most orthopaedists: *Rheumatology (Oxford)*. Our article—"Arthroscopy in Rheumatology. Where Have We Been? Where Might We Go?"[1]—traces the history of rheumatologists' interest and involvement in arthroscopy with an eye to what we may be using it for in the future. We think the article is worth a read for orthopaedists to help understand that rheumatologic and orthopaedic applications of arthroscopy are really quite different, making cooperation feasible and turf battles avoidable. A few rheumatologists were interested in arthroscopy since its inception, with most conditions examined then being chronic synovial processes. Frankfurt's Ernst Vaubel, a rheumatologist, wrote the first book on arthroscopy. Rheumatologists were part of Watanabe's diaspora, sometimes teaching orthopaedists about the procedure. Orthopaedists Bob Jackson, Lanny Johnson, and Dave Stulberg helped several rheumatologists get started with arthroscopy. With the developments of video arthroscopy, motorized shavers, and other special tools designed for arthroscopy, the procedure attracted a wider pool of both rheumatologists and orthopaedists. In his 1987 presidential address to the American Rheumatism Association, Bill Kelley stated that "we need to expand the specialty of rheumatology to cover some of the peripheral areas which now are largely ignored and sometimes poorly handled. This would include . . . the use of certain technical procedures which are appropriate to our specialty." It was about that time we both got going. Our training had included resective procedures applicable to patients with arthritis, and these became an important part of our practice. Enthusiasm for our activities was not widespread. Emergence of smaller scopes applicable in an office setting fueled rheumatologists' interest in the technique. However, results from prospective controlled trials showing limited utility of arthroscopy in knee osteoarthritis and the emergence of far more effective drugs for synovial disorders, greatly reducing the demand for synovectomy, both sharply reduced our clinical activity.

Extensive literature has developed describing arthroscopy detecting basic molecular aspects of synovial disorders and judging responses to treatments. Whereas ultrasound-guided synovial biopsy has become an increasingly popular method for obtaining synovium for research, arthroscopic assessment and biopsy continue to be considered the gold standard for this purpose. A new generation of small-bore arthroscopes promise to take arthroscopy even further from the operating room. Judging treatment responses by changes in target tissues is a long-term goal as we approach true "precision medicine" with treatments tailored to specific individual characteristics.[2] It remains generally accepted that arthroscopy is still preferred for assessing and obtaining the synovium that would guide such therapy[3] because variability in synovial characteristics cannot be accounted for by externally guided sampling.[4] Means to assess synovial tissue are about to take another quantum leap.[5] The arthroscope will have an important role in these efforts.

It is these issues we address in our article. We invite the readers of *Arthroscopy* to take a look.

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Note: The authors report no conflicts of interest in the authorship and publication of this letter. Full ICMJE author disclosure forms are available for this letter online, as supplementary material.

https://doi.org/10.1016/j.arthro.2021.02.024

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Regarding "*Editorial Commentary*: Artificial Intelligence in Sports Medicine Diagnosis Needs to Improve"

We read the Editorial Commentary, "Artificial Intelligence in Sports Medicine Diagnosis Needs to Improve," by Dr. Nikolaos Paschos with great interest.[1] This commentary was written with respect to the systematic review performed by Kunze et al.[2]

In the review, 11 studies that used artificial intelligence (AI)—based techniques for the detection of anterior cruciate ligament and meniscus pathology from magnetic

resonance imaging (MRI) scans were evaluated.[2] The systematic review reported that across 11 studies, AI-based techniques were able to identify anterior cruciate ligament (ACL) and meniscus pathology with impressive efficacy. Among the 5 studies comparing the computer to a clinical expert head-to-head, the human outperformed the model in 2 of the studies. As such, the concluding message was "AI did not outperform clinical experts." The editorial amplified this message by distilling the value of AI to whether AI was "ready to take over a part of the diagnostic process" and cautioned that "we need to resist our enthusiasm for these novel tools until we have robust, long-term, and replicated data." Although few will argue the importance of scientific rigor and remaining critical of new process adoption, it is important to understand what AI is and what it is not.

AI is not the rendering science fiction has portrayed wherein humans battle machines for dominance. AI is simply an analytic technique theorized over 50 years ago postulating that computers could recognize patterns to automate human tasks. AI is only now popular because the 2 missing ingredients have become commercially available: computing power and large datasets (i.e., "Big Data"). Of those 2 ingredients, computer processing power is a well-indoctrinated staple of society as a reliable tool. Just like clinical experience to a surgeon or specimens to a translational experiment, the quality and quantity of inputted data are the critical inflection point and therefore the key ingredients in determining the conclusion of any experiment, Netflix movie suggestion, or other AI-based prediction. Although nuanced differences among AI architectures exist, the quality and quantity of inputted data reflects the performance and accuracy of any AI-based model.

