Response to "Berger in retrospect: effect of varied weight training programmes on strength"

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I would not have believed in 1962 that my study¹ would have created such a brouhaha in the 21st century. Dr Carpinelli’s paper² credits my study as "the genesis of the unsubstantiated belief that multiple sets are required for optimal gains in strength". His opinion is complimentary in one respect, but I cannot take full credit for it. Most professionals in the fields of athletics and therapy have added credence and support to these words by employing multiple sets in their practice and research. In my opinion, most professionals train others with multiple sets because they have experientially discovered that multiple sets are more effective than one set. Some early research studies have compared different weight training programmes, but in practically all studies multiple sets were used in training.³⁻⁷ I am hardly the "genesis" of an "unsubstantiated belief". Historically the medical applications of strength training for therapy involve multiple sets. One set is the exception. So Berger is not as one "crying in the desert". There are many more therapists and coaches flinging down the gauntlet in support of multiple sets.

The probability level of 0.05, which academics hold so sacred in decision making, does not always supersede in importance common sense when considering the difficulties in experimentally attempting to control extraneous factors in strength research. One research problem is finding subjects who have had, preferably, no experience in weight training and who are able to train for long periods of time, well beyond 12 weeks, under controlled conditions. If I had concluded in my study in 1962 that one set was as good as multiple sets, I would have had more than just Dr Carpinelli voicing criticism of my paper. The practitioners in the field would have confronted me years ago to express their disagreement and would not have waited 40 years to do so.

A person who comes to my mind as one having promoted single sets in past years is Arthur Jones, the developer of the Nautilus machine. To my knowledge, he has never presented any acceptable scientific evidence supporting his belief. Furthermore, he has few adherents today of his training views, although one adherent is obvious. Of the 85 references in Dr Carpinelli’s paper, Mr Jones authored not one. Certainly his contribution to the body of knowledge in strength training should be recognised, if deserving.

I decided to deal with a limited amount of "evidence" in defence of my study. But I must preface my remarks by assuring the readers that my paper was reviewed by several researchers at the time of acceptance and approved by them for publication. The conclusions I drew were substantiated and
accepted by them. For Dr Carpinelli to refer to my study as the "genesis of the unsubstantiated belief . . ." runs counter to the opinions of these reviewers.

The data in tables 1, 2, and 3 of Dr Carpinelli’s paper, which were used to critique my study, were inappropriately used according to acceptable statistical protocol. Comparisons between subgroups 1-2, III-6, etc were not valid for critiquing my study. When a factorial design is used, as in my study, and no significant interaction is found between factors of sets and repetitions, then the only legitimate analysis to make is on main effects—that is, comparisons among sets 1, 2, and 3 across all levels of repetitions, and among repetitions 2, 6, and 10 across all levels of sets. When this was done, significant differences were found, with three sets and six reps resulting in the greatest improvement. I spoke to Dr Carpinelli earlier (1998 communication) about his misuse of statistics and suggested he consult a statistician. If this had been done, there would not have been a critique of my study, nor a need for one. I must admit, though, that I made the same mistake as Dr Carpinelli in my study. In table 4 of my study, I erroneously made comparisons among subgroups of sets and repetitions. However, as a neophyte in 1962 I accept the blame. Being wiser today than 40 years ago, and even considering Dr Carpinelli’s critique, I unequivocally support multiple sets over single sets for optimising strength. I would suggest to Dr Carpinelli that he conduct research of his own in the hope of gaining support for his position. If his zealousness, which is commendable, were redirected to research rather than to critiquing old studies, his academic contributions would be more fruitful.

References


Science versus opinion

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Dr Berger recently responded1 to my article in the British Journal of Sports Medicine entitled: Berger in retrospect: effect of varied weight training programmes on strength.2 Dr Berger presented no scientific evidence to support his opinion on single versus multiple sets, cited references that were irrelevant to the topic, and challenged the reported statistical analyses.

Dr Berger claimed that most athletic and therapeutic professionals have added credence and support to the belief that multiple sets are required for optimal gains in strength because they use multiple sets in their practice and research (p 372).1 However, just using multiple sets in practice is not evidence that multiple sets are superior to a single set; it merely perpetuates an unsubstantiated belief. Table 4 (p 322) in my retrospect2 lists 57 studies that reported no significant difference in strength gains as a result of performing a greater number of sets. Dr Berger3 failed to cite a single study to support his opinion on the superiority of multiple sets. In fact, Dr Berger’s own follow up studies,4,5 which are described in my retrospect,6 failed to support his opinion—that is, a greater number of sets did not result in a significantly greater strength gain in either of his follow up studies.4,5

Dr Berger claimed that some early studies compared different weight training programmes (p 372),1 and he cited five studies.6–10 However, these studies (described below) are irrelevant to the issue of single versus multiple set resistance training.

Delorme et al6 treated eight men and 11 women with poliomyelitis-weakened and atrophied knee extensor muscles. Their goal was to increase muscular strength and hypertrophy. The patients performed three sets of 10 repetitions (intensity not reported) of knee extension and leg press exercises four times a week for one to four months. Isometric force increased 1–200%. No statistical pre to post training analysis was reported. All the patients performed three sets of each exercise; consequently, there was no comparison of single versus multiple sets.

