

LETTER TO THE EDITOR

Reactivation or reinfection of COVID-19 disease?

Dear Editor,

I have read the original article by de Araujo Torres et al. entitled to "Re-infection of COVID-19 after 3 months with a distinct and more aggressive clinical presentation: Case report" with great interest.¹ I have several comments/concerns on their article.

They describe a 36-year-old female medical doctor, without comorbidities, presenting new clinical symptoms of COVID-19 approximately 12 weeks (less than 3 months, 81 days later) after the first episode of COVID-19.

First, the precise potential for reinfection with SARS-CoV-2 is still not well-known, although some case reports from the world indicate strong evidence that it is occurring. Animal studies have showed that there is no reinfection with same strains of SARS-CoV-2.² Yet, reinfection may occur under the circumstances of poor humoral immune response or decreasing immunity over time even in the presence of protective antibodies.

Second, as they told in the article, there is still no consensus in literature of reinfection by SARS-CoV-2. There are some new articles recently published on how to confirm the diagnosis of reinfection with SARS-CoV-2.^{3,4} And they describe some markers such as cycle threshold (CT) values, viral culture growth, etc. For instance; CT values >35 might imply possible contamination rather than true infection. Proof of a reinfection is defined as two positive SARS-CoV-2 reverse-transcription polymerase chain reaction (RT-PCR) tests with CT value of <35 (or proof of replicating virus by cell culture or detection of subgenomic RNA [of genetically different lineages/strains of SARS-CoV-2]) at different time-points.^{3,4}

I think that the authors have to discuss their case under the new diagnostic criteria^{2,3} described in recent articles.^{3,4} Essential criteria for reinfection is demonstrating the two episodes of infection by different strains of SARS-CoV-2. In the article by de Araujo Torres et al., the second infection (so-called reinfection) was not confirmed with PCR results or C_t value and/or viral culture growth. RT-PCR from nasal swabs of their patient on the 11th (June 20) and 13th (June 22) days of symptoms of second infection was negative. Second infection (reinfection) was confirmed by not PCR but enzyme-linked immunosorbent assay method. However, how can they be sure about this higher immunoglobulin G (IgG)/IgA levels not related with the previous infection? In the beginning, during the first infection, IgG titers was negative as 0.477, but they surely could become positive around 3 months later. Discussed in the literature,² serology (immunoglobulins) may not play a factor in the reinfection definition and could be either positive or negative after the first infection.^{3,4}

Furthermore, the second infection (so-called reinfection) symptoms appeared 81 days (from March 20 to June 09, see figure 1) after

the onset of the primary SARS-CoV-2 infection, less than 90 days. Nevertheless, period of more than 90 days after the onset of the primary infection symptoms is necessary for diagnosing reinfection according to the proposed diagnostic criteria.⁴ Therefore, how the authors could differentiate/decide their patient as being a relapse/reactivation and/or repositivity case?

Third, there are some typesetting errors in the text of the article such as Mai 29 in figure 1 and DHL instead of lactate dehydrogenase (LDH) in page 1 on the ninth paragraph.¹

I think that a confirmed diagnosis of COVID-19 reinfection should be standardized and made according to the proposed diagnostic criteria in the literature.^{3,4}

CONFLICT OF INTERESTS

The authors declare that there are no conflict of interests.

AUTHOR CONTRIBUTIONS

Öner Özdemir did the all work in the manuscript.

Öner Özdemir 

Division of Allergy and Immunology, Department of Pediatrics, Sakarya University Faculty of Medicine, Research and Training Hospital of Sakarya University, Sakarya, Turkey

Correspondence

Öner Özdemir, Division of Allergy and Immunology, Department of Pediatrics. Faculty of Medicine, Sakarya University, Research and Training Hospital of Sakarya University, Adnan Menderes Cad., Sağlık Sok., No: 195, Adapazarı, Sakarya 54100, Turkey.
Email: ozdemir_oner@hotmail.com

ORCID

Öner Özdemir  <http://orcid.org/0000-0002-5338-9561>

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