Strength training for soccer players:
A reply to Hoff and Helgerud (2004)

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We read with interest the article *Endurance and Strength Training for Soccer Players: Physiological Considerations* by Hoff and Helgerud. [1] We agree that proper strength training has the potential to increase soccer performance, and that strength training should therefore be an integral part of all soccer players' training. Though we congratulate Hoff and Helgerud for raising this important issue, and for providing some excellent suggestions for endurance training for soccer players, we find that a number of their statements and recommendations in the strength training section of the article are substantially flawed. Therefore, we provide the following comments:

ONE: We take issue with the authors' definition of strength, "…the integrated result of several force-producing muscles performing maximally" (p. 172). The authors go on to note that "maximal" strength is typically defined in terms of a one-repetition maximum (1RM) in a standard exercise such as the squat. The problem with this definition is that, whereas it is possible to determine the amount of force produced when performing such a task, it is impossible to determine where the force came from. For example, the squat exercise substantially involves the muscles of the buttocks, the hamstring muscles and the quadriceps. If an athlete has very strong buttocks and hamstring muscles, but relatively weak quadriceps, they may still be relatively strong in the squat. However, the use of such a crude strength measure may not enable this weakness to be identified and rectified. Instead, it is important that the strength of individual muscle groups is assessed to enable a balanced strength training programme to be constructed for each individual athlete, with particular focus on relatively weak muscles that are important to soccer performance.

TWO: The authors note that much strength training research has been conducted using isokinetic and isometric strength measures, and claim that such techniques have limited use in predicting strength-related performance in dynamic sports. We also have concerns regarding the validity of isokinetic strength measures. For example, such measures have been criticised due to the lack of empirical support for the validity of claimed 'gravity compensation' mechanisms on isokinetic machines, [2,3] the inclusion of non-muscular sources of torque (stored energy torque) in isokinetic strength measures [3] and the lack (indeed, impossibility) of a constant speed of movement on such machines [4,5,6]. This last point is particularly important, as artifact results from acceleration and deceleration. [2,3,4,6,7] The amount of information lost due to this can be as much as 50% of the range of motion with isokinetic tests. [8] However, given the fact that Hoff and Helgerud criticise the use of isokinetics, it is ironic that most of the research supporting their 'velocity specificity' argument has used isokinetic measures. In the review Hoff and Helgerud cite to support this argument [9], seven isokinetic studies are cited to support the velocity specificity effect. However, in at least one study cited [10], which has been cited thousands of times to support this effect, [11] the data clearly contradict the authors' own conclusions. This study examined the effect of 'slow' (36 degrees per second) and 'fast' (108 degrees per second) knee extension training on knee extension torque at various speeds. These authors argued that their data supported the idea that training at a fast speed produced strength gains at all speeds, whereas training at a slow speed produced strength gains only at slow speeds. In fact, as Wolf [11] pointed out, the only statistically significant between-group differences were at lower speeds, where the 'slow' group showed almost double the strength gains of the 'fast' group. Therefore, the findings of this study provide no support for the 'velocity specificity' argument of Hoff and Helgerud.

Two other studies in this review [12,13] contradicted the specificity hypothesis. Thus, the findings of studies using such measures appear rather mixed. In addition, given the concerns with isokinetic measures noted above, we contend that the results of all such studies should be treated with great caution. Of even more importance, however, we note that recent studies using the more ‘functional’ measures advocated by Hoff and Helgerud do not support the view that ‘explosive’ training is more effective than traditional, slow weight training in enhancing any aspect of muscle function. However, none of these studies are mentioned in the Hoff and Helgerud review. For example, Liow and Hopkins [14] examined the effects of slow and explosive weight training on kayak sprint performance. The ‘explosive’ group performed the concentric part of each exercise in less than 0.85 s, whereas the slow group took 1.7 s. Both methods produced an increase in 15m sprint performance (3.4% for the slow training group and 2.3% for the explosive training group), with no significant between-group difference. In another study using slow and explosive training groups, Blazevich and Jenkins [15] examined varying movement velocities in hip flexion and extension, knee extension and flexion and the squat. Subjects trained for 7 weeks, lifting either 30-50% of or 70-90% 1 RM for the high and low velocity groups respectively. No significant differences were found in sprint performance, 1RM squat or hip extension and flexion torque between the groups even though the subjects from both groups elicited significant improvements in these variables.

