance training modality to specific law enforcement tasks or to any specific activity. It is this officer's opinion that strength gains from any training modality can best be transferred by actually practicing the tasks of subduing and restraining in the police academy and regular in-service tactical police training sessions.

Exercise Order

Rhea [1] claimed that exercises focusing on an officer's weaknesses should be performed early in the exercise session. However, he did not cite any evidence to suggest that the sequence of exercise has any chronic significant effect on outcomes such as strength, power or muscular endurance. In fact, only three studies [2-4] compared the chronic effects of different training sequences. Dias and colleagues [2] randomly assigned 48 young males to train five upper body exercises three times a week for eight weeks. One training group performed the exercises in a sequence that progressed from the larger muscle group exercises to the smaller muscle group exercises and another group progressed in the reverse sequence of exercises. Both training groups significantly increased 1-repetition maximum (1RM) strength in all five exercises. However, the only significant difference in strength between groups was a greater gain for the latter group in the biceps curl and elbow extension exercises. There was no significant difference in strength gains for any of the large muscle group exercises.

Simao and colleagues [3] trained 31 young healthy males two times a week for 12 weeks. The two training groups performed the bench press, lat pull-down, triceps and biceps exercises in opposite sequences for four sets of 12-15 repetitions (weeks 1-4), three sets of 8-10 repetitions (weeks 5-8) and two sets of 3-5 repetitions (weeks 9-12). All sets were performed to what the authors referred to as concentric failure. Both groups showed significant strength gains in all four exercises, with no significant difference between groups in strength gains for any exercise–regardless of exercise sequence. These two studies [2-3] reported only a couple of differences in strength gains in smaller muscle groups (biceps and triceps) and no significant difference in larger muscle groups. Rhea [1] neglected to cite any training studies to support his claim.

Fisher and colleagues [4] randomly assigned 41 resistance trained middle age females and males to one of three protocols that involved nine upper- and lower-body resistance exercises. One group had very little rest (<5s) between the isolated and compound exercises (e.g., pectoral fly and chest press); a second group performed the exercises in the same order but rested 60s between each isolated and compound exercise; and a third group performed the same exercises in a different order (compound exercises *before* the isolated exercises) with 60s rest between each exercise. All participants performed one set of 8-12RM (noted as concentric failure) two times per week for 12 weeks. They controlled for repetition duration while training and strength testing on the leg press, chest press, and pulldown exercises. The researcher assigned to pre- and post-training strength testing was blinded to the group assignment. All the participants significantly increased strength and there was no significant difference in strength gains among the groups for any of the exercises. Fisher and colleagues [4] concluded that the strength gains were independent of exercise order or rest intervals.

Training Load (resistance) and Repetitions

Rhea [1] claimed that training loads for maximal strength gains in an athletic population should be approximately 85% 1RM (repetition maximum). Of course that statement raises the question if one should classify most police officers as part of an athletic population. He cited a meta-analysis by Peterson and colleagues [5] in an attempt to support his claim. However, that meta-analysis has been shown to be highly flawed [6]. For example, although Peterson and colleagues defined their population as competitive college or professional athletes, they did not explain how to differentiate between non-competitive advanced resistance trainees and college or professional competitive athletes who may have little or no resistance training experience. Several of the studies included in their meta-analysis involved subjects who had not performed resistance training for several months prior to the specific study or had no prior experience with resistance training. They did not offer any physiological hypothesis to explain how these competitive male and female athletes from varied sports such as soccer, running, tennis, baseball, track and field, basketball, etc., who may not have performed any resistance training prior to the study intervention, would respond differently from other non-competitive novice resistance trainees.

More importantly, Peterson and colleagues [5] claimed that there was a trend for better strength gains as a result of training with a heavier resistance (85% 1RM) in these so-called competitive athletes. A *trend* would indicate an increase in strength gains as the percent 1RM increased (e.g., training with 80% 1RM should produce a greater strength gain than training with 75% 1RM). However, their own reported data failed to support that claim. The effect sizes for strength gains as a result of training with 70%, 75%, 80% and 85% 1RM were 0.07, 0.73, 0.57 and 1.12, respectively. Their data suggested that strength gains after training with 75% 1RM were 10 times greater than training with 70% 1RM, which were almost non-existent. Strength gains were less as a result of training with 80% 1RM compared with 75% 1RM, and the strength gains with 80% 1RM were almost doubled when training with 85% 1RM. The illogical implication was that the reported dramatic differences in strength gains were the result of training with only a 5% difference in resistance.