Thus the studies in the systematic review did not actually evaluate AI itself; instead, they evaluated the quality and quantity of MRI images inputted into various AI-based models across 11 heterogeneous studies with variable ground truths and inclusion criteria. In the same spirit that transitioning from horse-drawn carriage to automobile did not require a randomized controlled trial to demonstrate superiority, the potential of AI need not be defended. Instead, our process acquisition of inputted data requires continued scrutiny to appreciate its strengths and limitations. Second, if studies truly sought to compare experts to the computer on a level playing field, it would be difficult to make the case that an AI-based model trained on a mere 1,370 ACL MRI screenshot images was outperformed when it performed similarly to 3 musculoskeletal radiologists with a combined expertise exceeding 36 years.[3] Third and most importantly, no one should be advocating, suggesting, or conceiving AI replace or compete with the role of the physician. Instead, AI exists as an adjunct, whether automating the documentation burden or expediting clinic triage in settings with limited expert access to immediately interpret ACL tears on MRI. The great volume of repetitive administrative burden thrust on physicians could be lessened with such automation AI offers, allowing more face-to-face patient care and paradoxically "making healthcare human again."[4]

If we are able to acquire training data with high fidelity and identify areas of meaningful use for task automation, we may be able to harness the potential of AI by mitigating the current demands and constraints of our dynamic healthcare environment. In so doing, we stand on the precipice of rendering leaner, higher-value care with newfound economies of scale that liberate both the orthopaedic surgeon on an individual level and the healthcare sector on a macro level.[5-11]

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Note: The authors report no conflicts of interest in the authorship and publication of this letter. Full ICMJE author disclosure forms are available for this article online, as supplementary material.

https://doi.org/10.1016/j.arthro.2021.03.011

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Response to "Regarding *'Editorial Commentary*: Artificial Intelligence in Sports Medicine Diagnosis Needs to Improve'"

The recent letter to the editor written by Ramkumar et al.[1] concerned several points raised by Dr. Nikolaos Paschos in a recent editorial commentary[2] on our systematic review entitled "Diagnostic Performance of Artificial Intelligence for Detection of Anterior Cruciate Ligament and Meniscus Tears: A Systematic Review."[3] In particular, Ramkumar et al.[1] sought to clarify the distinction between the appropriate use and current interpretation of artificial intelligence (AI) on the basis of several findings in our systematic review; or, in other words, between what AI is and what it is not. Although we are grateful for their well-intended defense of AI given their expertise in this growing area of research and concur with the majority of their primary disputes, we believe that the purpose and conclusion of our systematic review was misconstrued to align with their intended message. We also believe that the editorial commentary written by Dr. Paschos contained several statements that were imprecise as it pertains to the applications of AI.

The primary concern raised by Ramkumar et al.[1] was that our concluding message stated that AI did not outperform clinical experts and that Dr. Paschos was overzealous in his subsequent commentary. Specifically, the authors directed attention to Dr. Paschos' discussion of whether AI is "ready to take over *a part* of the diagnostic process" and subsequent cautioning that enthusiasm for AI models should be resisted until more data are acquired.[2] Their third point states "most importantly, no one should be advocating, suggesting, or conceiving AI replace or compete with the role of the physician." Our study did not insinuate that this is a possibility nor suggest that this represents the function that AI is intended to assume. Furthermore, Dr. Paschos did not claim this either, as his statement was that AI is currently not ready to take over *a part* of the diagnostic process. Suggesting that AI compete with or completely replace the role of a physician and expert is unfounded; this is a notion with which we entirely agree.

The authors discuss why our study was inadequate to (1) evaluate the ability of AI to diagnose anterior cruciate ligament (ACL) and meniscus pathology and (2) compare experts to AI performance. This is an inherent limitation based on the principles of any systematic review. We did not in fact evaluate the ability of AI to diagnose ACL and meniscus pathology, nor did we compare experts to AI performance; rather, we qualitatively described the existing literature pertaining to the topic. In opposition to the statement that we evaluated the quality and quantity of magnetic resonance imaging scans inputted into various AI-based models (which we did not), we simply evaluated and presented the quality and quantity of studies that examined AI-based task performance for meniscus and ACL pathology diagnosis. We did not infer that AI can or cannot be used as a diagnostic adjunct but rather presented the existing data in an unbiased manner. As with all systematic reviews, the level of evidence and strength of recommendations are only as strong as the available literature permits them to be.[4]

Dr. Paschos states, "These problems highlight the need for complete transparency and independent AI research with a high level of evidence prior to use in clinical practice."[3] Although AI clearly holds much potential through the ability to integrate patient-specific data into prediction models, perform task automation, and mitigate administrative and physician burden,[5] the literature on this topic in sports medicine remains limited at present. Presenting the heterogeneity of the current literature pertaining to AI should be viewed as an opportunity to identify areas to improve rather than a need to defend AI.[4] High quality inputs and data sources are essential, as the application of AI not only necessitates large amounts of data, but unbiased and high-quality data, and this should not be a point of contention. A systematic review is not capable of accounting for this limitation if its goal is merely to provide a subjective synthesis of the literature.[4]

However, there is much merit to the letter written by Ramkumar et al.,[1] and we support the authors' argument that some statements presented in the editorial commentary by Dr. Paschos risk being misinterpreted