Krusen7 randomly assigned military aged male (n = 23) and female (n = 1) patients with poliomyelitis to one of two knee extensor strength training protocols (sessions per week not reported). One group performed five repetitions with 25% 5RM, five repetitions with 50% 5RM, and five repetitions with 75% 5RM twice a day for 3–23 weeks. The other group performed five repetitions with 100% 5RM, five repetitions with 125% 5RM, and five repetitions with 150% 5RM, with limited range of motion on the last two sets twice a day for 3–19 weeks. Krusen claimed that, to rule out the difference in initial strength, 11 subjects in each group were matched for strength and compared after five weeks of training. Knee extension 1RM increased in both respective groups (74.5% and
60.0%), with no significant difference between groups in strength gains. A comparison of single versus multiple sets was not possible because both groups performed three sets of exercise. The actual comparison was between a low intensity protocol (five repetitions with 75% 5RM) and high intensity protocol (five repetitions with 100% 5RM), which resulted in similar strength gains.

McMorris and Elkins\(^8\) trained the right triceps in nine healthy men and three women (20–29 years of age) with four sets of exercise five times a week for 12 weeks. One group performed four sets of 10 repetitions with 25%, 50%, 75%, and 100% 10RM in ascending order, while the other group performed four sets of 10 repetitions with 100%, 75%, 50%, and 25% 10RM in descending order. McMorris and Elkins\(^8\) reported a 5.5% greater gain in strength for the group that performed the ascending protocol. There was no statistical analysis reported. Because all the patients performed four sets of each exercise, no comparison of single versus multiple sets was possible.

Hellebrandt and Houtz\(^9\) reported their observations of what they described as 620 experiments in 17 male and female competitive athletes. Wrist flexion and extension exercise varied from 3 to 40 sets of 25 repetitions with 25–100% 25RM three to five times a week for three to seven weeks. There was no statistical analysis reported, and more importantly, no comparison of single versus multiple sets.

McGovern and Luscombe\(^10\) reported on a two part investigation. In part 1, 10 healthy men (20–30 years of age) were divided into two groups (A and B). Group A performed one warm up set of unilateral quadriceps exercise for five repetitions using 50% 10RM, followed by one set of 10 repetitions with the 10RM. Group B performed one range of motion warm up set (no resistance), followed by three sets of 10 repetitions with 100%, 75%, and 50% 10RM, respectively, and one minute rest between sets. A third group (C) of five male orthopaedic patients (20–30 years of age) performed three sets of 10 repetitions with 50%, 75%, and 100% 10RM respectively. The groups trained five times a week for three weeks. No pre to post training or between group statistical analysis was reported. However, the authors stated that the increase in 10RM was similar for groups A, B, and C (7.7, 8.7, and 7.7 kg respectively) in part 1. In part 2, 28 male and female orthopaedic patients (20–30 years of age) trained five times a week for four weeks. Half the patients performed one set of 10RM and the other half performed three sets of 10 repetitions with 50%, 75%, and 100% 10RM. The two groups of patients showed similar gains in 10RM (5.8 and 6.0 kg). McGovern and Luscombe concluded that the two training protocols produced similar strength gains, with a time saving advantage to the single set group.

The study by McGovern and Luscombe\(^10\) was the only one of the five studies\(^6–10\) cited by Dr Berger\(^1\) that compared single versus multiple sets, and they reported similar strength gains as a result of one set or three sets of exercise.

Dr Berger claimed that the medical applications of strength training for physical therapy have historically involved multiple sets, and that one set was the exception (p 372).\(^4\) In 1951, Delorme and Watkins\(^11\) specifically stated that increasing the resistance after each of three sets provided an advantage for warming up the muscles in patients suffering from poliomyelitis, but probably did not contribute to the increase in muscular strength. They concluded that performing one set or three sets of 10RM would produce a similar increase in strength (pp 27–8).\(^11\) Contrary to Dr Berger’s claim that one set was an exception, and contrary to Dr Berger’s study\(^5\) where his nine groups performed
one, two, or three maximal sets, Delorme et al., Krusen, McMorris and Elkins, and McGovern and Luscombe actually trained all their subjects with only one maximal set.

Dr Berger claimed that he was not the genesis of an unsubstantiated belief (p 372). In fact, I noted in my retrospect that Dr Berger’s study—not he personally—was perhaps the genesis of the unsubstantiated belief that multiple sets are required for optimal strength gains (p 319). The flow chart (fig 1, p 320) in my retrospect clearly shows that Dr Berger’s study was the only strength training study cited.

Dr Berger stated that therapists and coaches are flinging down the gauntlet in support of multiple sets (p 372). Apparently, Dr Berger is claiming that therapists and coaches are challenging others to show that multiple sets are not superior. However, a primary tenet of science is that the burden of proof rests entirely on those making the claim. If Dr Berger and the therapists and coaches he refers to claim that multiple sets are superior, then the entire burden of proof is on them to support that claim with peer reviewed strength training studies.

