findings may represent overdiagnosis. However, we studied patients who had lung cancer diagnosed in growing nodules, confirmed by a panel of pulmonary pathologists. To their point that available evidence from observational studies may not support a conclusion that CT detection reduces mortality, that was not the study question in this article. Regarding the possibility of differential detection of clinically unimportant lesions by sex, the relevant question here is whether there is a sex difference in the propensity to diagnose lung cancer, as we discussed in the Comment section of our article.

We do not believe that, to any significant extent, men who started smoking at the same time as women were diagnosed earlier, before the onset of our study, and thereby differentially excluded; in our study, the median and mean ages at diagnosis were similar between the sexes, and diagnosis before 40 years of age is extremely rare, even among heavy smokers. We did note that other epidemiological studies have not found that women are more susceptible to smoking-induced lung cancer. Our discussion indicated that whether susceptibility to lung cancer among women is greater than among men remains unsettled.

Dr Bach notes concern about the use of the prevalence odds ratio instead of the incidence ratio. Prevalence odds and incidence ratios are related as follows: Prevalence = (incidence density × average duration) / (1 + [incidence density × average duration]). Therefore, prevalence odds (prevalence/[1-prevalence]) = incidence density × average duration. From this it follows that the prevalence odds ratio equals the incidence density ratio, given that the average duration is the same.

While we did cite epidemiological evidence at variance with ours, we regrettably did not refer to Bain et al., discussed by Bach and by Boffetta et al. That study did not show a higher susceptibility among women. One possible explanation of our result would be that, while lethal in both sexes, lung cancers in women do not progress as rapidly as in men, resulting in the cancers remaining asymptomatic yet detectable via CT for a longer duration on average; however, the indicators of the cancers’ “aggressiveness” that we addressed did not point to a such sex difference. Finally, compared with our study population, the mean pack-years of smoking was lower by a factor of 2 in Bain et al, which may reduce the ability to measure susceptibility.

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Rapid Response Team Responses

To the Editor: In response to the Commentary by Drs Winters, Pham, and Pronovost regarding rapid response teams (RRTs), we would like to describe why our institution became interested in implementing this intervention despite what the authors describe as equivocal published evidence.

Rapid response teams are specifically designed to address failure to rescue, which usually stems from (1) failure to recognize a problem, (2) failure to plan for the problem, or (3) failure to communicate regarding the problem.2 Sentinel event reviews, mandated by the Joint Commission on Accreditation of Healthcare Organizations,3 confirmed that these 3 themes were often present when unexpected occurrences unfolded at our institution. These reviews of our own care experience, albeit unpublished and observational in nature, convinced us that implementing an RRT was worth a try.

This situation may be similar to that of pulse oximetry, which was adopted as a standard of care not because of scientifically sound evidence from randomized controlled trials, but because detailed critical incident analysis suggested that this intervention was a feasible approach to reduce risks associated with anesthesia.4 Similarly, we concluded that our local critical incident analyses were more important than the currently available published data. Preventing failure to rescue is a major patient safety priority at our institution, and providing access to an RRT for our patients therefore was a logical choice for us.

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To the Editor: In their Commentary, Drs Winters, Pham, and Pronovost correctly identify an unfortunate trend in “quality improvement” in which initiatives are railroaded through that are inadequately supported by scientific evidence. However, I am a critical care fellow in an institution that quickly instituted the RRT after certain publications suggested efficacy, and the popularity of the RRT that I have observed among the house staff deserves comment.

At my institution, the RRT consists of the nursing coordinator of the intensive care units and the respiratory therapy
supervisor. In addition to providing patient assessment, these individuals provide immediate resources to residents, nurses, or others who believe a situation to be beyond their knowledge or capacity to manage. The result is that house staff, who may lack assessment or management skills, remain in control of the situation (thereby maximizing learning and building confidence) but can turn to experienced nurses and therapists for advice, skills, and facilitation of transfer if appropriate. Dopamine or other critical treatments can be rapidly initiated, and I believe that patients who are medically decompensated are more safely managed during the dangerous transition from a medical or surgical floor to the intensive care unit environment.

Whether RRTs should be standard in every hospital, I believe that they are a valuable resource in teaching hospitals, where inexperience may be a major source of preventable error, and the need to learn by doing must be balanced with patient safety.

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In Reply: Our Commentary reviewing the current published evidence on RRT systems suggested that the evidence in support of their widespread implementation is equivocal. In particular, the largest and best-designed study found no significant improvements in favor of the RRT. As such, the drive to push RRT systems as a nationwide standard of care needs to be reconsidered.

This, however, is not intended to be a condemnation of the MRT concept. On the contrary, MRTs may be effective. The problem is that we do not know, and the best evidence suggests that they are not. If the health care community wants to discard this evidence in favor of common sense, it seems that there is evidence; it is puzzling to us why it is ignored. The case of RRTs may be analogous to the broad use of estrogen therapy to prevent heart disease based on observational studies and pathophysiological reasoning. After randomized trials demonstrated no benefit, the use of estrogen to prevent heart disease was appropriately questioned. We would similarly expect dampened enthusiasm for RRTs as evidence from randomized trials emerged.

Hospitals that perceive benefit from RRTs should continue to use them. However, these local evaluations have a high risk for bias. We believe that the systems hospitals implement should be informed by local context and resources. For example, on surgical floors for which surgeons are frequently in the operating room, having hospitalists routinely available makes even more sense than using an RRT team when the patient is “pre-arrest.”

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One of the tenets of creating ultra-safe or highly reliable organizations is to reduce the urge to simplify. It seems that the health care community could heed this advice.

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