Response to: 'Correspondence to 'Prevalence of hospital PCR-confirmed COVID-19 cases in patients with chronic inflammatory and autoimmune rheumatic diseases" by Wu *et al*

We thank Wu *et al* for their interest in our study reporting on the prevalence of COVID-19 in patients with rheumatic diseases (RMD).[1] Wu *et al* suggest that potential detection bias due to more frequent regular visits to hospitals in patients with RMD may explain the observed greater prevalence of hospital COVID-19 cases in these patients. However, this is highly unlikely since the identification of hospital PCR+ cases was performed in April 2020, still in the peak phase of COVID-19 pandemic in Spain, when the regular scheduled follow-up visits had been cancelled and restricted to urgent medical care. In addition, PCR test shortage resulted in the need to restrict testing to 'COVID-19-likely' symptomatic patients at emergency departments, making unlikely preferential testing of RMD or other specific patients. Nevertheless, we cannot exclude other biases such as greater attendance to emergency departments of patients with RMD due to other factors, such as greater concerns on infection risk related to immunosuppressive therapy. However, this is also unlikely to be the case since in a further analysis of these patients, we found very high and similar rates of hospitalisation (>70%), which we can use as an estimate of severity in both RMD and reference cases, suggesting that the greater prevalence of COVID-19 in RMD is not explained by more attendance to emergency departments of milder cases.[2]

An important consideration to interpret our study is that we report crude prevalences not adjusted for age and sex. Since both factors influence COVID-19 severity, they should similarly influence the prevalence of hospital cases that are representative of the more severe symptomatic patients attending reference hospitals and not of mild community cases.[3] These factors may partially explain some of the observed differences in prevalences between different RMD groups, that is, the lower prevalence in patients with systemic lupus erythematosus (SLE), since these were younger and more often women than reference cases. Despite these limitations, our study suggests differences in the prevalence of COVID-19 among different RMD diagnostic groups, that is, non-SLE autoimmune diseases vs inflammatory arthritis, which do not seem to be explained just by age and sex bias.

With respect to the apparent greater prevalence of COVID-19 observed in Sjögren's syndrome in our patients, we should note that the number of cases with this condition in our study was small, not allowing for a firm conclusion on this aspect. Only grouped data on types of diseases (ie, SLE vs non-SLE autoimmune diseases) could be analysed. Comparing prevalence of hospital cases and outcomes in each RMD will require higher sample sizes.

It is not possible at this time to hypothesise a parallelism between the potential severity of RMDs and the prevalence of hospital COVID-19. In fact, our data do not support this concept. In our further analysis of the severity factors of COVID-19 in patients with RMD, age, sex or having any autoimmune or immunomediated disease, but neither chronic arthritis, nor therapies or comorbidities, were independent factors for severe outcomes.[2] Regarding the effect of antimalarials, we acknowledge that newer evidence suggests their unlikely therapeutic role. However, we believe that a potential prophylactic effect in patients with SLE patients is an still unanswered question, despite the results of the small study referenced by Wu *et al*.[4]

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Handling editor Josef S Smolen

Contributors All authors have contributed to writting this letter and have revised the final manuscript.

Funding The authors have not declared a specific grant for this research from any funding agency in the public, commercial or not-for-profit sectors.

Competing interests None declared.

Patient and public involvement Patients and/or the public were not involved in the design, conduct, reporting or dissemination plans of this research.

Patient consent for publication Not required.

Provenance and peer review Commissioned; internally peer reviewed.

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To cite Pablos JL, Galindo M, Blanco R, *et al*. *Ann Rheum Dis* Epub ahead of print: [*please include* Day Month Year]. doi:10.1136/annrheumdis-2020-219049

Received 10 September 2020
Accepted 11 September 2020

Linked

► http://dx.doi.org/10.1136/annrheumdis-2020-219000

Ann Rheum Dis 2020;**0**:1. doi:10.1136/annrheumdis-2020-219049

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Ann Rheum Dis: first published as 10.1136/annrheumdis-2020-219049 on 25 September 2020. Downloaded from http://ard.bmj.com/ on February 24, 2022 by guest. Protected by copyright.