To clearly and simply illustrate the absurdity of the aforementioned effect sizes generated by Peterson and colleagues [5], one would have to accept the premise that if the 1RM biceps curl was 50 kg and the trainee used 37.5 kg (75% 1RM) versus training with 35 kg (70% 1RM) and exerted a similar effort on the final repetition of the set, the 37.5 kg load would produce 10 times greater strength gains than the 35 kg load. Peterson and colleagues never attempted to explain how a very small difference in the resistance could produce such an unbelievably large difference (tenfold) in strength gains. Consequently, the many flaws in their meta-analysis prevent any rational conclusion regarding different strength gains with specific training loads.

The only other reference Rhea [1] cited regarding the amount of resistance and number of repetitions was a book by Baechle and colleagues [7]. He did not cite any resistance training studies to support his claim. It is important to note that an extensive review by Jungblut [8] reported that only eight studies showed some advantage for a few exercises as a result of training with a heavier resistance. However, 82 resistance training studies in her review [8] reported no significant difference in strength gains as a result of training with a heavier resistance. Based on the size principle of motor unit recruitment [9] and the preponderance of resistance training studies, Jungblut concluded that effective resistance training simply requires the selection of a reasonable range of repetitions (based on a personal preference) such as 4-6RM, 6-8RM, 8-10RM, 10-12RM, 12-14RM, etc., and progressing with the resistance to stay within the desired range of repetitions. The evidence strongly suggested that the degree of effort during a set of repetitions is the primary stimulus for strength gains rather than the amount of resistance [8-9]. Rhea's failure to cite any studies that support his claim regarding a heavier training resistance precludes any logical conclusion.

Most recently, Alegre and colleagues [10] trained the knee extensors (unilateral knee extension and leg press exercises) in 15 previously untrained young females three times a week for 10 weeks. All the trainees performed three unilateral sets of each exercise with both limbs. The resistance in one limb progressed from 50% 1RM to 80% 1RM by the 4th week. The resistance remained at 50% 1RM in the contralateral limb and the amount of work for both limbs was equalized in every set for the duration of the study. Muscular size (rectus femoris and vastus lateralis), whole body lean mass, isometric and dynamic strength significantly increased in both limbs. There was no significant difference between limbs for any outcome measures. The authors noted that because the participants trained both limbs, there was the possibility of cross-over effects. However, if these effects did exist, they would have been bilateral with limiting biasing potential as demonstrated in the nearly identical strength gains and similar degrees of muscular hypertrophy in both limbs. The researchers noted also that the size principle states that the development of maximal force requires the recruitment of the largest, strongest motor units; however, the reverse conclusion that near maximal forces are required to recruit the larger motor units is not supported in the literature—as previously noted by Jungblut [8] and Carpinelli [9]. Alegre and colleagues [10] concluded that with resistance training, „*...it is the degree of muscular effort that determines the extent of motor unit recruitment rather than the mass of the load being lifted*" (p. 99).

Volume of Exercise

Rhea [1] recommended an average of eight sets for each muscle group and eight to twelve sets for each *exercise* when performing complex exercises such as squats, deadlifts, and the clean and jerk to produce maximal strength development. The only references he cited were the aforementioned book by Baechle and colleagues [7] and the meta-analysis by Peterson and colleagues [5]. Peterson and colleagues claimed that their data demonstrated the added strength benefits of higher training volumes. However, the data in 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. They did not indicate the source of those effect sizes or how many studies produced those data. They also reported a lower mean effect for 12 sets per muscle group (ES = 0.69) followed by a higher mean effect (ES = 1.06) for 14 sets per muscle group. Peterson and colleagues did not attempt to explain their reported random pattern of a greater, lower, and then a higher mean effect (strength gains) with increased training volume.

A simple example to demonstrate the absurdity of any practical application of performing eight sets of each exercise is if a trainee were to choose two exercises for a muscle group and perform eight sets of each exercise with two minutes rest between sets and exercises, the total time required would be approximately 48 minutes for each muscle group (16 sets @ 1 minute per set + 2 minutes rest between each of 16 sets = 48 minutes)—an irrational training recommendation at best. However, one set for each of two exercises would require only about four minutes per muscle group.

Peterson and colleagues [5] also claimed that experienced trainees adapt to lower training volume and must increase their training volume to elicit a continued overload on the neuromuscular system. They cited only one training study [11] in an attempt to support that claim. Hakkinen and colleagues [11] trained the knee extensors in 11 physically active males for 24 weeks. They emphasized the importance of training intensity for maximal neural activation in subjects with previous strength training experience. However, 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.

