Athlete’s Heart

To the Editor:

I read with great interest the meta-analysis on athlete’s heart by Pluim et al.1 In the introduction, they quote previous investigations, including the results of a meta-analysis from my group,2,3 and state that “...they have not been able to resolve satisfactorily the question regarding the existence of 2 types of athlete’s heart.” I strongly disagree with this interpretation of our meta-analysis, in which my colleagues and I compared the echocardiographic data from young male runners, strength athletes, and cyclists with their respective matched controls (total, 725 subjects). We firmly concluded that “the development of so-called eccentric or concentric left ventricular hypertrophy according to the type of sports cannot be regarded as an absolute or dichotomous concept because training regimens and sports activities are not exclusively dynamic or static and because the load on the heart is not purely of the volume or the pressure type.”3 We also asserted that “variable static and dynamic components of the training regimens and sports activities lead to different relative adaptations, which are either predominantly eccentric (runners), predominantly concentric (strength athletes), or a balanced combination of the two (cyclists).”2 The conclusions by Pluim et al1 are remarkably similar, as illustrated by their statement that “...the classification as an endurance-trained heart or a strength-trained heart is not an absolute and dichotomous concept but rather a relative concept [italics added].” Furthermore, the design of the meta-analysis by Pluim et al1 was less rigorous than the approach used in the meta-analysis done by my group.4 To avoid the impact of divergent methodological approaches to echocardiography by different investigators and to eliminate the influence of age and body size on the findings, we restricted the meta-analysis to studies in which the control subjects were adequately matched to the athletes and used a meta-analytical technique in which athletes were always compared with their own within-study control group. By contrast, Pluim et al1 pooled light and heavy controls in a single control group and did not adjust for differences in body size, claiming that relative wall thickness is not dependent on body size. However, in a previous study of healthy young men,5 my group did find positive relationships between relative wall thickness and weight (r = 0.18; P < 0.05) and body mass index (r = 0.22; P = 0.01). In conclusion, it is comforting that the different meta-analytical approaches led to similar conclusions, strengthening the interpretation of athlete’s heart, but it is regrettable that Pluim et al1 neglected to state that they merely confirmed the previous interpretation by my group.2,3

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Response

We thank Dr Fagard for his comments on our article.1 The previous articles by Dr Fagard and his coworkers on athlete’s heart have been a great source of inspiration for our group. Their article “Athlete’s Heart: A Meta-analysis of the Echocardiographic Experience” clarifies many of the problems regarding athlete’s heart, and it inspired us to approach the issue from a slightly different angle.2 We regret that Dr Fagard disagrees with our interpretation of his meta-analysis, but we understand his response. It would probably have been better if we had formulated our conclusion differently and referred to his above-mentioned article. Our article does confirm his previous conclusions.3

The second remark by Dr Fagard regarding whether it is necessary to match for body size is a problem that is inherent to the athlete’s heart and is virtually impossible to solve. Weight lifters and shot-putters are simply bigger and heavier than long-distance runners. However, we wanted to compare the hearts of these 2 types of athletes without losing an important part of the population. We did discuss the issue of whether all studies should contain matched controls in great detail under the heading “Potential Limitations.”4

We were not aware of the possibility of a positive relationship between relative wall thickness and weight. Therefore, we carefully scrutinized the article “The Inheritance of Left Ventricular Structure and Function Assessed by Imaging and Doppler Echocardiography” that Dr Fagard mentions.5 In this article, the heritability of left ventricular structure and function is addressed in monozygotic and dizygotic twins. Unfortunately, the positive relationship between relative wall thickness and body mass index Dr Fagard is referring to is not reported in this article.3

In his letter, Dr Fagard assumes a positive relationship between relative wall thickness and weight (r = 0.18; P < 0.05) and between relative wall thickness and body mass index (r = 0.22; P < 0.01). These data indicate that variance in relative wall thickness is explained by weight for only 3% and by body mass index for only 5% of the study population. On the basis of these figures, the dependence of relative wall thickness on weight and body mass index is of no practical importance. Therefore, we doubt that there is a strong positive relationship between relative wall thickness and weight or body mass index.

Finally, we would like to stress the main goal of our article. Our aim was to examine the athlete’s heart in general. Our main question was related to 3 groups of athletes: how does the average heart of a strength-trained athlete, an endurance-trained athlete, and an athlete with both static and dynamic exercise in his training program behave? In our Discussion, we did indicate that some of the differences between the hearts of the 3 types of athletes might originate from differences in body size. However, our goal was not to determine the relative effect of training, genetics, doping, or other confounding factors. If that had been our goal, we would indeed have chosen a different design and, in that case, matching for body size would have been an essential part of the study.

Our goal was to describe and clarify the essential characteristics of the athlete’s heart, and we think that we reached that goal.
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