Letters to the Editor

Letters are welcomed and will be published, if found suitable, as space permits. The editors reserve the right to edit and abridge letters, to publish replies, and to solicit responses from authors and others. Letters should be submitted in duplicate, double-spaced (including references), and generally should not exceed 400 words.

Test for Assessing Tar/Nicotine Yields

The poor relationship between published cigarette yields of tar and nicotine and human intake is now well established, and reflects the fact that humans and machines puff differently on cigarettes. Individual smokers are therefore poorly placed to make inferences about actual yields obtained on the basis of published deliveries. Kozlowski, et al., have reported a method of rating color-staining of butts which is intended to provide individual smokers with a guide to mouth level delivery of tar and nicotine from their cigarettes. A study is described in which judges' color ratings correlated very well with the number of puffs taken. However, the conditions of the study were artificial and unrepresentative of human smoking, the cigarette being "smoked" by a syringe which took between 5 and 16 puffs, each of 35 ml. Different puffing intensities and durations, plus variation in the distribution of puffs over the length of the cigarette, would be expected to change the rate of tar deposition in the filter (and hence its color).

We carried out an experiment to assess whether judgments of filter color stains of cigarettes smoked by human smokers would related closely to butt nicotine content and the volume of smoke puffed from the cigarette, which are both indices of mouth level intake. Thirty-one subjects (11 male and 20 female) each smoked a filter cigarette with machine yields of 17.5 mg tar, 0.8 mg nicotine, and 20 mg CO. The cigarette was connected to a puff analyzer to enable the total volume of smoke puffed from the cigarette to be determined. The cigarette butts were rated by four observers as described by Kozlowski, et al., and then analyzed for nicotine content.

The combined ratings correlated well with butt nicotine (0.88). Individual judges' ratings fared slightly less well (mean r = .76) and were quite variable (range r = .64 to r = .85). The combined ratings also gave a good indication as to the volume of smoke puffed from the cigarette (r = .86) while individual ratings correlated on average .72 (range r = .66 to r = .82). Butt nicotine and total volume puffed correlated .90.

The results provide support for the potential applicability of the color matching technique as a means of estimating mouth level intake from a cigarette. The correlations of the combined color ratings with butt nicotine levels and volume of smoke puffed from the cigarette were good and, although individual raters fared less well, it should be possible for smokers using the scale to gain a broad idea as to how they are smoking their cigarette.

The ability of the color matching technique to provide a guide to health risks depends on the closeness of the relationship between mouth level delivery and inhale smoke dose. In a recent study, the total volume of smoke puffed correlated only moderately with plasma nicotine level (r = 0.49) (4) which depends on both puffing and inhalation. Further evidence is needed on the relation between mouth level delivery and inhaled smoke dose before the benefits of using the color matching scale can be fully determined.

REFERENCES


Kozlowski, Rickert Reply

We are pleased to see this interesting replication of our findings. No doubt some of the differences among the correlation coefficients are due to sampling variability (rs of .64 and .85 just missing being significantly different at .05 level).

Of course, yields from cigarettes are not the same as yields to a smoker's bloodstream. Based on Sutton, et al., the authors suggest that mouth level delivery and inhaled dose will correlate only moderately. While we might dispute the value of single sample plasma nicotine measures as indicators of inhaled dose and might doubt the comparability of a study design that employs several very different brands, we would rather emphasize that, in the land of the almost blind standard tar and nicotine assay, a one-eyed color matching technique would be king.

REFERENCES


Lynn T. Kozlowski, PhD
Addiction Research Foundation, Toronto, Ontario, Canada M5S 2S1
William S. Rickert, PhD
Labstat Incorporated, Kitchener, Ontario

Modified EPA Algorithm Needed

Re: Asbestos in Schools

Findley, et al., conclude that the EPA's Asbestos Hazard Evaluation Algorithm is too unreliable a measure of the potential for hazardous exposures from spray on asbestos in schools and too non-specific a guide to corrective action.

In 1981, the EPA Algorithm was scrapped for a simpler "Guidance System . . ." which has since been strangled by Congressional failure to fund the Asbestos Hazard Detection and Control Act of 1980. At present,
schools are federally required only to identify friable asbestos containing materials and notify those affected. Federal regulations, in force, neither require corrective action nor provide decision guidelines. Because I believe such guidelines are necessary and because they will presumably reappear and be evaluated, I wish to comment on the authors' analysis of the original EPA Algorithm.