It may be worth noting that at the 2004 National Strength and Conditioning Conference, Rhea gave a presentation entitled *The Dose-Response for Strength Development: Scientific Evidence and Practical Applications* [12]. He expressed his strongly-held belief that as a trainee becomes stronger and stronger, the volume of exercise required to provide an adequate stimulus for adaptation must be greater and greater. Perhaps Rhea has misinterpreted a greater volume of exercise to mean exclusively a greater number of sets. However, the volume of exercise (resistance x reps x sets) can be increased (if necessary) by simply increasing the resistance to stay within the desired range of repetitions. Rhea failed to adequately support his opinion that a greater number of sets is required for an optimal stimulus.

Rhea [1] and Peterson and colleagues [5] failed to meet the burden of proof for their claims that a greater volume of training is required for maximal strength gains and that further progression to higher training volume (a greater number of sets for each exercise) is required for continuous strength gains.

Interset Rest

Rhea [1] recommended 2-5 minutes rest between sets for optimal strength development but cited only the book by Baechle [7]. He did not cite any resistance training studies to support his recommendation. Several studies have reported a significant reduction in the number of repetitions during the performance of multiple sets with some specific exercises when shorter versus longer interset rest intervals were employed (see reference #13 for a detailed description of those studies). However, only three out of the nine training studies cited in that review [13] reported significantly greater strength gains for a few exercises after employing longer interset rest intervals compared with very brief interset rest intervals. As noted in previous sections, Rhea did not fulfill his burden of proof.

Training Frequency

Based only on the meta-analysis by Peterson and colleagues [5], Rhea [1] recommended training each muscle group 2-3 times a week. Peterson and colleagues did report very similar effect sizes for two times (0.70) and three times per week (0.69) training. Curiously, both of Peterson's co-authors (Rhea and Alvar) were the lead authors of another meta-analysis [14] published one year prior and they reported that advanced trainees produced twice the effect size (strength gains) after training only two times a week compared with three times a week (ES = 1.4 and 0.7, respectively). Peterson, Rhea and Alvar did not address the conflicting results of their two meta-analyses.

Periodization

Resistance training periodization refers to varying the training protocol at particular time intervals in an attempt to produce specific optimal physiological adaptations such as muscle hypertrophy, increased strength, power, endurance, etc. [15]. Linear periodization varies one or more of the training variables (e.g., sets, repetitions and resistance) every couple of weeks or months (so-called mesocycle) and remains relatively constant throughout that phase of training. Nonlinear or undulating periodization changes one or more training variable from workout to workout or from exercise to exercise during each week of the training [16].

Rhea [1] stated that a resistance training program should be periodized (specifically programmed variability in resistance, repetitions, frequency, time under load, interset rest intervals, sequence of exercises, etc.) and he cited only a meta-analysis by Rhea and Alderman [17] in an attempt to support his claim. Rhea and Alderman cited 11 studies [18-28] in the first part of their meta-analysis that they claimed compared so-called periodization with non-varied traditional resistance training. The details of those studies are discussed in another review [29] and are briefly summarized below. Readers can decide on the credibility of these 11 studies that Rhea and Alderman included in their meta-analysis.

Baker and colleagues [18] randomly assigned 33 young males to one of three training programs: linear periodization, undulating periodization or a traditional group (no variation in the number of repetitions, sets, etc.). After training three times per week for 12 weeks, the three groups showed a significant increase in 1RM bench press, 1RM squat, vertical jump, and lean body mass. There was no significant difference among the groups for any of the measured variables. Baker and colleagues concluded that strength gains and changes in body composition were similar for periodization and traditional resistance training.

Herrick and Stone [19] randomly assigned 22 young females to a traditional progressive resistance or periodization program, two times per week for 15 weeks. Both groups significantly increased 1RM bench press and squat, with no significant difference in strength gains between the traditional and periodization groups. Herrick and Stone concluded that their results did not support any benefit of periodization over traditional progressive resistance training.

Kraemer [20] published a series of so-called *experiments* in the Journal of Strength and Conditioning Research. As Editor-in-Chief of that journal, he resurrected the data for these *experiments* from a database that he accumulated as a coach at least 15 years prior to their publication. Kraemer claimed in his Introduction that the answers to important training questions were initially determined through his role as a coach, which revealed his bias prior to the analysis and publication of his series of *experiments*. Rhea and Alderman [17] never addressed these issues and the reviewers and editor (Kraemer) failed to impose rigorous standards of acceptance and publication. Consequently, all the data from Kraemer's *experiments* are questionable at best. The fact that Rhea and Alderman included that data in their meta-analysis certainly questions the validity of their inclusionary criteria.

