Smoking verification and the risk of myocardial infarction

We read the paper by Godtfredsen et al with interest.1 The paper reported on the effect of smoking reduction on the incidence of myocardial infarction (MI) and found that although patients who stopped smoking had a decreased risk of MI, those who reportedly reduced their smoking did not. The conclusions drawn were that smoking reduction, rather than complete cessation, did not produce any benefit with regard a reduction of risk of MI. The major drawback to this study is that the information about smoking was totally reliant on self reported smoking habit. There is abundant evidence that patients who smoke, when questioned about a smoking habit, frequently under-report their cigarette consumption or deny smoking altogether. The more significant the effect smoking has, the greater the “social desirability bias”, so increasing the likelihood of denial. To overcome this bias biochemical verification of smoking by measurement of nicotine metabolites, specifically cotinine, has become almost obligatory.

To improve the accuracy of information about smoking and to facilitate easier nicotine metabolite measurements we developed a six minute point of care test called SmokeScreen.2 The easy to use colorimetric urine test can provide qualitative, semi-quantitative, and quantitative measurements of nicotine intake. Using this test we undertook an audit of smoking habits of 154 new patients attending a large inner city hospital taking an audit of smoking habits of 154 new patients attending a large inner city hospital cardiology outpatient clinic, comparing the test identification of smoking with self completed questionnaire of current smoking habit. The results identified 112 (72.7%) patients as non-smokers, 30 (19.5%) as confessed smokers, and 12 (7.8%) as “smoking deceivers”. We followed this with another study of the same population (n = 85, 33 smokers and 22 never-smokers) to examine the interaction of smoking and risk factors associated with coronary artery disease, as assessed by a biochemical screening and a blood count. Interestingly, none of the parameters measured in the biochemical screen, such as cholesterol, HDL and triglycerides, urea and electrolytes, and liver function tests were associated with smoking habit or quantitative measurement of exposure in the individual smoker.

We agree that nowadays almost every study of smoking habits apply one or more measurements of biochemical markers of smoking in addition to self report. It is also correct that in our paper the study participants are divided into the different smoking categories on basis self reported smoking and changes in smoking. However, as mentioned in the discussion, measurements of expired carbon monoxide (CO) and serum cotinine were undertaken in one of the follow up examinations. We found increasing levels of CO (table 2) and cotinine (not shown) with increasing self reported tobacco consumption, indicating that underreporting of smoking alone cannot explain our results, but clearly misclassification cannot be ruled out in this observational study. Furthermore, a previous review and meta-analysis3 concluded that self reported smoking is an accurate measure of tobacco exposure in population based studies, whereas this is not the case in intervention and clinical studies. Our data were based on a sample of the general population; participants with known coronary heart disease before study entrance were excluded. In addition, information on smoking habits and changes in smoking were part of a large questionnaire initiated in the late 1970s and the 1980s, thus minimising the risk of “social desirability bias” in this study.

Cope and Batterby describe a pilot study using a urine cotinine test for measuring nicotine intake. There are various methods of validating tobacco intake including biochemical markers, and cotinine is one of the better because of its comparatively long half life and the possible linear relation with number of cigarettes smoked. However, cotinine is not very useful in smoking reduction studies as most of the participants in these trials are supplied with nicotine replacement medicaments. Interestingly, the intervention studies of smoking reduction all report that despite nicotine replacement the percentage decline in amount of tobacco is followed by a smaller decline in biochemical markers of smoking exposure. Evidence of the effects of reduced smoking on risk of coronary heart disease is limited. The few ongoing smoking reduction trials report favourable changes in blood analyses of parameters of arteriosclerosis up to two years after smoking reduction. Unfortunately, these studies have a very high “drop out” percentage, but it will be interesting to see the clinical results of a long term follow up in this type of “risk reduction” study.

In summary, we believe that the self reported smoking habits in our study are fairly precise. However, biochemical verification of smoking is necessary in intervention and clinical studies although there are no ideal markers of tobacco exposure specifically with respect to assessment of smoking reduction.

G F Cope, N Batterby
Institute of Research and Development, University of Birmingham, UK

Correspondence to: Dr G F Cope, University of Birmingham, Birmingham Research Park, Vincent Drive, Edgbaston, Birmingham B15 2SQ, UK; g.cope.mermaid@ukonline.co.uk

References

Authors’ reply
We appreciate the comments from Cope and Batterby on our paper reporting the association between smoking cessation and smoking reduction and subsequent risk of myocardial infarction. Specifically, they propose that the lack of a beneficial effect of reduced smoking—in contrast with smoking cessation—could be attributable to inaccuracy (under-reporting) of the self reported tobacco consumption. In addition, they raise the important question of which measurement method most accurately reflects tobacco exposure in the individual smoker.
Fibrinogen, social position, and risk of heart disease

The report by Jousilahti and colleagues adds to growing evidence of a consistent association between serum inflammatory markers—particularly fibrinogen—and social position. The authors interpret their data as suggesting that the fibrinogen-social position link is not merely a reflection of the social patterning of prevalent disease, smoking, and obesity (all of which are associated with increased serum fibrinogen and lower social position) as a strong trend of increasing fibrinogen with decreasing social status survived statistical adjustment for these confounders. Fibrinogen, they conclude, is therefore a promising candidate for the “missing link” between social position and cardiovascular health.