There are 90,000 public schools in the United States. Of the two-thirds constructed from 1946—1973 when spray-on asbestos enjoyed wide use, a relatively small number (6—12 percent) actually contain spray-on asbestos. An even smaller number (3—4 percent) contain friable spray-on asbestos in areas of student exposure to an extent sufficient to warrant corrective action under the EPA Algorithm. The authors' analysis underutilizes this information.

Studying only sites which in fact contain spray-on asbestos has led the authors to unduly deflate the Algorithm's specificity. Including 41 study sites free of this material would increase the specificity from the 53 percent reported to 96 percent. If, as some might argue, the Algorithm is only to be used at schools proven to contain spray-on asbestos, the specificity of 53 percent would be offset by the then 30—40 percent prevalence of schools requiring corrective action. Either broadening the Algorithm's study spectrum or limiting its scope of practical application would lead the Algorithm to be right half the time it opted for corrective action.

If we could rely on the 50 per cent positive predictive value, and the 98—99 per cent negative predictive value suggested by the authors' data, 90 percent of the schools would be quickly exonerated. The remaining 10 percent would only require a second, more specific test. Unfortunately, air sampling cannot be considered an appropriate second test for school settings.

The Algorithm's reported sensitivity of 98 per cent poorly reflects the characteristically marked underreporting by school administrators. I would recommend that industrial hygienists—not school administrators—inspect all schools constructed before 1973, characterize all spray-on as well as non-spray-on asbestos exposure sources, and base corrective action needs on a yet to be agreed upon algorithm. Vermont has shown that the implementation of such recommenda-

tions need not present an overwhelming burden.

David Kern, MD, MOH
Internal Medicine, Occupational Medicine, 13 Orkney Road, Brookline, Massachusetts 02146

REFERENCES

Response from Findley, et al.

Dr. Kern's assertion that we have in a sense deflated the Algorithm's specificity is mathematically correct, but is not consistent with the EPA Algorithm we evaluated. The process starts by first determining through laboratory tests if any friable material contains asbestos. Thus, specificity should be evaluated in the context of the presence of asbestos.

Even using Dr. Kern's estimate of 96 percent specificity would still yield a 4 per cent false positive rate. The predictive value (PV) of the algorithm for action (to encapsulate or remove) was judged to be 75 per cent (127/167). This would imply that 3 out of 4 decisions to take any action actually needed some action as judged by the experts. The PV for a decision to remove was only 61 per cent (67/110). Finally, the implications of the predictive value must not be ignored in the context of only those schools "proven to contain spray-on asbestos." While it is easy to argue the obvious—removal of the risk entirely is the only optimal solution—the economics mitigate against it. Therefore we find ourselves faced with serious risks and a proposed risk assessment protocol which when applied as in our test population yielded agreement with our panel of experts of 58 per cent, 16 per cent, 67 per cent, 49 per cent and 88 per cent for the five sites we surveyed. The poor predictive value and wide variability from the "suggested" action leaves great potential for unnecessary concern on the part of the community, expenditures, and unnecessary risks of legal actions. Finally, in the two situations where removal was recommended by the experts, 12 per cent and 33 per cent of the lay individuals recommended less stringent action.

In our paper we computed sensitivity as action, either encapsulation or removal. We did not make a major point of differences in classification, since it is our belief that either action would lead to more professional advice presumably leading to the correct action. An algorithm which has so much variability has tremendous potential to confuse and incite a community while adding little over common sense and suggestions to obtain professional advice. The use of "EPA Algorithm" may also convey a false sense of security ("the Algorithm's exquisite sensitivity") while leaving potential risks untouched. However, in conclusion we do agree with Dr. Kern's suggestion regarding industrial hygienists and use of a modified algorithm.

Mike Findley
University of Alabama, School of Public Health, Department of Epidemiology, University Station, Birmingham, AL 35294

On Variability in Children's BP Measurements

I have read with interest the article by Osborne, Mullin, and Roberson, The Variability of Blood Pressure Measurements in Children, in a recent issue of the Journal (Am J Public Health 1983; 73:1207—1210). There are two important causes of variability unaccounted for in this article on sources of variability.

First, the authors state, "Observers were blind to each others' measurements and to previous measurements."