Young and Bilby [16] compared the effect of slow versus explosive repetitions on performance of barbell squats. After 7.5 weeks of training three times per week, both methods significantly increased 1RM, as well as isometric peak force, vertical jump, thigh circumference and muscle thickness, with no significant between-group differences. Palmieri [17] split subjects into three groups based on repetition cadence (fast cadence, slow cadence and a combination of both) and examined the effects of a 10-week training programme, consisting of squats and machine exercises, in each group. The fast cadence group performed the concentric part of each repetition in 2s or more, the slow cadence group performed it in .75s or less, and the combination group spent the first 6 weeks performing fast cadence repetitions and the last 6 on slow cadence repetitions. Overall, all groups improved significantly and there were no significant between-group differences. Interestingly, however, when the combination group switched to the fast cadence condition they failed to produce any further increases in the dependent measures, 1RM squat and lower body power. The findings of these studies clearly do not support Hoff and Helgerud’s view that weight training movements need to be performed explosively to produce optimal increases in muscle power. These findings also do not support their view that slow weight training may actually reduce the ability to develop torque at high velocities. This claim is based on a descriptive study by Tesch and Larson, [18] who performed muscle biopsies and dynamic strength tests on three competitive bodybuilders and a reference group of competitive weightlifters. They noted that the bodybuilders were less able to develop torque at a high velocity than the weightlifters. However, the very small sample size in this study does not meet the most minimal requirements for the findings to be considered reliable, and thus they cannot be generalised to bodybuilders as a whole. Also, this was not a training study with randomly-allocated groups assigned to different training styles. Therefore, it is simply not possible to conclude that the slow training of the bodybuilders has made them slower. In fact, the speed of movement used in the bodybuilders’ training was not even measured during this study and compared to that of the weightlifters. The claim that this finding is due to velocity specificity simply cannot be supported by the study design: an intervention study would be necessary to examine this and, as noted previously, the results of such intervention studies do not tend to support the velocity specificity view.

Not only is ‘explosive’ weight training unnecessary for increasing muscle power, there is also a great deal of evidence that such training poses considerable injury risks. For example, Hall [19] found that fast lifting speeds greatly increased shear forces in the lumbar region. Kulund [20] noted that injuries to the wrist, elbow and shoulder were commonplace when individuals performed fast, Olympic-style lifting. There is also evidence that explosive lifting can lead to spondylolysis. [21,22] For example, Kotani et al. [21] found that 30.7% of a sample of weightlifters, all of whom performed explosive lifts, suffered from this problem. Given that one of the main benefits of strength training for athletes is a reduction in the likelihood of injury, [23] to advocate a training regime that may actually cause injury is questionable from both an ethical and a performance standpoint.

THREE: As well as advocating explosive movements, Hoff and Helgerud recommend a low number of repetitions per set (4-6), and 3-4 sets of each exercise, to increase strength and power without increasing bodyweight through hypertrophy. For example, in their 2002 study, [24] soccer players trained three times per week for eight weeks, performing four sets of five repetitions per exercise. They found increases in 1RM half-squat, rate of force development and sprinting performance. In this, and another study using a similar protocol, [25] no comparisons were made with a slow, moderate-repetition weight training group, and therefore no real conclusion can be drawn from these studies as to the relative efficacy of such methods. However, as noted previously, when such explosive protocols have been compared with slow weight training in other studies, no advantage has been found from such training.

Also, the great preponderance of scientific evidence does not suggest any advantage from performing multiple sets of an exercise, contrary to the advice of Hoff and Helgerud. For example, in a review of 35 studies, Carpinelli and Otto [26] found no significant difference in strength gains from single versus multiple sets in 33 of those studies. In a critique of American College of Sports Medicine guidelines, [27] which advocate the performance of multiple sets, Carpinelli et al. [28] noted that the overwhelming scientific evidence showed that single sets were as effective as multiple sets for enhancing any aspect of muscle function. This held true both for novices and experienced weight trainers. They cited 47 studies that showed single sets to be at least as effective as multiple sets, and only four studies showing that multiple sets were superior. Two of the latter four studies were also loaded with confounding variables that call the validity of their findings into question [29, 30] Therefore, the great preponderance of research suggests that Hoff and Helgerud are advocating several times as many sets as are necessary to produce optimal results.

The suggestion made by Hoff and Helgerud that a low number of repetitions should be performed to maximise increases in strength and the rate of force development, is presented with five 'supporting' references (p. 175), none of which are studies that compare the effects of different repetition ranges. This suggestion appears to be pure speculation. We have been unable to find any studies that have specifically examined the effects of different repetition ranges on the rate of force development, but a number of studies show that performing a low number of repetitions does not produce greater strength increases than performing a higher number of repetitions. For example, Pruitt et al. [31] examined the effects of two training programmes on muscle strength in geriatric females. Subjects trained with either 7 repetitions at 80% 1RM or 14 repetitions at 40% 1RM on various exercises three times per week for a year. Both groups improved significantly on all seven (1RM) strength measures, with the only significant difference being a greater increase in arm strength in the 14RM group. Chestnut and Docherty [32] examined the effects of 10 weeks of training on elbow flexor and extensor strength, as well as arm circumference and cross-sectional area. Strength and muscle size increased significantly in a 4RM group and a 10RM group, with no significant between-group differences. Graves et al. [33] examined the effects of performing knee extensions twice per week for 10 weeks, in two groups of identical twins. There was no significant difference between the strength increases achieved by a 7-10 RM group and a 15-20 RM group. Several other studies [34,35,36,37] have shown similar results. Therefore, despite the claims made to the contrary by Hoff and Helgerud, the scientific evidence suggests the performance of a moderate (6-20) number of repetitions is equally as effective as a low number (<6) for increasing strength. As the use of a very low number of repetitions will expose the body to greater forces, potentially increasing the risk of injury, we suggest that a moderate number of repetitions should be advocated rather than the low repetition protocols advocated by Hoff and Helgerud.

