We thank Lillard et al. (1) for their comment on our recent practice of epidemiology article (2). Our study concluded that the reconstruction of smoking prevalence through a simple tool offered a feasible approach for countries lacking previous smoking surveys to assess historical trends in cigarette smoking (specifically in developing countries in the initial stages of the tobacco epidemic). This tool also allows for correction of the excess mortality rate among smokers, although this excess mortality rate had an impact only on the prevalence of smoking in men in the first decades of the study. Given the complexity of the mortality correction method, we recommended against it if the objective of the reconstruction is to evaluate recent smoking prevalence rates.

Lillard et al. affirm in their letter that “a large body of literature validates this type of data in general and smoking data in particular,” even using “an improved method with better data” (1, p. 656). We thank Lillard et al. for their comprehensive review of previous studies on this topic. Most of these studies, however, aimed to test the validity of individual retrospective reports rather than to validate cigarette smoking prevalence rates in a given population. On the other hand, inferring from these individual-level studies that prevalence rates can be correctly estimated could be a case of the atomistic fallacy (i.e., that individual-level inference applies to population-level data (3)). Different processes may be at work in determining individual cases of smoking and population-level prevalence (4). One of the cited studies did indeed validate smoking prevalence rates, and we thank Lillard et al. for pointing us to their work on this topic (5). We are also glad to see that their results in the United States are analogous to ours (2). The conclusion that mortality correction is especially important in men is also consistent across both studies (2, 5).

In their second broad point, Lillard et al. (1) indicate that the excess mortality correction method we used (6) is outdated, pointing to their work on this matter (5). The authors also claim that improved methodology is available to tackle measurement error in retrospective data (5). Although we agree that more refined methods might sometimes be desirable, we remain convinced that simplicity in terms of data is required if we want to use this method in settings characterized by low resources—settings that may benefit the most from retrospective reconstructions. As we stated previously, “We are convinced that this is an affordable method developing countries can use to obtain reliable data on historical trends in smoking in their populations. While this method may not ultimately generate a completely accurate smoking prevalence rate for a given year (especially for women), it provides valid and useful information on historical smoking trends” (2, p. 19). We thank Lillard et al. (1) for their comments and for their shared interest on the reconstruction of historical tobacco dynamics as a tool for better tobacco control.

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