In their recent paper, Barone-Adesi et al. conclude that smoke-free policies may result in short-term reduction in admission for acute myocardial infarction (AMI). However, from the methods and data presented in this paper, we believe that the observed effect may not be real and that the authors’ conclusion must be treated with caution.

Our main concern is the manner in which the authors present their data. They have chosen not to provide simple information about the rate of AMI over time. Instead, they compare the 5-month period during the ban (February–June 2005) with the same period the previous year. Using this technique, they have been able to demonstrate a marginal effect. However, no explanation has been offered as to why a 5-month period was chosen or what effect is observed when a different time period is considered. In our opinion, this raises the possibility of selection bias.

Even if any selection bias is ignored, the significance of the results remains questionable. It should be noted that the marginally significant overall rate ratio (RR) of 0.89 (95% CI 0.81–0.98) is largely attributable to an RR of 0.75 in females under 60. This favourable RR was calculated comparing February–June 2005 (during the ban) with February–June 2004. However, looking at Table 1, it appears that the high rate of AMI among women under 60 in February–June 2004 is anomalous and unlikely to be related to smoking. This is supported by the fact that the rate in women under 60 had decreased even before the ban was imposed (RR 0.88, comparison of October–December 2004 with October–December 2003).

In conclusion, this study’s methods have introduced selection bias. This bias, in conjunction with the very marginal and possibly insufficient RR, means no robust conclusions about the effectiveness of the smoking ban can be drawn from these data.

Reference