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Authors' response

We thank the authors for their close reading of our article and comments on the same[1]. The preferred approach to select controls in a case-control study is to sample controls from a population which could have become cases in the study, had they developed the disease. In the investigation we conducted, controls had symptoms like that of the cases, but were not detected to have SARS-CoV-2 infection on real-time reverse transcription-polymerase chain reaction (RT-PCR) test, and thus qualified to be considered as controls. This similarity between cases and controls added strength to the process followed by us in the selection of controls and minimized biases, which could have otherwise been introduced if separate sampling considerations for cases and controls would have been used.

Contrary to the statement by the authors, we identified potential benefit of exposure to hydroxychloroquine (HCQ) in the univariate analysis and indicated the same in the results[1]. While doing so, we remained cognizant of the fact that the precision of a 95 per cent confidence interval is guided by the width of the interval (which was narrow in our study), rather than solely by the inclusion of null or any specific value within an interval[2]. Further, the adverse effects experienced by the study participants, during the course of HCQ intake as prophylaxis, were self-reported. As such, there were no provisions for undertaking specific adverse event monitoring within the mandate of the current design except for brief telephonic interviews.

While we reiterate that the evidence from randomized controlled trials (RCTs) are awaited to support further actions pertaining to pre-exposure prophylaxis, the RCT on post-exposure prophylaxis, cited by the authors, refers to a completely different clinical context[3]. The findings of the cited RCT had their own strengths and limitations, which were aptly elaborated upon in an accompanying editorial[4].

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