FOUR: Hoff and Helgerud suggest that muscular hypertrophy is undesirable for soccer players as they will have to transport a greater body mass. Based on the Tesch and Larson [18] study mentioned previously, they argue that increased muscle mass does not necessarily increase high velocity strength. However, as noted previously this study was not a training study and did not examine the effects of a bodybuilding training programme on strength, and therefore such a conclusion is simply not warranted. We also question the relevance of this study to soccer players, most of whom are very unlikely to have the requisite genetic potential to produce hypertrophy comparable to that of the advanced bodybuilders in the Tesch and Larson study. Many soccer players appear thin and light, and may well benefit from the increased strength and decreased injury potential that greater muscle mass will bring, contrary to what Hoff and Helgerud suggest. Indeed, given that the strength of a muscle is proportional to its cross-sectional area, [38] we suggest that some hypertrophy would be advantageous rather than disadvantageous to soccer players. It is interesting that, in contrast to soccer players, sprinters and rugby players are often large and muscular, displaying obviously hypertrophied muscle, and yet they are able to run extremely fast. We, therefore, concur with Darden, [39] who states, "Well-developed, strong, lean body parts will help any athlete or non-athlete perform better" (p. 45).

Hoff and Helgerud claim that soccer players should aim to increase strength without increasing muscle size, and note that studies have shown increases in the 1RM squat with little change in bodyweight and little hypertrophy. [24,40] However, it is important to note that the 1RM squat is not purely a measure of muscular strength. It is also a test of the skill required to perform a 1RM squat, with neural adaptations occurring to enable the subject to co-ordinate the relevant muscles to perform the task most efficaciously. Therefore, improvements can occur in the ability to perform this task without actual increases in muscle strength occurring. However, given that skills are highly specific and that correlations between them are universally low, [41] an increase in the ability to squat will not necessarily produce a better soccer player: it will only produce a better squatter, unless of course this improvement is due to increases in actual muscle strength rather than squatting skill. We suggest that, if a weight training programme produces increases in weights used with no hypertrophy, then that programme is not a very effective one, and has produced increases in weightlifting skill rather than useable muscle strength. This probably explains why Thorstensson et al. [42] found a 70% improvement in 1RM squat, but only a 20% increase in static strength: that is, most of the increase in the 1RM squat was due to an improvement in squatting skill rather than an increase in strength. Thus, the claim of Hoff and Helgerud (p. 174) that explosive movements with low repetitions may produce "neuromuscular adaptation with minimal hypertrophy" is probably true, but there is no evidence that the neuromuscular adaptation produced will generalise to the high-force producing situations on the soccer pitch, and thus produce a better soccer player than would a traditional, slow weight training programme with moderate repetitions.

Of course, there may be a point at which, with the performance of a proper strength training programme, a player hypertrophies to an extent that any greater increase in body mass would be considered undesirable. However, the player would simply need to refrain from increasing the training loads used to prevent any further hypertrophy.

Conclusion

Contrary to the claims of Hoff and Helgerud, the great preponderance of scientific evidence does not support the view, often espoused by exercise physiologists, that fast weight training movements with low repetitions, performed for multiple sets, are necessary to produce optimal increases in muscle strength or power. Contrary to their claim that slow weight training is ineffective in increasing muscle power, and that it may even be detrimental, the research literature shows that such training is at least as effective as the methods espoused by Hoff and Helgerud. Contrary to their claim that training methods that increase muscle mass could be detrimental to soccer performance, we suggest that not only is this claim without foundation, but that strength training methods that fail to produce hypertrophy are unlikely to be optimally effective. We are disappointed that Hoff and Helgerud make such a selective use of the literature in attempting to substantiate their claims, and that they advocate training programmes that are more time-consuming and potentially injurious than necessary. We strongly advise that those interested in increasing their muscle strength and power, to aid their soccer performance, perform a thorough examination of the research literature, and base their training protocols on sound scientific evidence rather than relying on the advice offered by Hoff and Helgerud. It is unfortunate that this paper has reinforced several prominent myths and superstitions in the field of strength training, none of which are actually supported by the scientific evidence but appear to be accepted without question by many exercise physiologists.

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