In Response to “Association Between Youth Smoking, Electronic Cigarette Use, and COVID-19”

To the Editors:

We are writing with respect to an article by Gaiha et al [1] that was published on 11th August 2020 in the Journal of Adolescent Health. We believe that this article has multiple scientific shortcomings leading to flawed data analysis that render the results unreliable. Of particular concern is the fact that this flawed study is being used as ‘evidence’ by Congressman Krishnamoorthi to campaign for a ban on the use of all e-cigarettes in the U.S. [2]. It is critical that such public policy issues are based on reliable scientific evidence. Several public health figures have already provided detailed critiques of the study that illustrate the serious deficiencies in the analysis and conclusions of the article [3]. We set out our concerns regarding the study as follows.

The study subjects consisted of a poorly described sample where subjects self-reported all pertinent demographic factors over the internet in a variety of forums, and it is not clear how the sampling took place or the authors weighted this data to be “representative of the U.S. population.” The conclusions that can be drawn from this analysis are further limited because of the lack of adjustment for multiple variables, with the most important one being the number of positive cases to be adjusted for the number of tests. The data suggest that more tests would likely result in more cases detected. The actual overall positivity rate (based on total results shown in table 1) is similar for e-cigarette users and nonusers at 13.14% and 14.04%, respectively, and even slightly in favor of e-cigarette users. This suggests adjusting for number of tests may lead to different conclusions. Of particular note, the authors do not highlight that odds for solus past 30-day e-cigarette users to test positive for COVID-19 did not reach statistical significance.

No raw frequencies of number of cases were included. These should be provided for completeness and context for readers. With some low counts, this kind of analysis might not be robust.

No consideration is given to temporality, which means it is unclear how vaping before the pandemic relates to a COVID-19 diagnosis. Temporality is a key criterion for any conclusion of causality. We note that an article by Battha and Glantz investigating the association between e-cigarette use and myocardial infarction [3] was retracted in 2019 because of concerns about the lack of temporality analysis [4,5].

Finally, the methods used to calculate weights reported in the article's supplementary data are not well described. The article did not clarify whether the sampling method was probabilistic or provide enough detail about how the application of the weights in the supporting data was ensured to be appropriate (e.g., considered all weighting elements, such as design and post-stratification weights).

This article would have benefited from a clear and explicit section listing its limitations. Despite the multiple shortcomings identified in the article, we have found difficult to identify clear statements stating any limitations and hence departing from usual practice for peer-reviewed scientific articles.

While we recognize the importance of emerging science to improve our understanding of COVID-19, we believe that this article is not sufficiently robust and should have not been published in its current condition. Accordingly, we believe that the article should be thoroughly re-evaluated by statistical and epidemiological experts and a retraction considered if more clarity cannot be provided.

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References


Conflicts of interest: Oscar M Camacho and James J Murphy are current employees of British American Tobacco (Investments) Limited.