Title

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In the early 1990s, a series of randomized trials demonstrated the efficacy of both pharmaceutical aids with behavioral therapy for smoking cessation and treatments for metastatic breast cancer. These treatments were approved by the US Food and Drug Administration and quickly disseminated into clinical practice. A decade later, the population mean survival following a metastatic breast cancer diagnosis had improved 50%, demonstrating the effectiveness of the treatment (1); however, there was no improvement in the proportion of smokers who had successfully quit (2). A critical public health question is how to account for the apparent lack of translation of quitting success into the population.

In our study, smokers who reported using a pharmaceutical aid in their recent quit attempt rarely paired this with behavioral therapy. Indeed, the high quality of behavioral therapy used in well-designed efficacy trials is not generally accessible in the community and thus challenges the scalability of the combined efficacious interventions. Using established propensity score matching methods, we chose comparison groups who did or did not use a cessation aid that were matched on baseline characteristics that might confound an assessment of successful quitting. We achieved close matching on numerous confounders, which led to precise results indicating a null effect that was robust to several sensitivity analyses.

In their correspondence, Shiffman and Gitchell are concerned that our results could be confounded because of biases associated with an observational study. They note that use of propensity score matching was a "heroic" attempt to balance potential confounders in our study. But they are not sure about so-called "confounding by indication." Yet we obtain good balance on self-efficacy and smoking intensity, two variables associated with both use of a pharmaceutical aid and success. Further, they worry about unmeasured/residual confounding. We note that in order for a yet-to-be-identified variable to counteract a twofold higher quitting success from use of a pharmaceutical aid, it would need to have a substantial negative impact on quitting. If such a variable exists, let's hope someone identifies it soon.

They appear to believe, along with others (3), that good evidence from multiple efficacy trials is all that is needed to make causal inferences on population effectiveness. Yet, eligibility criteria for the smoking cessation trials would rule out more than half of the smokers in the US population (4), which in itself is sufficient to question the generalizability of trial conclusions. We agree with Imai et al. (5) that avoiding confounding is important in both experimental and observational studies. They go on to note that many current proponents of one study design over another often misunderstand how the other method can provide evidence for causal inference. Our results suggest a problem with how interventions from cessation trials have been disseminated to the population. There is an urgent need to revisit how we help people quit smoking (6).

Notes

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