Kraemer and colleagues [21] trained young females with a single set or varied multiple set program 2-3 times per week for nine months. Although they did not report data for absolute or percent change pre- to post-training, they claimed that the periodization group increased 1RM bench press, leg press and military press approximately three times greater than the single set group and that these female athletes in the multiple set group purportedly gained more than three kg of lean body mass. Curiously, the previously untrained females in the single set group were not able to produce a significant change in any of the performance variables (strength and body composition).

Kramer and colleagues [22] randomly assigned 53 young males to a 1-set, 3-set, or varied multiple-set group who trained three times per week for 14 weeks. All the groups significantly increased the 1RM squat. However, there was no significant difference in strength gains between the 3-set group and the periodization group and there was no significant change in body mass or body composition in any group.

Marx and colleagues [23] trained previously untrained young females with a single-set or varied multiple-set program 3-4 times per week for six months. The multiple-set group had a significantly greater increase in 1RM bench press and leg press compared with the single-set group. However, the authors speculated that the results could have been attributed to experimental differences between groups in exercise volume, repetition duration, amount of resistance, resting hormonal levels, and exercise selection (e.g., barbell cleans and squats only in the multiple set group).

Rhea and Alderman [17] referenced the study by McCarthy [24] as an unpublished doctoral dissertation, which actually was an unpublished Master's thesis. McCarthy [24] trained 24 male high school wrestlers two times a week. The traditional resistance training group performed three progressively heavier sets of 10RM, 8RM and 6RM barbell curls during each exercise session for 10 weeks. The periodization group gradually progressed from one 90-rep set (per workout) with 30% 1RM weeks 1-3, one 60-rep set with 40% 1RM weeks 4-5, one 30-rep set with 50% 1RM week six, and then adopted the aforementioned traditional training program for the remaining four weeks. It is important to note that the traditional training group is actually a form of periodization because the resistance and the number of repetitions varied from set to set. In fact, this study actually compared two types of periodization: linear periodization and daily underlating periodization, which was called the traditional resistance training group. Eight additional wrestlers (controls) did not perform barbell curls. Although the traditional and periodization groups got stronger (28.6% and 24.4%, respectively), the strength gain in the periodization group was not statistically significant. The strength gain was statistically significant only in the traditional resistance training group.

McGee and colleagues [25] assigned 29 young males to perform the squat exercise using one of three protocols: single set, multiple set, or varied multiple set protocols three times per week for seven weeks.

The authors noted there was no significant difference among the groups in any post-training outcome.

Schiotz and colleagues [26] randomly assigned 22 young male Army volunteers to a periodization or traditional resistance-training program. Both groups performed the bench press, squat and seven other exercises two times per week for seven weeks. There was no significant difference between groups in strength gains or muscular endurance and no significant increase in lean body mass in either group. Nor was there any significant difference between groups in the four Army Ranger Challenge physical performance tests. Schiotz and colleagues concluded that trainees could follow either a periodization or a traditional resistance-training program and would produce similar improvements in muscular strength and performance.

Stone and colleagues [27] trained 21 young males with either a traditional resistance training program or one of two varied multiple-set resistance-training programs two times per week for 12 weeks. Although they claimed that the periodization resistance-training programs produced superior results compared with the constant-repetition program, they did not report any between-group statistical comparisons. In addition, four out of the five subjects in the single set group dropped out of the study leaving only one subject in that group for evaluation, which precluded any practical application of this study.

Willoughby [28] trained 48 young males who performed three sets of 10 RM, three sets of 6-8RM or a varied multiple set (3-5 sets) protocol two times per week for 12 weeks. He did not report absolute or percent change in strength and no post-training values for body mass. Because it is not known if body mass changed as a result of the resistance training, neither the absolute nor percent change in strength can be calculated from the published data. Rhea and Alderman [17] claimed that one of their inclusion criteria was that each study must have reported baseline and post-intervention strength values, which raises the question of how they were able to calculate effect sizes for strength gains without that data.

None of these inclusive studies [18-28] in Rhea & Alderman's meta-analysis [17] followed a double-blind or single-blind protocol for the trainees or the trainers, and more importantly, the evaluators who tested the outcome variables (e.g., strength gains) were not blinded to the specific training protocol. Thompson and Pocock [30] have noted that the value of any meta-analysis is totally dependent on the lack of bias in its inclusionary studies and that the meta-analysis should be restricted to inclusion only of randomized studies that provide a blinded outcome assessment.

