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Guillain–Barré syndrome

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Editor – We read with great interest the article by Sancho-Saldaña *et al.*¹ Firstly, we would like to congratulate the authors who have nicely described a case of Guillain–Barré syndrome (GBS) following a SARS-CoV-2 infection and was found to have a leptomeningeal enhancement in magnetic resonance imaging (MRI) of the spine. Although there is numerous reporting on the neurological involvement of SARS-CoV-2 infection that has been published in the literature, we believe it is still justified to report any extrapulmonary cases of SARS-CoV-2 infection, as we are still learning about the disease. We have written a comprehensive review of literature on all published scientific articles of SARS-CoV-2 infection with neurological involvement and summarised the wide spectrum of presentation which can present with or without respiratory symptoms.² However, this article is the first to report leptomeningeal enhancement in addition to the GBS, which warranted further investigation.

Nevertheless, there is some important information that is missing in the article, which is the background history of the patient, especially in terms of her underlying comorbidity and also the list of regular medication that she is taking. We believe this information is vital to determine the aetiology and prognosis of the condition. Apart from that, we believe a follow-up reporting is warranted on the rate and status of recovery after rehabilitation and the presence of other neurological sequelae.

At any rate, we agree with the authors that the causal relationship between GBS and SARS-CoV-2 infection follows the classical para-infectious and post-infectious pattern, as shown in this patient. Moreover, although leptomeningeal enhancement is not uncommon in GBS, we believe that further study and close follow-up is imperative because, apart from being a supplementary diagnostic sign in GBS, it can be linked to the development of a neurological dysfunction, such as multiple sclerosis that can lead to long-term or permanent neurological disabilities.³ ■

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Lung ultrasound in COVID-19

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Editor – We thank Smallwood *et al* for their timely article about lung ultrasound (US) in COVID-19.¹ We completely agree that US can rule-in COVID-19 and that there is no published data on lung US and screening for COVID-19. We would gladly participate in the pragmatic research trial proposed, and would be happy to help in the set up. We have recently received numerous new US machines, as have many NHS trusts, have participated in a COVID-19 ultrasound database and published our ongoing experience.^{2,3} However, the main sticking point to all of this is the number of practitioners who can and/or are ‘signed off’ to perform a standardised lung US with adequate reporting tools (which hopefully will follow on from the database). I am a respiratory consultant by trade, and very experienced at pleural ultrasound. A few years ago, I had attended a focused acute medicine ultrasound (FAMUS) course with the view to get formally accredited. However, lack of trainers in the north east of England and engagement from radiology colleagues to mentor me locally mean that my colleagues and I are completely self-taught in lung US and know that we are competent and confident. I do not have a set programmed activity for teaching US or any of the governance aspects around it, although we are currently writing up a business case.³ I am sure that I am not alone in the UK. Furthermore, longitudinal competence programmes for basic point-of-care US do not exist.⁴ So, should this pandemic be a time for widespread upskilling of emergency care, acute medicine and respiratory practitioners and not just doctors? Perhaps, but then the governance behind this is mind-boggling, and perhaps hampered by years of underfunding and under-recognition. The recent incorporation of US training into the acute medicine curriculum is welcome but not timely enough.⁵ I am afraid there is no easy answer to any of this, and would welcome any further comments from lung US practitioners. ■

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