Dr Berger claimed that the 0.05 level of probability does not always supersede common sense when attempting to control extraneous factors in strength research (p 372). In fact, it was Dr Berger who chose the 0.05 level of probability for his study. In my retrospect, I simply reported the level of probability that he used.

Dr Berger claimed that if he had concluded that one set was as good as multiple sets in 1962, practitioners in the field would have confronted him to express their disagreement (p 372). It is not my fault that the reviewers and the editor of Research Quarterly initially failed as gatekeepers and that in the last 40 years other scientists in the field have not challenged Dr Berger’s methodology, statistical analysis, and conflicting conclusions.

Dr Berger noted that Arthur Jones, who developed and sold Nautilus machines, was a proponent of single sets and that he has never presented any acceptable scientific evidence to support his belief (p 373). Mr Jones was never mentioned or cited in my retrospect because Mr. Jones’ beliefs, or anyone who agrees or disagrees with those beliefs, are completely irrelevant to Dr Berger’s study, my retrospect, and Dr Berger’s response to my retrospect.

Dr Berger claimed that the data in tables 1, 2, and 3 of my retrospect were inappropriately used according to acceptable statistical protocol (p 373). In fact, it was Dr Berger who used the statistical analyses that I reported in my retrospect. The data in table 1 (p 320) of my retrospect, which show the increase in 1RM bench press for each of Dr Berger’s nine groups, were gleaned directly from Dr Berger’s table 1 (p 172). The data in table 2 (p 320) of my retrospect, which show the groups that differed significantly at the 0.05 level of probability, were from Dr Berger’s table 4 (p 176). When I questioned Dr Berger about the three typographical errors in his table 4, he said that he was not aware of these errors and claimed that no one else had ever questioned them before our telephone conversation in 1998. The correct data are shown in table 2 of my retrospect (p 320). Table 3 (p 321) in the retrospect shows that for the groups who performed the same number of repetitions (two, six, or 10), seven out of nine of Dr Berger’s comparisons showed no significant difference in the magnitude of strength gain. For example, the strength gain in the 3x6 group was significantly greater than the 2x6 group, but not greater than the 1x6 group. Likewise, 3x2 was sig-
nificantly greater than 2×2, but not significantly greater than 1×2. The data are all from Dr Berger’s

table 4 (p 176).

Dr Berger claimed that comparisons between his nine groups were not valid for critiquing his study
(p 373). I did not make any comparisons between Dr Berger’s groups. However, I did accurately
note in my retrospect exactly what Dr Berger reported in his study. Dr Berger’s subsequent ad-
mission that he erroneously made comparisons among the nine groups in table 4 of his study (p
373) nullifies his previous claim.

Dr Berger claimed that when he compared the main effects among one, two, or three sets, and
among two, six, or 10 repetitions, three sets and six repetitions resulted in the greatest improvement
in strength (p 373). In his Results section Dr Berger stated that analysis of covariance was used to
test for a significant interaction between sets and repetitions—that is, whether one, two, or three sets
might be better in combination with two, six, or 10 repetitions. The F ratio was not significant at
any period of training (p 178). Dr Berger also reported in Conclusion 6 that training with one, two,
or three sets in discrete combination with two, six, or 10 repetitions per set (interaction) was not
more effective for improving strength than other combinations of sets and repetitions (p 181). However,
in Conclusion 7 Dr Berger claimed that a combination of 6RM performed for three sets
was more effective in improving strength than any other combination of sets and repetitions (p
181). These antithetical statements were reported in my retrospect. Dr Berger failed to address
them in his response or in our personal correspondence (1998).

Dr Berger noted that he spoke with me about my misuse of statistics and that he suggested that I
consult a statistician (p 373). I followed his advice and consulted two statisticians before submit-
ting my retrospect. Their opinion was that Dr Berger should not have made the 36 individual com-
parisons that he reported because the multiple comparisons increased the chance of a type I error,
which could have led to rejection of the null hypothesis when the null hypothesis was actually true.

Dr Berger stated that he still unequivocally supports multiple sets over a single set for optimising
strength (p 373). Dr Berger is perfectly entitled to his opinion, but it has nothing to do with sup-
porting his belief with scientific evidence. As noted in my retrospect, Dr Berger expressed his
opinion about the superiority of multiple sets in our personal correspondence (1998), and in at least
five publications. The only study he cited to substantiate his opinion was his 1962 study. Dr
Berger neglected to cite his own follow up studies that failed to support his opinion.

Dr Berger suggested that I conduct research in the hope of gaining support for my position, al-
though he does not define my position (p 373). In fact, my position is simply that if someone in the
scientific community makes a claim about anything, they should be held accountable to support that
claim with peer reviewed scientific evidence. As noted in the flow chart of my retrospect, most of the
physiologists who recommend multiple sets of each exercise cite each other’s opinion, reviews and books, cite nothing at all, or they cite Dr Berger’s study. Because I am not making a
claim about the superiority of one strength training protocol over another, I am under no obligation
to conduct research. I am simply challenging the widely held belief that multiple sets are superior to
a single set, and in turn, challenging the apparent genesis of that belief—Dr Berger’s study.
References


