

ferences, recommendations from specialty (eg, radiology) societies, concerns about litigation, or possible financial considerations. However, our data could not address causal mechanisms.<sup>4</sup>

Adoption of USPSTF recommendations is estimated to be associated with reductions in false-positive mammography findings (61% vs 42% of women over 10 years) compared with nonadoption of recommendations, with fewer subsequent biopsies, surgeries, and other therapies for the treatment of benign tumors or indolent cancers.<sup>5</sup> Earlier screening has both risks and benefits; patient decisions are based on complex individual factors that are best elicited and addressed through shared decision-making. The potential harms of cancer screening are challenging to communicate. Decision aids may help, as women and their physicians tend to overestimate the benefits and underestimate the harms of screening mammography.<sup>4</sup> Optimal screening practices might differ for higher-risk populations (eg, Ashkenazi Jewish and Black women); this issue requires further study. Findings from ongoing clinical trials, such as the Women Informed to Screen Depending on Measures of Risk (WISDOM) study,<sup>6</sup> may help to guide discussions with women, assist with decisions about advertising mammography services, and provide standards for appropriate screening intensity.

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## COMMENT & RESPONSE

### Community Outbreak Investigation of SARS-CoV-2 Transmission Among Bus Riders in Eastern China—More Detailed Studies Are Needed

**To the Editor** We read with interest the report by Shen et al<sup>1</sup> and the ensuing media flurry that followed suggesting that the episode described provided evidence for “airborne spread” of the severe acute respiratory syndrome coronavirus 2 (SARS-CoV-2) virus. While we are aware that the virus may be “opportunistically” airborne during certain aerosol-generating procedures, we find it hard to believe that this was the case with this super-spreading event. There are several reasons for our skepticism:

1. According to an earlier report<sup>2</sup> of what appears to be the same event, the index patient was moderately symptomatic at the time of the bus trip, and had contact with a number of individuals at the religious event, including the first secondary case identified who traveled by car. Transmission may also have occurred during the meal, as in many other cases.
2. There is also the possibility that there was transmission occurring in Ningbo unknown to the authors, as was indicated by other investigators<sup>3</sup> who have suggested that the first cases in the city occurred on January 14, 2020, before the index case for this report. Without molecular epidemiology data, it is not possible to know if all the cases were indeed infected from the index case or elsewhere.
3. The role of fomites in transmission of SARS-CoV-2 has been shown by others,<sup>4</sup> and in a crowded bus full of pilgrims, this mode of transmission cannot be ignored.
4. If airborne transmission of the virus through recirculated air was the mode of transmission, it seems odd that only 2 of 16 individuals seated closest to the warm air vents according to the Figure<sup>1</sup> were infected.
5. It also seems odd that multiple secondary cases were reported (from Figure S1 in the Supplement<sup>1</sup>) well past 14 days, which is beyond the known incubation period of coronavirus disease 2019 (COVID-19). This skews the data toward a higher attack rate and may have led to spurious conclusions. A reporting of the 13 cases occurring within the first 7 days would have been much more meaningful.

We do not believe that this report supports the hypothesis that the virus is readily transmitted through the air because there are significant gaps in the analysis and interpretation of the data. More detailed studies of similar super-spreading events are needed with supporting molecular data to establish the mechanisms of the various routes of transmission to adequately control the march of this pandemic.

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**In Reply** Tambyah et al raise several reasons why they do not believe that our case report of bus riders in eastern China<sup>1</sup> supports an airborne transmission route of severe acute respiratory syndrome coronavirus 2 (SARS-CoV-2).

First, they note that an earlier report<sup>2</sup> states that on the day of the event, January 19, 2020, the index patient felt that she had a fever. We were unaware of this report until after our article was published.<sup>1</sup> After this report was brought to our attention, we learned of follow-up interviews of the index patient and family that were conducted by the field investigators who were in charge of the data reporting to the China Information System for Disease Control and Prevention (CISDCP) in February 2020, during which the index patient reported that she had a mild cough the night before the trip (personal communication, Dongliang Zhang, Ningbo Center for Disease Control and Prevention [CDC], September 4, 2020). However, these investigators, who are not among our coauthors, determined that there was not enough information to indicate that this mild symptom was coronavirus disease 2019 (COVID-19) related, and they did not update the original investigation report, which was a source for our study. The symptom onset date for the index patient is still recorded as January 19, 2020, in the CISDCP database. As we originally reported, the index patient “was initially asymptomatic during the bus trip but started to have cough, chills, and myalgias on the evening after returning from the temple.”<sup>1</sup> Identification of minor symptoms can be difficult to ascertain accurately retrospectively. However, we understand that the index patient felt well enough to attend the trip, and whether she was asymptomatic or minimally symptomatic does not change

whether or not the evidence of our study demonstrated airborne transmission of SARS-CoV-2. To address the concern about the onset date of symptoms for the index patient and to improve transparency about what was and was not reported, we have requested a correction to our article to include the information from the subsequent field investigation. In addition, we are adding an acknowledgment to thank the index patient and her family members for providing permission to publish this information.

Second, the authors suggest that transmission may have occurred during the meal at the event. However, it was unlikely to be the major transmission route, as otherwise we would expect secondary cases on Bus 1. Participants were mixed during lunch, and individuals on Bus 2 did not all stay together.