Rhea and Alderman [17] concluded that males and females of all ages and training experience will experience greater strength gains with periodization programs. However, out of the 11 studies [18-28] that Rhea and Alderman cited in the first part of their meta-analysis, only two questionable studies [21, 23] reported significantly greater strength gains from periodization training. Therefore, Rhea and Alderman's conclusion is not sufficiently supported by the data from the studies in their meta-analysis.

There were 105 inclusive references in the second part of Rhea and Alderman's meta-analysis [17]. They claimed that they calculated effect sizes from studies that used either periodization or non-periodization training programs. However, that claim is misleading. It appears that Rhea and Alderman simply picked some studies that investigated the outcomes of resistance training; for example, studies that compared a nutrient supplement with a placebo, or compared single versus multiple sets. None of those 105 studies directly compared periodization with non-periodization training. Overall, both parts of their meta-analysis failed to meet the burden of proof.

Proponents of periodization training claim that a programmed variety of sets, repetitions, volume, load, rep duration, rest intervals, frequency, etc. creates a constantly varying stimulus that results in superior strength gains [1, 17, 31] and that the changes in these acute training variables are required for long term progression. In fact, the authors of the American College of Sports Medicine position stand on resistance training [31] cited only the previously discussed meta-analysis by Rhea and Alderman [17] in an attempt to support their claim that periodization produces superior strength gains compared with non-periodized training.

It is presumptuous—if not absurd—to believe that planned complex manipulations of acute training variables (so-called periodization) could influence varying physiological adaptations at specific time intervals; for example, that different ranges of repetitions or the amount of resistance could elicit specific chromic adaptations at a specific time in training. There is a lack of evidence to suggest that the neuromuscular system responds differently to these training variations and very little evidence to support their claim for superior strength gains [29].

Rhea [1] claimed that a significant body of evidence exists that examined the proposed benefits of periodization. Actually, the meta-analysis by Rhea and Alderman [17], which was the only reference Rhea cited, provided very little evidence to support the superiority of so-called periodization over traditional progressive non-varied resistance training.

There are several studies [32-37] worth noting that reported no significant difference in strength gains as a result of traditional strength training and so-called periodization. For example, DeBeliso and colleagues [32] randomly assigned 60 older previously untrained males and females to a control group (no resistance training) or one of two progressive resistance training groups: 1) fixed resistance (three sets of each exercise with a 9RM resistance); or 2) linear periodization (two sets of 15RM weeks 1-6, three sets of 9RM weeks 7-12, and four sets of 6RM weeks 13-18). Trainees performed three lower body and five upper body machine exercises two times a week for 18 weeks. Exercise sequence, machines, rest between sets, frequency and total exercise volume (sets and repetitions) were identical for the two training groups. Strength gains were significant for each of the eight exercises and for total body strength (sum of the eight exercises). However, there was no significant difference in strength gains between the training groups for any exercise or total strength. DeBeliso and colleagues concluded that complex manipulations of training resistance (RMs) were not required for optimal strength gains in their study.

In a similar study, Hunter and colleagues [33] randomly assigned 30 previously untrained older males and females to a control group or one of two progressive resistance training groups: 1) fixed resistance (80% 1RM resistance); or 2) undulating periodization (50%, 65% and 80% 1RM for the three successive weekly sessions). Trainees performed two sets on each of the 8-10 machine exercises three times a week for 25 weeks. Both training groups significantly increased strength (1RM) on the four exercises tested (two upper body and two lower body exercises). However, there was no significant difference in strength gains between the training groups on any of the exercises. The fixed resistance (traditional) and periodization protocols produced similar strength gains.

In a recent study Fonseca and colleagues [34] randomly assigned 70 physically active young males to a control group or one of four resistance training groups who; 1) varied the loading scheme (amount of resistance and number of repetitions) but not the exercise (squat); 2) varied the loading scheme and the exercises (squat, leg press, deadlift, and lunge); 3) did not vary the loading scheme but varied the exercises; or 4) did not vary the loading scheme or the exercise. The four training groups performed a similar volume of exercise two days per week for 12 weeks and they all significantly increased strength (1RM squat), which ranged from 23% to 53%. Interestingly, the group that did not vary the loading scheme but did vary the exercises produced the greatest strength gains (53%); that is, varying the exercises elicited greater strength gains than varying the loading scheme. As determined by magnetic resonance imaging, all the training groups significantly increased muscle cross-sectional area but the groups who varied the exercises produced a more homogeneous muscle hypertrophy of the quadriceps. Fonseca and colleagues [34] concluded that the variation in loading scheme (periodization) was not as effective as simply varying the exercises.

