tor in reducing TB infection rates, we think it prudent to recommend HEPA-filter respirators based on the physical behavior of TB droplet nuclei and the effectiveness of these respirators in reducing inhalation exposure to particles in this size range.

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To the Editor:

In their commentary on isolation rooms for tuberculosis control, Nicas et al (1993;14:619-622) have framed appropriately the discussion on the occupational hazards of tuberculosis (TB) in healthcare settings. Specifically, I agree that we need to answer two principal questions: What is an acceptablerisk *of Mycobacterium tuberculosis* infection for healthcare workers? And on what evidence should TB control programs be based? (Possibly, a third question, relevant not only in developed countries but, more importantly, in developing countries where occupational TB is a much more substantial risk, is what resources are we as a society willing to invest to reduce occupational TB risks?)

I disagree with their answers to both of the questions. They recommend an annual occupational risk of *M tuberculosis* infection of 0.01%. This, they say, is analogous to similar risks judged acceptable for occupational carcinogenesis. The risk level that they proposed would appear to be substantially more rigorous than is currently in place for occupational needlestick injuries. I think any survey of healthcare workers almost certainly would suggest that disease transmission by needle injury is viewed by healthcare workers as being much more important to them than that of TB. Second, there obviously is a difference between the risks of potentially fatal occupational cancers and the readily treatable outcome of clinical TB. Finally, the use of *M tuberculosis* infection as the outcome measurement ignores the clinical fact that only 10% or fewer of infected individuals will develop clinical TB over their lifetime[1]; thus, 90% of the individuals who develop the outcome of interest will never have any clinical effect of this outcome. Surely, these clinical data should be used to increase the annual acceptable risk at least by an order of magnitude.

Nicas et al propose an answer to the second question that physical science principles should be employed when developing TB control programs. They suggest that, if droplet nuclei have aerodynamic diameters of 1 to 5 microns, then physical apparatuses that will capture such droplet nuclei efficiently would be used reasonably to reduce the hazards of occupational TB. I suggest, on the other hand, that the level of evidence for occupational protection of healthcare workers should be analogous to that used elsewhere in clinical medicine for therapeutic interventions? and that evidence based on efficacy ("can it work") really is inadequate for any level of decision-making. A more relevant standard would be effectiveness ("does it work"). I am concerned that many of the standards being suggested for occupational TB control are based on efficacy data alone. These would include not only disposable dust/mist filter respirators, but also particulate respirator devices and ultraviolet light. There is no evidence that these devices, when implemented on top of more established control measures, such as the identification of infectious patients, early institution of antituberculous chemotherapy, and air handling controls, will produce an added benefit in reducing TB hazards to healthcare workers.

In my own institution, almost every instance of occupational acquisition of *M tuberculosis* infection can be attributed to failure to diagnose an infectious patient. Consequently, the elaborate (and expensive) additional precautions being proposed likely would have almost no impact on the well-being of our workers unless they were applied universally. We probably would produce more effective use of scarce resources by emphasizing through policies, programs, and educational efforts the importance of early diagnosis. Others may find that resources spent on patient follow-up and directly observed therapy would have the greatest impact on reducing occupational risks. What is needed is clinical trial evidence that proposed control measures will reduce the risk of developing a clinically relevant outcome.

In hospital infection control, we have made a great deal of use of standards, national and otherwise, in guiding our practices. We would do well, however, to remember the words of the physician-playwright Anton Chekov, who once noted, 'There is no national science just as there is no national multiplication table; what is national is no longer science."

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The authors were given the opportunity to reply to Dr. Taylor's letter but have chosen to respond at a later time.

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