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Letters

To the Editor:

Kenanidis E, Potoupnis ME, Papavasiliou KA, et al. Adolescent idiopathic scoliosis and exercising: is there truly a liaison? Spine 2008;33:2160–5.

We read with great interest the article by Kenanidis *et al*[1] that shows, despite certain limitations, that sport has no correlation with scoliosis. However, the article said nothing in regard to exercising and scoliosis, as was misleadingly stated in the title and abstract.

The terms exercise and sport are clearly distinguished from one another by the medical community. In the context of MeSH the terms sport, exercise, and exercise therapy have different definitions, and sport is not even in the same section of the MeSH tree. In the spine medical community, this is very well known when back pain is concerned: the terms stay active, physical activity, and exercise are clearly distinguished from one another and are therefore, studied independently.

Regarding adolescent idiopathic scoliosis (AIS), sport has been studied with contrasting results, while 1 RCT,[2] some prospective controlled studies, and 3 systematic reviews have confirmed the usefulness of exercise therapy in controlling the evolution of AIS, reducing the occurrence of brace prescription, and increasing brace efficacy.[3] The actual strength of evidence on the usefulness of exercise therapy for the treatment of AIS is 2a.[3,4]

We can certainly understand a bit of linguistic confusion in everyday life, but this is not acceptable in the technical domain. When scoliosis is concerned, it seems the orthopaedic surgeons community has difficulties in considering an exercise therapy, even in the face of the cited evidence.[3] Thus exercising can be an improper generalization by the authors or the reviewers, but the leading journal of the spine medical community (*Spine*) should pay greater attention to the terminology used–mainly in titles and abstracts–so as not to generate incorrect messages within (and outside) our community.

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In Response:

We would like to thank Dr. Negrini for his interest in our recent article[1] and we would like to make the following comments in response.

We regret that Dr. Negrini has completely misunderstood our article. If he had read it more carefully, he would have realized that it is impossible to evaluate the influence of any factor on the natural course of Adolescent Idiopathic Scoliosis (AIS), based only on an observational, cross-sectional study. Our article never claimed that exercise therapy can influence (positively or negatively) the natural evolution of AIS, as this was neither its aim nor our intention; furthermore, the study's design was not suitable for such an endeavor.

As far as the use of the potentially "misleading" term exercising is concerned, we would like to make the following comment. Exercise is defined as "the performance of physical exertion for improvement of health or the correction of physical deformity."[2] According to the MeSH database, exercise is ". . . physical activity which is usually regular and done with the intention of improving or maintaining physical fitness or health."[3] Based on the prementioned definitions, we strongly believe that our "athletes" did exercise (*i.e.*, they were exercising), hence the use of the term exercising was both correct and justified.

Concerning the accusations against *Spine*, we are the last ones that need to stand in support of your distinguished journal. We strongly believe however, that *Spine* would have never succeeded in becoming one of the world's leading journals, if it had not implemented excellence in every stage of its publication procedures.

We respect a scientist's passion when defending his/her point of view. We understand the different interpretation of facts, studies, and well established scientific knowledge. However, we cannot understand the wrong and unfair accusations against orthopedic surgeons, especially when considering the fact that orthopedic surgeons will have to deal with all the patients with AIS that did not respond well to exercise therapy.

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To the Editor:

Re: Karamanis EM, Matthaiou DK, Moraitis LI, et al. Fluoroquinolones versus β-lactam based regimens for the treatment of osteomyelitis: a meta-analysis of randomized controlled trials. Spine 2008;33:E297–E304.

Karamanis *et al*[1] presented meta-analysis of randomized control trials to compare fluoroquinolones to β-lactams for the treatment of osteomyelitis based on publications in 1970 to 2001. We agree with authors that fluoroquinolones are useful alternative to β-lactams, but such therapy may be restricted due to increasing resistance to methicillin and fluoroquinolones in *Staphylococcus aureus*, the main pathogen causes osteomyelitis. According The European Antimicrobial Resistance Surveillance System (Availabe at: http://www.rivm.nl/earss/database/) in 2006, almost half countries (15 for 31) reported MRSA proportions higher than 25%, whereas 6 countries revealed proportions of over 40% (Malta, Romania, Portugal, Greece, Ireland, and United Kingdom). Moreover, methicillin-resistant *Staphylococcus aureus* (MRSA) show also high level of resistance to ciprofloxacin. MRSA isolates from Belgian hospitals revealed 98% nonsusceptibility to ciprofloxacin, eliminating this therapy.[2] We examined in 2000 to 2005 the group of 263 patients with chronic osteomyelitis. We isolated 204 (77.6%) strains of *S. aureus*, 41 (15.6%) coagulase negative staphylococci (CNS), 59 (22.4%) Gram-negative rods, and 7 (2.7%) *Streptococcus* spp. Among 41 strains of CNS, 28 (68%) were methicillin resistant and 11 (39%) showed nonsusceptibility to ciprofloxacin, whereas only 2 of 13 (14%) methicillin sensitive CNS isolates were resistant to ciprofloxacin.[3] Of 204 *S. aureus* strains, 43 (21.1%) were MRSA with prevalence rate 37% (15 strains) of nonsusceptibility to ciprofloxacin, whereas all methicillin susceptibile *S. aureus* revealed susceptibility to ciprofloxacin. Antimicrobial susceptibility patterns of *S. aureus* in Poland showed very similar proportion of MRSA isolations from surgical site (21%), joint and bone infections (21%), but nonsusceptibility to ciprofloxacin is higher (62% isolates).[4] We support authors' thesis that β-lactams and fluoroquinolones should be used with caution and according to the antimicrobial resistance patterns, however, the further studies are required for assessment new antibiotics in effective therapy of osteomyelitis.

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In Response:

We thank Drs. Kłuciński, Ekiel, Wilk, and Martirosian for their interest in our study as well as for adding to the literature data on the implicated pathogens and antimicrobial resistance of their case series with osteomyelitis. The reported prevalence of methicillin-resistance in *Staphylococcus aureus* (MRSA) clinical isolates varies in different parts of the world, from less than 1% in Northern Europe[1] to 77% in Taiwan,[2] whereas *S. aureus* accounts for over 50% of osteomyelitis cases.[3]

Fluoroquinolones have been extensively used for the treatment of various infections in many settings. For this reason, our group has studied their role in the treatment of patients with infections of various sys-

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tems/organs.[4–5] Although fluoroquinolones have some excellent pharmacokinetic and pharmacodynamic characteristics, there are also special concerns related to their use, including their potential for arrhythmia and pressure for emergence of advanced antimicrobial resistance.[6–8]

The treatment of MRSA infections depends on various factors, including the pathogen's susceptibility pattern and whether the infection is community acquired or healthcare associated. Antimicrobial agents such as vancomycin, cotrimoxazole, and clindamycin may be considered in the treatment of MRSA osteomyelitis.[9] In addition, newer agents, including daptomycin, ceftobiprole, and linezolid, may be useful alternatives in the light of increasing resistance of MRSA to traditionally used antibiotics.[10–11] Thus, we agree with the authors on the need of further studies that will assess new antibiotics in terms of effectiveness and safety for the treatment of patients with osteomyelitis.

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