The authors’ reasoning implicitly accepts that fibrinogen is a cause of coronary heart disease (CHD). However, this runs contrary to recently published evidence using the principle of “Mendelian randomisation” (the situation where a particular genetic polymorphism influences exposure level of a putative disease risk factor, and should in turn be related to increased risk of disease if the risk factor is indeed a cause). In fact we discussed this evidence in a recent commentary on psychosocial explanations of health inequalities. Plasma fibrinogen concentrations are related to a polymorphism in the β-fibrinogen gene, with presence of the “T” allele being associated with higher levels. Among controls of a recent large case-control study, fibrinogen increased by 0.12 g/l per T allele present. Comparing cases with controls, a 0.12 g/l higher fibrinogen was associated with a relative risk of CHD of 1.20 (95% CI 1.13 to 1.26). If increased fibrinogen actually causes heart disease then a similar per allele relative risk of CHD should be seen. In fact the per-allele relative risk of CHD was 1.03 (0.96 to 1.10). People whose genotype would cause heart disease then a similar per allele increase in fibrinogen and CHD risk are not causal. This finding is in keeping with evidence from randomised controlled trials that suggests that drugs lowering fibrinogen do not decrease the risk of CHD.

Fibrinogen probably predicts cardiovascular events because of reverse causation (atherosclerosis is an inflammatory condition and raises circulating fibrinogen concentrations) and because of confounding—smoking, abstaining from alcohol, not exercising and being poor are all associated with raised fibrinogen and themselves increase the risk—or are markers for factors that increase the risk—of cardiovascular disease.

The data presented by Jousilahti and colleagues illustrate how easy it is to misattribute causality to associations in social epidemiology. Many factors are socially patterned and thus appear as possible candidates for a causal role in the processes that generate any disease outcome that is also correlated to fibrinogen. Arguing away a statistical independence of associations between such exposures and outcomes does little to infer their causal basis as it is often likely to reflect issues of residual confounding and measurement imprecision of correlated covariates. Strategies such as randomised control trials and Mendelian randomisation can help untangle these issues. Where feasible, they should be more widely adopted in epidemiology.

Air pollution and asthma in children

Lin and colleagues have published an interesting paper on asthma hospitalisation in children. They concluded that the study showed positive relationships between gaseous pollutants and asthma hospitalisation in children and that the effects of certain specific gaseous pollutants were found to vary in boys and girls. There are several issues that should be discussed before these conclusions can be confidently accepted.

The authors performed separate regression analyses for boys and girls, and compared the sexes by examining the odds ratios. Examination of their figure 1 suggests the possibility that differences between the sexes might be chance fluctuations. It has been recommended that statistical tests for interaction, which directly examine the strength of evidence for the treatment difference varying between subgroups, are the most useful approach for evaluating subgroup analyses. The most simple hypothesis is that there is no difference in susceptibility between boys and girls. It would thus be helpful to know if these apparent differences are statistically significant.

A consideration in performing hypothesis tests is the standard error of the coefficients. The authors have treated all hospitalisation events as independent. It is probable, however, that some children were admitted to hospital more than once during the course of the study. I have compiled a cohort of some 108,000 people from primary care and respiratory practices in the cities of Hamilton and Toronto, Ontario (Toronto was the setting for the study by Lin et al). Hospitalisations for asthma, 1992–1999, were ascertained by linkage to the Provincial Hospital Discharge database. Ninety four children, aged 6–12, were hospitalised a total of 145 times, with a mean admission frequency of 1.5 times. Twenty two per cent of children were admitted more than once. Table 1 displays the distribution of numbers of asthma hospitalisations. The study of Lin et al is thus, in a sense, a repeated measures longitudinal study. Failure to take account of the independence of events will lead to underestimation of the standard errors and the possibility of inappropriate rejection of the null hypotheses of no effect of pollutants or no difference between the sexes.

References


Table 1 Distribution of the numbers of hospitalisations for asthma, 1992–99, among 94 children aged 6–12, in a cohort from the cities of Hamilton and Toronto, Ontario

<table>
<thead>
<tr>
<th>Number of admissions</th>
<th>Number of children</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>73</td>
</tr>
<tr>
<td>2</td>
<td>10</td>
</tr>
<tr>
<td>3</td>
<td>5</td>
</tr>
<tr>
<td>4</td>
<td>4</td>
</tr>
<tr>
<td>5</td>
<td>12</td>
</tr>
</tbody>
</table>

Correspondence to: Dr J Macleod, Division of Primary Care, Public and Occupational Health and The Medical School Birmingham, UK; G Davey Smith Department of Social Medicine, University of Bristol, Bristol, UK

J Macleod
Division of Primary Care, Public and Occupational Health and The Medical School Birmingham, UK

G Davey Smith
Department of Social Medicine, University of Bristol, Bristol, UK

Correspondence to: Dr J Macleod, Division of Primary Care, Public and Occupational Health and The Medical School Birmingham, B15 2TT, UK; j.a.macleod@bham.ac.uk

References


Authors’ reply

We appreciate Dr Finkelstein’s comments on our paper regarding sex differences in effects.