In another study, Hoffman and colleagues [35] randomly assigned 51 young resistance trained collegiate American football players to one of three training groups: 1) non-periodized (no manipulation of resistance load); 2) linear periodization (increasing resistance load and decreasing volume in a linear progression over the 15-week study); or 3) nonlinear periodization (resistance load differed from workout to workout over the 15 weeks). Trainees performed multiple sets of 6-17 exercises in a 4-day per week split routine for 15 weeks. All groups significantly increased the free weight bench press and squat 1RM with no significant difference in strength gains among the groups for either exercise.

Pacobahyba and colleagues [36] randomly assigned 24 young male soccer athletes to perform 10 upper- and lower-body resistance exercises for 12 weeks. One group trained with three sets of 10RM for each exercise three times a week (non-periodization group) and another group (periodization) trained with three sets of 4-6RM, 8-10RM and 12-15RM for each exercise on the first, second and third training days, respectively, each intervention week. They reported that there was no significant difference between groups in strength gains.

Souza and colleagues [37] randomly assigned 31 young athletes to a control group or one of three resistance training groups who performed isolated knee extension and squat exercises two times per week for six weeks. The non-periodized group followed the same protocol (2-3 sets of 6-12RM) for the duration of the study. The two periodization groups (traditional and underlating) executed 2-4 sets of 6-12RM. The resistance (RM) and the number of reps varied throughout the duration of the study. The volume of training was similar for the three training groups. Lower body strength significantly increased in the non-periodized and underlating periodization groups, with no significant difference in strength gains between those groups. Quadriceps cross-sectional area significantly increased in the three training groups, with no significant difference among these groups for the increase in muscle hypertrophy.

The six aforementioned studies [32-37] that compared nonperiodized to periodized resistance training and reported no significant difference in measured outcomes between protocols are curiously missing from Rhea's so-called needs analysis [1].

The term *confirmation bias* refers to selectively choosing or giving undue weight to evidence that supports one's belief while neglecting to gather or discounting evidence that opposes that belief [38]. It is a type of selective thinking whereby one tends to notice and to look for what confirms one's beliefs, and to ignore or undervalue the relevance of what contradicts those beliefs. Confirmation bias is a phenomenon wherein people who hold strong beliefs or opinions have been shown to actively seek out and assign more weight to evidence that confirms their hypothesis, and ignore evidence that would fail to confirm their hypothesis. There is a fine line between deliberate selectivity of evidence and the unwitting molding of claims to support a specific belief. Discerning readers can decide which authors, reviewers and editors are guilty of confirmation bias.

Comments on meta-analysis

The statistical process of meta-analysis is not without controversy. For example, the inclusion or exclusion of the studies in a meta-analysis is entirely based on the discrimination, opinions, and potential inherent bias of the statistician conducting the meta-analysis. The inclusive studies will have a profound effect on the results of the meta-analysis because the data from those studies may be the consequence of methodological flaws or fraud [39]. The validity of a meta-analysis can only be as good as the quality of the inclusive studies because a meta-analysis inherits all the flaws of the inclusive studies. If there was bias within the inclusive studies (as noted with Kraemer's *experiments* [20]), the meta-analysis will reinforce that bias. Feinstein [40] has noted that any bias may result in a misguided impression, and perhaps a delusion; that is, a meta-physical conclusion rather than meta-analytic. Eysenck [41] also has noted that the results of good, bad or indifferent studies are fed into a computer with the hope that people will stop caring about the quality of the material on which the meta-analyst bases his conclusions.

Combining poor quality data or overly biased data that do not make sense produces unreliable results [42]. This also is known in statistics as garbage in, garbage out. Charlton [43] noted that a meta-analysis is not a replacement for substantive knowledge.

It is worth noting that the meta-analysis was promoted by a statistician named Glass [44]. While earning his doctorate, Glass developed what he called a major neurosis and he truly believed that psychotherapy helped him cope with his mental problems. However, most scientific studies concluded that there was no significant difference in outcomes as a result of psychotherapy compared with a placebo treatment. Subsequently, Glass stated that he found these results to be personally threatening and he admitted that he used a meta-analysis to confirm his belief in psychotherapy [45]. The results of that meta-analysis were published in a psychology journal; however, Glass failed to reveal his preconceived bias for the efficacy of psychotherapy [46]. This incident with Glass clearly demonstrates that those who have very strong beliefs may seek (consciously or sub-consciously) confirming evidence for support and ignore or misinterpret evidence that does not support their belief [47].

