Passive smoking and the tobacco industry

To the Editor—Repase and Lowrey’s article on passive smoking in the workplace claims to provide a rebuttal to arguments on the smoking of tobacco products (ETS) used by the tobacco industry. The authors imply or, in cases, specifically state that the industry takes quotations, examples and studies totally out of context in formulating its arguments. I would argue that the industry does not do so, but that in fact it is Repase and Lowrey who could be accused of doing so in this article, in the enthusiasm of their attempt to discredit the scientific arguments of the tobacco industry.

The paper is riddled with statements that simply cannot be justified by the current scientific data. They claim, for example, that “Epidemiological studies of passive smoking show conclusively that exposure to ETS is a cause of lung cancer in nonsmoking women.” They refer to a study (much criticised in the scientific press) by Fontham and colleagues that at the time of writing, was the largest case-control study ever done and that reported a small, but statistically significant increase in risk. However, at the time a study had been carried out by Wu-Williams et al., which differed in size from the Fontham study by only three cases, and reported a statistically significant reduction in risk for those exposed to ETS! Both studies have since been superseded by a larger case-control study by Brownson et al., who report no statistically significant increase in risk for exposure to ETS during adulthood, by the spouse or at the workplace, or in childhood.

This situation of conflicting data is typical of the ETS story; of the more than 30 studies currently published on this topic, the vast majority (around 80%) do not report a statistically significant association between spousal ETS exposure and lung cancer. Why do Repase and Lowrey fail to disclose these facts, unless it is because they can be accused of exactly the same bias that they assign to the tobacco industry?

It is difficult to see how Repase and Lowrey can use this evidence to justify the claim that exposure to ETS other than by spousal smoking, for example in the workplace, must therefore also result in an increase in risk. Of the 12 studies that have investigated exposure to ETS in the workplace and an increased risk of lung cancer, again, 80% do not report a statistically significant increase in risk.

Looking at this evidence, it is difficult to see how the authors can criticise either the tobacco industry—or, for that matter, the many independent scientists (whether or not they consult for the tobacco industry) who have also questioned the evidence—for hold-

In reply—In her letter to the editor protesting our indictment of the tobacco industry’s highly deceptive practices, we are grateful to Sharon Boyse of the British-American Tobacco Company, North America for providing us with several textbook illustrations of how the tobacco industry quotes scientific studies and methods out of context and ignores contradictory studies in formulating its arguments on the subject of environmental tobacco smoke (ETS).

The tobacco industry would have the public believe that statistical significance of epidemiologic studies is the gold standard to be used in judging the potency of suspected environmental carcinogens. Such a cavalier attitude toward public health is common among the tobacco industry and its consultants, and perhaps is a prerequisite for those who would successfully market tobacco products. However, standard scientific criteria employed by public health authorities for judging carcinogenicity employ the total weight of evidence, of which statistical significance is only one of many factors. One of the most important of these factors is biological plausibility: e.g., intentional exposure to tobacco smoke has been known for many decades to be a massive cause of lung cancer. Moreover, there is plenty of statistical significance in passive smoking epidemiologic studies if one only looks for it. If only the highest ETS exposure categories were considered in the numerous epidemiological studies of passive smoking and lung cancer (in Greece, Hong Kong, Japan, USA, Sweden, and China) where this information is reported, the odds ratio is 1.91 (90% confidence interval = 1.60-2.08), (p < 0.000001). Perhaps such a p-value is not considered significant enough by the tobacco industry?

Boyse makes similar complaints about lack of statistical significance in workplace studies of passive smoking. However, in studies of passive smoking and lung cancer, the comparison of more-exposed non-smokers to less-exposed non-smokers, rather than to unexposed non-smokers, due to the environmental pollution of buildings with tobacco smoke, decreases statistical significance and systematically depresses odds ratios.

To support the tobacco industry’s argument that passive smoking does not cause lung cancer, Dr. Boyse cites a large case-control study of passive smoking and lung cancer among women in two industrial cities in north-east China by Wu-Williams et al. which she correctly says reported a statistically significant reduction in lung cancer risk associated with exposure to ETS. However, in BAT’s shameless quote-out-of-context, Boyse omits all mention of Wu-Williams’ conclusion that “Perhaps in this study population the effect of environmental tobacco smoke was obscured by the rather heavy exposures to pollutants from coal-burning Kang, other indoor heating sources, and high levels of neighbouring air pollution.”

In further support of BAT’s claim, Dr. Boyse cites a second large case-control study by Brownson et al., which she asserts found “no statistically significant increase in risk for exposure to ETS during adulthood, by the spouse or at the workplace, or in childhood.” True. However, Boyse omits to say that Brownson et al. did report statistically significant increases in risk for all subjects (lifetime non-smokers plus ex-smokers), and for lifelong non-smokers with heavy ETS exposure. Further, Boyse ignores all mention of the primary conclusion by Brownson that “On and other recent studies suggest a small but consistent increased risk of lung cancer from passive smoking. Comprehensive actions to limit smoking in public places and work sites are well-advised.” Dr. Boyse also includes two other new studies on passive smoking and lung cancer, published earlier than the Brownson study, which found statistically significant increases in lung cancer associated with passive smoking.

Thus, even while vehemently denying the practice of the selective citation and the out-of-context quote, the tobacco industry cannot refrain from using this technique to mislead the public. This practice has become as addictive as nicotine.

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Smoke Screen award

To the editor—in your editorial entitled “Tobacco sales in pharmacies: mixing good drugs and bad drugs,” you suggested a number of useful ways to encourage pharmacists to stop selling tobacco and cigarettes. However, ANSR presented its first annual Smoke Screen award to Wailgren Drug, the"
tobacco control organisations were to "honor" them in a similar way.

SANDRA D SANDELL
Association for Nonsmokers-Minnesota
St Paul, Minnesota, USA

1 Davis RM. Tobacco sales in pharmacies: mixing good drugs and bad drugs. Tobacco Control 1993; 1: 84-6.

The 'habit' of nicotine

To the editor—Let me express my congratulations for your editorial1 on the "habit" of calling nicotine addiction a "habit". Your past and present efforts to purge the word "habit" from our lexicon are commendable.

In this context, I should mention that during the preparation of the Pan American Health Organization report on "Tobacco or Health: Status in the Americas" (recently published in English and Spanish), a special effort was undertaken to purge the use of the word "habit" in connection with the consumption of tobacco.

Unfortunately, this practice is extremely pervasive, both in the lay and scientific languages, and is even more common in Spanish because the word "smoking" is too often translated as "el hábito de fumar" (the habit of smoking). Nevertheless, a new term—"tabaquismo"—has been coined and is beginning to become popular. This term conveys the connotation of an addiction (similar to alcoholism, heroinism, etc) rather than that of a mere habit as inoffensive as drinking milk or shaking hands.

Let us join our efforts now to "kick this habit."

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