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of air pollution on asthma hospitalisation and the possible impact that autocorrelations in the data would have on our risk estimates. In our study, the effects of certain gaseous pollutants on asthma hospitalisation were found to differ between boys and girls 6 to 12 years of age. Some of these differences were “statistically significant”, for example, the regression coefficient for six to seven day SO$_2$ effect was significantly greater for girls than for boys ($p<0.001$). Although individual results can be examined for statistical significance in this manner, we prefer to base our study, the effects of certain gaseous pollutants on asthma hospitalisation in children: a bi-directional case-crossover analysis. J Epidemiol Community Health 2003;57:50–5.

Smoking cessation services may not reduce inequalities

Given the oft quoted proposition that health promotion interventions frequently increase, rather than decrease, socioeconomic inequalities in health (SEIH), we were interested to read the article by Lowey et al. However, we believe that the title of this article is misleading, that the authors have not analysed the data to its fullest potential, and that a number of the conclusions drawn are unjustified.

The authors do not present any data on the magnitude of inequalities in smoking in the areas studied either before or after the introduction of smoking cessation services. Without such data, conclusions relating to the impact of the intervention on inequalities in smoking rates by deprivation quintile cannot be drawn. As the authors note, the lack of data on overall smoking rates in the different deprivation quintiles means that one cannot immediately determine the impact of the intervention on smoking rates and hence inequalities. However, it is possible to carry out sensitivity analyses based on estimated smoking rates to investigate the potential impact of the intervention on inequalities in smoking.

Table 1 shows the number of male smokers before and after the intervention, using data published in the paper and hypothetical pre-intervention smoking rates representing people living in the most and least deprived quintiles using hypothetical smoking rates.

### Table 1

<table>
<thead>
<tr>
<th>Deprivation quintile</th>
<th>Pre-intervention</th>
<th>Post-intervention</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Smoking rate, %</td>
<td>Number of smokers</td>
</tr>
<tr>
<td>Least deprived</td>
<td>187064</td>
<td>25</td>
</tr>
<tr>
<td>Most deprived</td>
<td>336800</td>
<td>25</td>
</tr>
<tr>
<td>Least deprived</td>
<td>187064</td>
<td>5</td>
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<tr>
<td>Most deprived</td>
<td>336800</td>
<td>50</td>
</tr>
<tr>
<td>Least deprived</td>
<td>187064</td>
<td>15</td>
</tr>
<tr>
<td>Most deprived</td>
<td>336800</td>
<td>39</td>
</tr>
</tbody>
</table>

*Data from Lowey et al.; †sensitivity estimates including data from Lowey et al.; ‡data calculated by us; †relative risk of smoking in most deprived compared with least deprived quintile.

**References**


**Authors’ reply**

Adams and White fall shy of refuting that smoking cessation services are reducing inequalities. However, using our data and...
speculative levels of smoking, they examine how smoking cessation services might have changed the relative risk of smoking between the least and most derived areas. Like Adams and White we are aware of data from the national household survey on prevalence of smoking according to deprivation. Furthermore, since then a smoking prevalence survey has been published for two of the primary care trusts (one relatively affluent, Bebington and West Wirral and one relatively deprived, Birkenhead and Wallasey) within our study area. Prevalence of smoking for these was measured at 13% and 25% respectively. Using these figures and those from the General Household Survey (15% smokers in least deprived areas and 39% in the most deprived) we have tested whether greater proportions of smokers are quitting from deprived areas. Data from our study showed that the proportions of males quitting from deprived areas are significantly higher (table 1). In our original paper we chose not to publish any such analyses as the actual prevalence of smoking across our study areas was not known. Instead, in our comments we acknowledged the urgency with which such data are needed.1 We would not dispute that currently changes in smoking prevalence resulting from smoking cessation services are relatively modest. However, this is to be expected as only a fraction of all smokers are currently accommodated by such services. Furthermore, disproportionate effects on areas of high deprivation are also reduced by a greater drop out rates of people recruited from those most deprived areas. Again this is an issue we have suggested is tackled as a matter of urgency.

In reality, smoking cessation services can never dramatically affect relative risks of smoking (between least and most deprived areas) while the number of people they see represent such small proportions of the smoking population. However, we have shown that modest investment in such services has had an impact disproportionately on more deprived populations. Consequently, greater investment in smoking cessation services (as part of a suite of interventions to reduce smoking) may even deliver the changes in relative risk sought by Adams and White.

H L Lowey, B Fullard, M Bellis, K Tocque
Centre for Public Health, Liverpool John Moores University, Liverpool, UK
Correspondence to: Dr H L Lowey, Centre for Public Health, Liverpool John Moores University, 70 Great Crosshall Street, Liverpool L3 2AB, UK; helen.lowey@bwwpct.nhs.uk

References