In this issue, Drs Saquib, Saquib and Ioannidis perform a valuable service by reviewing the evidence that screening for various diseases save lives. The authors examined the randomized controlled trials (RCTs) and meta-analyses of trials of the various screening strategies, and then assessed the outcomes of disease-specific mortality and all-cause mortality. They found that evidence of an effect on disease-specific mortality was relatively uncommon, and that evidence of an effect on all-cause mortality was essentially non-existent. The authors conclude that the effects of screening on mortality are likely to be modest at best, and that future evaluations of screening tests for diseases where short- and medium-term mortality are common, RCTs should be the default evaluation tool and disease-specific and all-cause mortality should be the main outcomes.

This raises the larger question of what should be the evidence upon which to base decisions about the appropriateness of screening tests, which by definition are
Commentary: Screening: a seductive paradigm that has generally failed us

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Screening healthy people has face value and great public and political appeal. It looks so simple, and yet screening is fraught with difficulties. These start already with the terminology, and common slogans like, ‘Catch the disease early, before it has produced any symptoms!’ are misleading on two counts.

First, disease means lack of ease, which is not what we understand by being healthy; but people who work

administered to large masses of the general population who do not exhibit any signs or symptoms of the disease. The purpose of screening is to detect in a preclinical phase the presence of a disease or precursor to a disease whose subsequent clinical course can then be ameliorated or even eliminated with treatment at that stage, in comparison with beginning treatment when the patient develops signs or symptoms of the disease (I am intentionally ignoring the issue of screening for genetic diseases, where a better understanding of prognosis in the absence of effective treatment can be considered an important outcome, as well as the prevention of a genetic disease in any offspring).

What needs to be considered then, are the outcomes of the disease for which screening is being contemplated. Although death is of course an important outcome, it is not the only outcome, and for some diseases may not even be the most important outcome. Many chronic diseases, such as heart failure, diabetes and chronic obstructive pulmonary disease (all three included in the set of diseases studied in this analysis) have numerous symptoms and outcomes other than mortality, such as dyspnoea, blindness, kidney failure and amputation. Even in the absence of any effect on mortality it is easy for me, as a primary care clinician, to imagine that patients would highly value any screening test and intervention that decreased the risk or severity of these outcomes. So I do not agree with the authors that, for these diseases, any screening test should be assessed with mortality as the main outcome. Whether these values are common among a broad community of patients deserves further study. Then there is the issue of patient preferences for different outcomes. Even if there is no effect on all-cause mortality, my clinical experience is that most patients would prefer some other cause of death to a death from cancer. Therefore for most diseases, I do not think that all-cause mortality should be considered the main outcome. Where the authors’ data are most compelling is the evidence or lack thereof for a disease-specific effect on mortalities of cancers. Here, my clinical experience is that what patients are most concerned about is death from that cancer, be it lung, prostate, breast etc., and reducing the risk of that outcome is their paramount concern. It is hard for me to imagine having any enthusiasm for a screening test for cancer without convincing evidence that it would reduce disease-specific mortality.

The second issue I wish to comment on is what constitutes convincing evidence. The authors claim that this must come from randomized controlled trials with one group being offered screening and the other group not getting screened. For the most part, I agree with them. But there are exceptions. Cervical cancer screening has not been subjected to the kind of randomized controlled trial advocated by the authors, yet the observational evidence that mass screening programmes have had a beneficial effect is sufficiently strong to conclude that there is a cause-and-effect relationship. However, this is a historical issue, and I can agree with the authors that newly proposed tests should be subject to randomized trials assessing their benefits and harms.

In sum, the evidence synthesized by Drs Saquib, Saquib and Ioannides should be considered by anyone contemplating clinical practice guidelines about screening or proposing new screening tests. We have let too much get into routine practice without an adequate evaluation, and once widely disseminated, it can be very difficult to re-orient patient expectations and clinical behaviours to an understanding that a randomized trial comparing screening with no screening is ethically justified.

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