Carbon Dioxide Insufflation on the Number and Behavior of Air Microemboli in Open-Heart Surgery

To the Editor:

Although useful neuropsychological (NP) benefits may result from the recent report by Svenarud et al\(^1\) of a reduction in air microemboli in a randomized controlled trial of CO\(_2\) insufflation during single-valve surgery, there is a number of issues relating to cerebral microemboli, valve versus coronary bypass surgery, and NP outcome that are not adequately addressed in this paper. A number of studies have linked cerebral microemboli to NP outcome, but the relationship between the two remains unclear. Other factors such as inflammatory response and altered cerebral blood flow must also be considered. Neville et al\(^2\) compared 193 patients having coronary artery bypass surgery with 73 patients having valve replacement surgery and confirmed previous findings that there were more microemboli detected during valve surgery but there was no difference found in NP outcome. Their result suggests that the number of microemboli may not always be the most significant determinant of NP outcome. Additionally, Svenarud et al\(^1\) only measured microemboli within the heart and ascending aorta, and these may not correlate with cerebral microemboli.

I would be interested to know why Svenarud and colleagues\(^1\) did not assess cerebral microemboli when they have done so in the past.\(^3\) They quote two papers from the same group\(^4,5\) as evidence that air microemboli are harmful, but both papers are flawed. Taylor et al\(^6\) assume that the microemboli they detect are air, but this is unproven, and the technology to distinguish gaseous from particulate microemboli has not yet been used clinically. Borger et al\(^7\) found that the group with fewer than 10 interventions had less NP impairment after surgery; however, this group also had significantly shorter cross-clamp and cardiopulmonary bypass times. A further flaw was the failure to perform actual microemboli counts such that Borger et al\(^7\) were not able to make a direct correlation between the microemboli count and NP outcome.

How did Svenarud et al\(^1\) count microemboli on a 2-dimensional image over time? Counting cerebral microemboli as they appear sequentially on a Doppler recording is relatively straightforward. However, when microemboli are “whirling around in the left ventricle and left atrium,”\(^8\) it must be very difficult to keep track of which microembolus is which for a whole minute and not doublecount any. It is not adequately explained how the microemboli were counted so that their methods could be repeated by others.

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**Response**

We thank Dr Whitaker for his interest in our study.\(^1\)

First, as stated in the title, we investigated the “effect of CO\(_2\) insufflation on the number and behavior of air microemboli in open-heart surgery” (p 1127)\(^1\) and not the effect of the gas on neuropsychological outcome. We confirmed that most microemboli that appear in the heart after release of the cross-clamp stay there until the beating heart is being filled, which is in accordance with our earlier transcranial Doppler study.\(^2\) Moreover, we showed that the number of microemboli ejected into the peripheral circulation during and after weaning from cardiopulmonary bypass can be substantially reduced with CO\(_2\) insufflation into the cardiothoracic wound. Thus, it seems reasonable to assume that also the number of microemboli ending up in the brain will become less. It also seems reasonable to assume that the microbubbles that do end up there are less harmful to the brain if they consist of CO\(_2\) instead of air. Of course, these findings do not exclude that other factors, be it cerebral blood flow or inflammatory response, may influence postoperative neuropsychological outcome.

Second, regarding the question whether air microemboli are harmful to the brain, there is an abundance of studies showing that this is indeed the case. We refer to the review recently published in the *New England Journal of Medicine*\(^3\) and to the convincing study of Martens et al,\(^4\) who compared the effect of the injection of air and of CO\(_2\) into the carotid artery. “All animals of the ‘air’ group showed marked circulatory reactions leading to death in 2 animals. In the whole group, diffusion-weighted magnetic resonance imaging revealed irreversible hyperintense signals in both hemispheres” (p 51). Martens et al\(^4\) concluded, “In contrast to the dramatic effect of air emboli, identical quantities of carbon dioxide injected into cerebral arteries of the pigs were not associated with major clinical symptoms. The early reversibility of ischemic reactions visualized in diffusion-weighted magnetic resonance imaging encourages the use of carbon dioxide insufflation as a protective method in cardiac surgery” (p 51).

Third, it is clearly stated in Methods how the emboli were counted, so that others can repeat the methods. “The TEE probe was positioned in such a manner that a mid-esophageal long axis view could be kept. That view included 3 areas of interest, ie, the left atrium, the left ventricle, and the proximal part of the ascending aorta.\(^5\) Video recordings of this view were started from the release of the aortic cross clamp until 20 minutes after [cardiopulmonary bypass] was discontinued. After the study was finished, an examiner, who was unaware of the treatment given, analyzed the videotapes of all the patients. The maximal number of microemboli in the left atrium, the left ventricle, and the proximal part of the ascending aorta that appeared on one frame, was determined for each minute by scrolling the tape back and forth in slow motion. Thus, microemboli could be differentiated from moving heart tissue including the valve structures” (p 1128).

Finally, if the method had been unreliable, it would not have been able to discriminate between the 2 treatment groups.

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_Circulation_. 2004;110:e55-e56
doi: 10.1161/01.CIR.0000141262.91992.F1

_Circulation_ is published by the American Heart Association, 7272 Greenville Avenue, Dallas, TX 75231
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Print ISSN: 0009-7322. Online ISSN: 1524-4539

The online version of this article, along with updated information and services, is located on the
World Wide Web at:
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