Third, we recognize that transmission could have occurred prior to the bus trip in the city of Ningbo. However, we think that this is unlikely. Spread of SARS-CoV-2 in Ningbo, a city with over 8 million residents, was in its early stages. The Ningbo CDC conducted thorough and detailed contact tracing but could not link any confirmed cases other than the index patient to the other infected participants in this worship event. In terms of other possible methods of transmission, we agree that fomites cannot be ruled out and included this in the Discussion section of our article.<sup>1</sup>

In terms of proximity to the warm air vents, 4 infected individuals (C28, C4, C20, C32) were seated beside a warm air vent.<sup>1</sup> Three additional infected riders (C2, C14, C15) sat between 2 air vents, and almost all passengers were within 6 feet of at least 1 air vent. We do not believe airborne transmission would necessarily produce a significantly elevated risk of infection among those seated closest to the air vents. Indoor airflow and heterogeneity in individual susceptibility may play a more important role than proximity to the air vents.

We disagree with the proposal of only including cases occurring within the first 7 days. There is still ongoing debate regarding the range of the incubation period of COVID-19. The current estimate of up to 14 days with a median of 4 to 5 days is based on several early studies.<sup>3,4</sup> A recent study<sup>5</sup> with a larger sample size estimated the median incubation period to be 7.76 days. Meanwhile, longer incubation periods were found in older adults.<sup>6</sup> The secondary cases reported in our study had a median incubation period of 7 days when excluding asymptomatic cases,<sup>1</sup> in line with the new study findings. Two of the secondary cases reporting late disease onset dates were asymptomatic (C30 on February 5 and C32 on February 13). Since they never had symptoms, their disease confirmation dates were recorded as disease-onset dates. The known incubation period of COVID-19 does not apply to asymptomatic cases.

While we acknowledge our study's limitations, we continue to believe that airborne transmission is the most likely route of transmission in this outbreak and that in closed environments with air recirculation, SARS-CoV-2 is a highly transmissible pathogen.

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## Physicians' Electrocardiogram Interpretations

**To the Editor** Cook and colleagues<sup>1</sup> should be congratulated for compiling studies on the accuracy of medical students' or physicians' electrocardiogram (ECG) interpretations. However, in reading their report, we identified several methodological issues that undermine the validity of their analyses and the suggested implications.

First, the analyses did not account for some specific factors of heterogeneity within and between study populations. For instance, medical students' exposure to and competency in interpreting ECGs might vary significantly depending on how advanced they are in their training (from year 1 to year 7 in some schools) and whether they are in preclinical or clinical years.

Second, the inclusion of data sets with extremely low sample size is problematic. Indeed, in Figures 3 and 4,<sup>1</sup> a large proportion of studies among resident physicians (13 of 37), practicing physicians (6 of 10), and cardiologists (7 of 8) included fewer than 30 participants, with a study that included only 1 participant. Such studies cannot provide precise and reliable estimates of the accuracy of ECG interpretation and should have been excluded from the meta-analysis. Alternatively, the authors could explore the effect of the study sample size on the reported accuracy by presenting the results of a test for small-study effect (with the related funnel plot), performing additional subgroup analyses by sample size, or performing a meta-regression.

Third, the significant between-study variability in the method used to assess the accuracy of ECG interpretations is concerning. In Figures 2 through 4,<sup>1</sup> the wide differences in diagnosis types and difficulties, and in the number of test items, make cross-study comparison difficult and the appropriate-

ness of a meta-analysis questionable. Moreover, most of the included studies (55 of 78) did not use a robust method to establish correct answers. The sensitivity analysis done to address this issue provides limited information in the absence of a stratification by level of training or qualification, which influences clinical competencies.

Fourth, the appropriateness of pooling together estimates from studies published 50 years apart is equivocal, in view of the recent developments in training and assessment of competencies in ECG interpretation.<sup>2</sup> Additional subgroup analyses by date of study publication would be useful to appreciate potential changes in accuracy over time.

Fifth, to evaluate the effect of an additional training on ECG interpretation skills, the authors compared pooled pretraining assessment scores with pooled post-training scores. This approach is not classic, and might be more problematic when some studies are only included in the meta-analyses of pretraining scores. For instance, 37 studies contributed to pooled pretraining scores and only 15 studies to pooled post-training scores, in residents. Ideally, changes in accuracy before and after the training should be pooled across studies using standardized mean differences, using only studies that performed both pretraining and post-training assessments.

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**To the Editor** We read with great interest the systematic review and meta-analysis by Cook et al,<sup>1</sup> who found that the accuracy of physicians' interpretations of electrocardiograms (ECGs), whether trained in interpretation or not, was inadequate, indicating the importance of enhancing ECG training for physicians. Electrocardiograms have been widely used from intensive care to regular physical examination, and have played an important role in the development of cardiovascular medicine.<sup>2</sup> The conclusions of this meta-analysis<sup>1</sup> are of great importance for medical construction. However, we raise 3 concerns regarding this systematic review and meta-analysis.

First, it is universally accepted that medical levels vary across different countries. A Global Burden of Disease study<sup>3</sup>