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Commentary

Can coronavirus disease 2019 (COVID-19) trigger exacerbation of multiple sclerosis? A retrospective study

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Dear Editor,

I have read the paper entitled “Can Coronavirus Disease 2019 (COVID-19) Trigger Exacerbation of Multiple Sclerosis? A Retrospective Study” by Barzegar et al. (2021) with great interest, and I do believe that such studies should be conducted in order to further our knowledge, and to identify the possible post-COVID-19 complications in patients with MS (PwMS). However, there are a few points that I would like to share with the scientific community regarding this study.

First of all, in the methods section, the presence of gadolinium enhancement is stated as a criterion of definition for an MS relapse: “Relapse was defined as ... and confirmed by presence of gadolinium enhancement on magnetic resonance imaging (MRI).”. However, in the one to last paragraph of the discussion section (the limitations paragraph), lack of information regarding gadolinium enhancement in all patients is stated as a limitation of this study: “...lack of information on presence of gadolinium enhancement on MRI in all patients are other limitations of this study.”, which contradicts the previous statement and might be a source of confusion. Furthermore, in various instances in this paper, an episode of MS relapse was considered an effect of COVID-19 infection, which considering the limited data in this regard and the preliminary nature of this study, might be too soon to conclude. On this matter, in the first paragraph of the discussion section, the authors state that that the conclusion of this study is in line with previous studies regarding upper respiratory tract infections: “Our findings are in line with previous observation that upper respiratory viral infections (Edwards et al., 1998; Kriesel et al., 2004), of which 10-30% are caused by coronaviruses (Paules et al., 2020), is associated with risk of MS relapse.”. However, this statement may be inaccurate, confusing, and a source of a possible confirmation bias, as SARS-CoV-2, in contrast with most other coronaviruses, infects the lower respiratory tract, not to mention that in the first two cited papers in this statement (Edwards et al., 1998, Kriesel et al., 2004), coronaviral infections were not among the studied/detected infections. Furthermore, it is worth mentioning that another preliminary study showed no increase in relapse rates of PwMS after COVID-19 contraction (Etemadifar et al., 2021).

Most important of all, the inclusion of patients in this study was stated to be based on an MS clinic visit or contact after a COVID-19 infection: “All relapsing-remitting MS (RRMS) patients with a confirmed COVID-19 diagnosis based on reverse transcription-polymerase chain reaction (RT-PCR) who visited our outpatient MS clinic or contacted the center were included.”. This might be a major source of selection bias, as the COVID-19-infected PwMS who experienced a relapse are more likely to visit or contact an MS clinic, in comparison to PwMS who did not experience a relapse after their COVID-19 infection. There is also a lack of clarification regarding the chronology of the study and the participant flow e.g., when the study was conducted, if any patients were excluded etc.

Nevertheless, I appreciate the work from the authors in conducting this study and sharing it with the scientific community. I am hoping that more controlled studies in the future would clarify the possible relationship between MS relapses and COVID-19, informing and alerting PwMS and physicians.

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Declaration of Competing Interest

None

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