

No potential conflict of interest relevant to this letter was reported.

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**THE AUTHORS REPLY:** Although randomized trials have much to offer, they too suffer from a form of selection bias: not treatment selection bias but rather bias in the selection of the patients who ultimately undergo randomization. This bias limits the generalizability of findings. Randomized trials comparing PCI with CABG have been further limited by small sample size, and most have become somewhat outdated. The ASCERT study is observational, large, and contemporary, and it used an exhaustive array of sound statistical methods to adjust for treatment-selection

bias. The effects of potential unmeasured confounders were considered in detail. ASCERT does not replace the randomized trials but rather complements them. The lower mortality of CABG as compared with PCI has been noted in both observational studies and randomized trials.

We recognize that survival is not the only outcome of importance in this field. For example, randomized trial data have shown an increased risk of stroke with CABG as compared with PCI. Data on stroke, myocardial infarction, and the composite, along with death, will be forthcoming in additional publications from ASCERT.

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## Low-Dose Abdominal CT for Diagnosing Appendicitis

**TO THE EDITOR:** The study by Kim et al. (April 26 issue)<sup>1</sup> explores a major issue in radiology. It is well known that decreasing x-ray tube kilovoltage and current is the simplest way to reduce radiation dose. But this reduction in dose is obtained at the penalty of image noise.<sup>2</sup> Recent developments in equipment are helping to reduce radiation dose without decreasing image quality. When used in standard abdominal examinations, both adaptive statistical iterative reconstruction and adaptive image filtering can reduce the radiation dose a patient receives by 80%, without compromising image quality and without increasing noise.<sup>3</sup> The authors did not state whether they had used such an algorithm.

Kim et al. used computed tomographic (CT) units with 16, 64, and 256 detector rows but did not conduct a separate analysis of radiation dose. Recently developed units equipped with 250 detector rows provide high resolution with low noise and low radiation dose. Outdated equipment with 16 detector rows cannot match this performance. Lowering the radiation emitted

during imaging is important, but it should not be accomplished at the price of poor diagnostic performance. The use of new algorithms, not compromised x-ray tube parameters, should be the method of choice in reducing patient exposure to radiation.

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**TO THE EDITOR:** In their article, Kim et al. present their experience with the use of low-dose CT in patients with suspected appendicitis. While the authors should be commended for the timeliness of this large, randomized trial, concerns remain regarding the limitations of low-dose CT when used in the diagnosis of patients with suspected appendicitis.

Patients with an appendiceal mass who undergo surgery on their index admission have a significantly worse outcome as compared with those whose care is managed conservatively, with a delayed appendectomy.<sup>1,2</sup> Among the 358 patients who underwent surgery for the treatment of presumptive appendicitis in the study by Kim et al., 3 were treated for an appendiceal mass with delayed appendectomy. At 0.84%, the detection rate for an appendiceal mass appears relatively low as compared with previously reported rates of up to 3.8%.<sup>3</sup> The poor sensitivity of low-dose CT for perforation (36.4%) may explain the low rate of detection for appendiceal masses. If low-dose CT is to be adopted, it will be imperative to define its role in detecting complications of appendicitis in order to ensure that patients are receiving appropriate treatment.

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**THE AUTHORS REPLY:** We did not use iterative reconstruction or other techniques that reduce image noise; these techniques might help to further reduce radiation dose. Our study was designed to provide a similar radiation dose across scanners. We cannot confirm whether the image quality was better with the newer scanner since we did not formally analyze image quality. However, we did not observe any apparent difference in qual-

ity among the images produced by different scanners. The comments on the new CT techniques being used to reduce radiation dose raise the important issue of evaluation. Although these techniques appear promising, thorough validation would be required before widespread adoption. In fact, our study may highlight a few critical points in such validation. First, the primary focus should be patient outcome or diagnostic performance rather than subjective image quality. Second, the acceptability of image quality generally varies across specific diagnostic tasks.<sup>1,2</sup> These points have often been overlooked in past studies.

As O'Leary et al. noted, three study patients underwent percutaneous abscess drainage and delayed appendectomy (one in the group receiving low-dose CT and two in the group receiving standard-dose CT). The proportion of patients with this outcome may vary according to the practice patterns of the hospital. More important, the percentage of such patients in our study (0.84%) should not be directly compared with the percentage in the study by Andersson and Petzold (3.8%).<sup>3</sup> The latter finding represents the proportion of patients who had abscess or phlegmon, and only a portion of those patients appear to have undergone percutaneous abscess drainage and delayed appendectomy. Although we observed a marginal difference in diagnostic sensitivity for appendiceal perforation between the low-dose CT group and the standard-dose group, caution is needed in interpreting these results, since we also included microperforations, which may have had little effect on patient outcome. Furthermore, simple numeric comparison of the outcomes related to appendiceal perforation between published studies may not be truly meaningful, since the definitions of appendiceal perforation are often unclear or inconsistent, as we discussed in the study protocol. Further validation of the use of low-dose CT in evaluating complications of appendicitis such as perforation would be worthwhile, but the formulation of more standardized, clinically relevant end points should come first.

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## Two Hundred Years of Surgery

**TO THE EDITOR:** The 200th Anniversary article by Gawande (May 3 issue)<sup>1</sup> omitted the fact that the first abdominal surgical procedure in the United States was an ovariectomy performed in 1809 in Danville, Kentucky, by Dr. Ephraim McDowell. The remarkable story, as well as a description of the procedure, is told by John L. Wilson in his 1998 manuscript, “Stanford University School of Medicine and the Predecessor Schools: An Historical Perspective,” available through the Lane Medical Library at Stanford (<http://elane.stanford.edu/wilson/html/chap4/chap4-sect10.html>).

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1. Gawande A. Two hundred years of surgery. *N Engl J Med* 2012;366:1716-23.

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**TO THE EDITOR:** Johns Collins Warren (1778–1856) had a major interest in ophthalmic issues and published three papers in this field — the first on cataract surgery<sup>1</sup> and the other two reporting cases of ocular trauma, ocular inflammation, and the recommendation of leeches to restore lost sight.<sup>2,3</sup> Nevertheless, his original contributions in ophthalmology are not known. Thus, the recent analysis by Gawande of Warren’s article on cataract surgery merits further discussion.

Jacques Daviel (1696–1762) had already introduced the extracapsular technique of lens extraction in 1747. Charles de Saint-Yves (1667–1733) in 1722<sup>4</sup> was the first to remove an intact lens that had been displaced owing to trauma to the anterior chamber. Much earlier, Sushruta (600 B.C.) devised a technique to expel extraocular cortical masses.<sup>5</sup>

In sum, during the time of Warren, the lens-extraction technique had already been known

for at least 65 years. Accordingly, it cannot be argued that he published “a new approach to the treatment of cataracts.”

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5. Mehta H. Extra-capsular cataract removal — not couching — pioneered by Sushruta. *Surv Ophthalmol* 2011;56:276-7.

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**THE AUTHOR REPLIES:** McDowell is right to point out that the first ovariectomy was, in fact, in 1809 — on an ordinary table in Dr. Ephraim McDowell’s home — and not in the 1850s. It was nonetheless decades before others took up the procedure, owing to its severe dangers in the absence of anesthesia or antisepsis. The surge of reports of successful hysterectomy and ovariectomy that began to appear after the advent of ether anesthesia indicated the confidence surgeons finally felt that they could now penetrate the abdomen safely.

Likewise, Grzybowski is correct in noting that Dr. John Collins Warren’s description of the cataract procedure was not reporting on an entirely new procedure but rather on a particular technique developed by a Mr. Gibson. I am grateful to both letter writers.

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