It is also worth noting that Rhea [48] has previously proposed elevating the scale for interpreting effect sizes in resistance training studies. However, his suggestions were not supported with any data or logical rationale and are contrary to contemporary statistical standards.

Reviewers assigned to a meta-analysis may rarely have the time to perform the tedious task of reviewing any of the inclusive or exclusive studies. Bailar [49] has stated that very few reviewers and editors carefully read and interpret a sufficient sample of the original inclusive studies, which he noted is one reason there are so many poor meta-analyses in the literature.

Shapiro [50] stated that the meta-analysis may succumb to its own absurdity and that the next step may be a meta-analysis of several meta-analyses. Consequently, the meta-analyst may become totally divorced from reality and be totally surrounded by numbers without any context. A good example of disconnect from reality and numbers with context is the aforementioned meta-analysis by Peterson and colleagues [5]. They failed to address the previously discussed inexplicable pattern of effect sizes (strength gains) with 70, 75, 80 and 85% 1RM. Shapiro believed—as does this author—that the meta-analysis is a bad concept and he accurately described a meta-analysis as numerological abracadabra [51].

Fitness Testing

A 1RM is the amount of resistance that can be moved in good form through the entire range of motion only one time at a specific repetition duration for a particular exercise. Rhea [1] presented a table that he labeled a tactical fitness testing battery, which contains tests that are either unnecessary or irrelevant to police work. For example, it has been clearly shown that 1RM tests are not required to measure strength gains [52]. There is a direct relationship within an individual for a specific exercise between muscular strength and performing a maximal number of repetitions with a submaximal resistance, and that relationship does not change significantly as a result of resistance training. Therefore, unless the trainee is a competitive weightlifter or powerlifter, there is no need to test the 1RM for any exercise. Simply determine the amount of resistance for the selected range of training repetitions such as 6-8, 8-10, etc. and compare the pre-training and post-training resistance for that exercise at a specific repetition duration (e.g., 3s concentric, 3s eccentric); that is, the percent increase in resistance—muscular strength gains.

The percent increase in resistance for a given number of repetitions for any exercise such as the bench press is also a measure of muscular endurance [52]. For example, a 10% increase in the resistance required for 8-10 repetitions is equivalent to an approximately 10% increase in muscular endurance. This eliminates the need for a pushup test, which usually does not control for repetition duration. Not controlling for repetition duration during testing can have a profound effect on the number of pushup repetitions performed. Several studies [53-57] have shown that that the number of completed repetitions is greater with shorter repetition duration (faster movement) than with longer repetition duration (slower movement). For example, LaChance and Hortobagyi [54] tested 75 moderately trained young males for the maximal number of push--ups at three repetition durations: self-paced (~1.2s total time for the concentric and eccentric muscle actions), 2s concentric, 2s eccentric muscle actions, and 2s concentric, 4s eccentric muscle actions. There was a significant difference in the number of completed repetitions among the three repetition durations, with shorter durations eliciting the greater number of repetitions (self-paced > 2s:2s > 2s:4s). Consequently, any pre- and post-training push-up test without control for repetition duration may be highly inaccurate.

Conclusions and Recommendations

Rhea [1] claimed that it was common for law enforcement officers to be provided time to exercise while on duty. However, with the exception of a few small departmental divisions such as special weapons and tactics, emergency services, air and marine bureaus, most police departments require their officers to *protect and serve* their citizens while on duty. Therefore, time efficient training sessions should be a priority for officers who must exercise on their own off-duty time.

In sharp contrast to the complex, irrational, time consuming program that Rhea [1] recommended and based on the preponderance of scientific evidence, this author recommends the following brief, simple, feasible, and time efficient resistance training recommendations for other law enforcement officers. Interested readers may refer to several excellent reviews [9, 29, 58-62] for detailed descriptions of hundreds of supporting resistance training studies.

