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Kwee, Robert M.; Adams, Hugo J. A.; Kwee, Thomas C.

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Letters to the Editor

Chest CT in Patients with COVID-19: Toward a Better Appreciation of Study Results and Clinical Applicability

From

Robert M. Kwee, MD, PhD,* Hugo J. A. Adams, MD, PhD, and Thomas C. Kwee, MD, PhD[†]Department of Radiology, Zuyderland Medical Center, Heerlen/Sittard/Geleen, Henri Dunantstraat 5, 6419 PC Heerlen, the Netherlands*
e-mail: rmkwee@gmail.comDepartment of Radiology, Nuclear Medicine and Molecular Imaging, University Medical Center Groningen, University of Groningen, Groningen, the Netherlands[†]**Editor:**

We read with great interest the article by Dr Herpe and colleagues (1) published online in September in *Radiology*. We commend the authors for compiling such a large study with data from multiple centers in France during the peak of the national coronavirus disease 2019 (COVID-19) pandemic. The authors reported high diagnostic performance values of chest CT for diagnosing COVID-19, with sensitivity, specificity, negative predictive value, and positive predictive value of 90% (95% CI: 89, 91), 91% (95% CI: 91, 92), 89% (95% CI: 87, 90), and 92% (95% CI: 91, 93), respectively. These diagnostic performance values are in the high range compared with data reported in other studies from other countries (2,3).

The data in the study by Dr Herpe and colleagues (1) were accumulated during the first peak of the pandemic in their country, and as a result it was understandably difficult to perform a well-designed study without any flaws. Nevertheless, there are some rather crucial issues that may need to be clarified to better appreciate their results, and that may also be helpful for the design and reporting of future studies on this topic.

First, it is unclear whether Dr Herpe and colleagues (1) included a consecutive or random sample of patients. It appears that nearly 40% of patients were excluded because reverse-transcription polymerase chain reaction (RT-PCR) test results were lacking or because of excessive or unknown delay between RT-PCR and chest CT. These excluded patients may systematically differ from those who remained for inclusion and may influence the diagnostic performance. Furthermore, it is unclear whether the study also included patients with chronic pulmonary diseases. In these patients, diagnostic performance of chest CT may be lower when rather nonspecific diagnostic chest CT criteria are used (1) (ie, any single finding of bilateral ground-glass opacities with peripheral distribution, bilateral “crazy-paving” appearance with intralobular thickening, reverse halo sign, or any other sign compatible with organizing pneumonia). Second, it would have been valuable to report the duration and severity of symptoms of the included patients because it is known that both of these variables correlate with the

presence and severity of lung abnormalities (4,5). In patients with longer-lasting disease and critical illness, it is likely that chest CT performs better in diagnosing COVID-19. Third, it appears that chest CT formed part of the reference standard, which may have led to overestimation of diagnostic performance. Aforementioned methodologic issues may potentially hamper the interpretation of the efficacy of chest CT in diagnosing COVID-19. In addition, we would like to note that chest CT may not only help in diagnosing COVID-19, but it also has an important role in revealing a potential alternative diagnosis that may explain the patient’s clinical features (6). Data regarding the alternative diagnoses that may have been established in the 2260 patients who were deemed not to have COVID-19 in the study by Dr Herpe and colleagues would have been helpful to better appreciate their study results and clinical applicability.

Disclosures of Conflicts of Interest: R.M.K. disclosed no relevant relationships. H.J.A.A. disclosed no relevant relationships. T.C.K. disclosed no relevant relationships.

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Response

From

Guillaume Herpe, MD,*[†] and Jean-Pierre Tasu, MD, PhD*[‡]

Department of Radiology, University Hospital of Poitiers, 2 rue de la Milétrie, 86021 Poitiers, France*

e-mail: herpeguillaume@gmail.comDactim Mis, LMA, UMR CNRS 7348, Poitiers, France[†]LaTIM, UMR 1101, INSERM, Bretagne University, Brest, France[‡]

We would like to thank Dr Kwee and colleagues for their interest in our work (1). We read their remarks with interest and we would like to respond to each point.

The first point concerns how patients were included; whatever their medical history, consecutive patients consulting at the participating centers who were suspected of having COVID-19 pneumonia were included. It is true

that 40% were secondarily excluded and that the high rate of secondary exclusion could have introduced bias. In a self-assessed survey constructed to reflect clinical practice, our study design was obviously not able to control all bias.

The second point concerns patients with chronic pulmonary diseases. We included these patients to have the most accurate representation of the population suspected of having COVID-19 pneumonia (2). Even though we did not find any scientific data to support potentially lower accuracy of CT in this population, the remark by Dr Kwee and colleagues is interesting and calls for further studies.

The point concerning duration and severity of symptoms of the included patients is also interesting. However, we did not collect these data at the time of our survey, and it is therefore difficult to answer on this point without a further study.

The fourth point concerns the alternative diagnosis. Because it was not a study end point, we did not record these data, but we would like to underline that there already exist substantial data on this point in the medical literature (3,4).

Last, as pinpointed by Dr Kwee and colleagues, chest CT formed part of the reference standard (5). However, we would like to remind the reader that for the COVID-19 pneumonia diagnosis, multiparametric criteria were used, including level of disease exposures and prevalence, clinical symptoms and

evolution, biologic parameters, and repeated RT-PCR tests. It is difficult to estimate the potential bias, but it must be underlined that weight of chest CT was pondered by the other diagnosis criteria.

To conclude, our study has obvious limits, mainly due to the survey design. However, the results demonstrate the role of the chest CT in the current pandemic and could be useful for radiology leaders in their organization of local patient workflow.

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