



## Comment on “The atherogenic index of plasma as a predictor of mortality in patients with COVID-19.”



### Letter

Dear editor,

The article “The atherogenic index of plasma as a predictor of mortality in patients with COVID-19” by Özge Turgay Yldrm et al. has been read with considerable attention.<sup>1</sup> It was a delight to read such a well-written paper, and we appreciate the writers’ extraordinary efforts. We concur with the conclusion of the article that the Atherogenic Index of Plasma (AIP) may be employed as a biomarker for death in COVID-19 patients. As a result, patients with elevated AIP levels should be closely watched and treated with care. With this, greater triglyceride levels and lower total cholesterol, LDL-C, and HDL-C values are seen in COVID patients who died, which may be a consequence of the SARS-CoV-2 on the lipid metabolism of the host subject. Nonetheless, a few extra points may have enhanced the work’s conclusion.

First, the study’s methodologies and designs raise significant concerns. The authors could have used multivariate Cox regression analysis using age, smoking habit, history of diabetes mellitus, myocardial infarction, stroke, and PAD (peripheral arterial disease), as well as statin therapy, as confounding variables in order to discover a connection between all-cause mortality and statin treatment. Additionally, the study’s retrospective design raises numerous issues, including the likelihood of recall bias and inaccurate patient reporting, which may have been mitigated if the researchers had included current patients at the time. Second, a single-centered study may be biased due to disparities in health, socioeconomic, and environmental characteristics and statistical analysis limitations. In addition, the tiny sample size may affect the findings’ justification. Regarding the matching phenomena, in 2017, Matej et al.<sup>4</sup> recruited three times as many male participants as female participants, with a nearly 1:1 ratio between the sexes, whereas the current study’s authors recruited significantly more male participants.

Third, the authors could have mentioned that AIP has also been found to have a strong predictive ability for ischaemic heart disease, since this would have made AIP a trustworthy and independent value for stratifying multiple cardiometabolic risks in COVID-19 patients.<sup>2</sup> Fourth, the authors should have specified the ethnicity of the patients throughout the research, as this would have revealed more about a diverse population. Finally, a research logistic regression analysis

showed that a moderate course of COVID-19 among Novosibirsk women is associated with BMI [odds ratio (OR) = 1.090, 95% confidence interval (95% CI) 1.019–1.166,  $p = 0.012$ ], and a severe course is associated with WC (OR = 1.041, 95% CI 1.001–1.084,  $p = 0.046$ ), AIP  $\geq 0.11$  (compared to both mild (OR = 13.824, 95% CI 1.505–126.964,  $p = 0.02$ ) and moderate (OR = 11.579).<sup>3</sup>

### References

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