Reply to the Letter to the Editor of Zehao Jing et al. concerning “Scoliosis convexity and organ anatomy are related” by T. P. C. Schlösser et al. (Eur Spine J; 2017: doi:10.1007/s00586-017-4970-5)

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Received: 22 July 2017 / Accepted: 29 August 2017 / Published online: 5 August 2017
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We thank you for your interest and critical review of our manuscript [1]. We understand your concerns regarding the potential pathogenetic link between primary ciliary dyskinesia (PCD) and scoliosis.

We agree, on the one hand, that recent data on the ependymal cell cilia development and the onset of scoliosis by Oliazadeh et al. could support the hypothesis that scoliosis in PCD patients may not be of the ‘idiopathic’-type [2]. On the other hand, we do not fear that our conclusions are questionable based on the prevalence of scoliosis in our PCD population. Despite the frequent citation of the overall prevalence of 1.5–3% for idiopathic scoliosis, Konieczny et al. have demonstrated that the prevalence of idiopathic scoliosis as reported in epidemiological studies varies widely and is dependent of age, gender, race and screening methodology: They found prevalences reported between 0.47 and 11.1% [3]. Even, in epidemiological studies that are most comparable to our design (screening on chest X-rays), the prevalence of thoracic scoliosis is 8–24% [4–6]. Therefore, we would like to remind the readers that the prevalence of scoliosis in our population of PCD patients (8%) is within the range reported for the prevalence of scoliosis in the general population.

Based on our data, we cannot support or withdraw any hypothesis on the etiopathogenesis of development of scoliosis in patients with primary ciliary pathology, other than curve convexity. Going forward, it is our hope that

research on ciliary dysfunction will spark our understanding of the development of scoliosis of the idiopathic type.

Compliance with ethical standards

Conflict of interest T.S. received a grant by AOSpine; R.C. receives research support by K2M Group Holdings, Inc. and a grant by AOSpine.

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