- Choose 10-12 exercises to stimulate the major muscle groups. Upper body exercises should be followed by a grip strengthening exercise. Inadequate grip strength may be the weak link between an otherwise strong officer and the ability to quickly and effectively subdue and restrain an uncooperative perpetrator.
- Select one or two free weight or machine exercises for each muscle group that provide resistance throughout a pain free range of motion (e.g., one single joint and one compound exercise such as a lateral raise and military press for the deltoids). Despite strong opinions, there is a lack of evidence to support the superiority of free weights or machines.
- Use a repetition duration that is conducive to maintaining consistent good form throughout each repetition (e.g., 3 s lifting, 3 s lowering the resistance). The trainee should be in total control of the resistance throughout every repetition of each exercise.
- Choose a range of repetitions between 3 and 20 (for example, 3-5, 6-8, 9-12, etc), which may vary from exercise to exercise or session to session. This is a personal preference because there is a lack of evidence to support one range of repetitions over another (e.g., 3-5RM versus 8-10RM) for any specific outcome such as muscular size or strength.
- Continue each exercise until it becomes difficult to maintain proper form during the concentric phase of a repetition. The level of effort required for optimal strength gains is unknown. However, a higher level of effort ensures maximal motor unit recruitment.
- Progressively increase the resistance when necessary to maintain a high level of effort and to stay within the desired range of repetitions.
- Eventually most dedicated trainees will reach their genetic potential for specific outcomes such as muscular strength and hypertrophy, and there is a natural age-related decline in those attributes. However, if they can perform the same exercise with the same resistance using identical form (e.g., range of motion, repetition duration, etc.) in future training sessions (months, years), they will have progressed in their training relative to their age. Any form of progression is relative to some baseline.
- Breathe as normally as possible throughout the set of repetitions. Try not to hyperventilate or hold breath.
- Perform one set of each exercise. There is very little evidence to suggest that multiple sets of each exercise are superior to a single set for strength gains or muscular hypertrophy.
- Allow enough rest between exercises to execute proper form for every repetition.
- Each whole body workout session should require only about 20-30 minutes.
- Depending on individual recuperation and response, train each muscle group approximately two times per week (e.g., three times per week for novice trainees, two times per week for advanced trainees, one time per week to maintain strength gains).
- Trainees who wish to vary their training routine for psychological reasons may simply modify any of the aforementioned training variables—including different exercises—from session to session or

exercise to exercise. Police officers could *periodize* their regularly scheduled training days by taking a break from training during their vacation. It's not rocket science.

- Good nutrition and improved aerobic capacity are beyond the scope of this review but certainly should be a part of a training program.

These simple time efficient recommendations are more likely to be adopted, promote long-term adherence, and produce significant favorable adaptations in physical fitness (e.g., muscular size, strength, power, endurance, bone density, body composition), health (e.g., cardiovascular risk factors such as lipids, insulin resistance, blood pressure), reduced injuries (e.g., lower back pain and disability), and quality of life (e.g., improvement and maintenance of functional ability) in the general population as well as police officers [63-66].

A perpetrator is less likely to challenge a fit, confident police officer who is more capable of responding to physical challenges such as grappling with or overcoming a fleeing suspect on foot.

In addition, stronger officers are less likely to sustain injuries in the line of duty [67]. Similar to other predators, human predators (criminals) size up their potential prey and in many circumstances attack the police officer who appears most venerable. Fit police officers may reduce their chances of being the prey. Appearance counts.

Rhea [1] claimed that all his previously discussed assertions for resistance training were evidence based recommendations. However, he did not support his recommendations with peer-reviewed resistance training studies and consequently failed to satisfy the burden of proof. Science dictates that these types of unsubstantiated assertions are nothing short of pseudoscience or junk science—at best, or simply nonsense.

Author disclosures

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TRENING SIŁOWY DLA POLICJANTÓW

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Abstract

Dokonano przeglądu ostatnio opublikowanego artykułu opisującego programy treningu siłowego dla policjantów oraz zawierającego konkretne zalecenia treningowe, które według jego autora oparte są na dowodach (evidance-based).

Niniejsza praca/recenzja ma na celu przyjrzenie się założeniom i twierdzeniom autora dotyczącym wielu zmiennych treningowych, takich jak: odpowiedni dobór ćwiczeń, kolejność wykonywania, ich ilość, obciążenie i częstotliwość, przerwa między powtórzeniami jak i podział na fazy/serie.

Zgodnie z założeniami EBM (medycyny opartej na faktach) wszystkie postawione przez autora tezy oraz wypracowane zalecenia treningowe muszą być udowodnione naukowo, opierać się na faktach i mieć swoje odwzorowanie w wynikach przeprowadzonych badań.

Większość twierdzeń i zaleceń przedstawionych we wspomnianym artykule jest bezpodstawnych, tym samym nie spełniają one założeń EBM..

Key words: *trening siłowy, ciężar dowodu, meta-analiza, zalecenia*

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