Smoking and Pregnancy

SIR,—Your invitation for Dr Yerushalmy to respond to your editorial¹ to the correspondence on smoking during pregnancy arrived a few days after his death. Dr Yerushalmy would have enjoyed contributing to the discussions which centred around the two propositions that he described²: that cigarette smoking acts as an exogenous factor which interferes with the intra-uterine growth and development of the foetus and that smokers may represent a group of people whose reproductive experience would have duplicated the observed pattern whether or not they smoked.

His study on 'Infants with Low Birth Weight Born Before Their Mothers Started to Smoke'³ seems to support the hypothesis that the higher incidence of low-birth-weight infants is due to the smoker, not the smoking. Dr Yerushalmy would probably have made a strong plea, as he did in his paper, to repeat this study on larger samples in other population groups to get a definite verdict on the alternative hypothesis. This could be done by asking large numbers of pregnant women who smoke when they started to smoke, what was the birth weight and the health data of previously born children.

Yours faithfully,

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¹Nature, **245**, 61 (1973).

²Yerushalmy, J., *Proc. 6th Berkeley Symp. Math. Statistics Probability*, **IV**, 329 (1972).

³Yerushalmy, J., *Am. J. Obstet. Gynec.*, **112**, 2 (1972).

Smoking, Pregnancy and Publicity

SIR,—Professor Burch¹ and Dr Hickey *et al.*² rely heavily on the publications of the late Professor Yerushalmy to criticise my own remarks³. As there still seems to be some confusion about the value of his statistical evidence, I would like to elaborate a little on the points I made in my earlier letter.

First, estimates of the mean difference in birthweight between babies of smokers and non-smokers do, of course, differ, but are all in the 150 to 250 g range, and the exact value for a particular population is not relevant to the argument. Yerushalmy's⁴ test for a shift of 200 g in the mean of the birthweight distributions of babies of smokers is inappropriate.

What he should have tested for was an equivalence of the distributions apart from a shift in mean value, which is estimated from the data and need not be

exactly 200 g. As Dr James⁵ has shown, however, we will still find a reversal in mortality rates in the babies weighing 2,500 g or less, if a given percentage reduction in birthweight due to smoking is assumed, rather than a constant absolute reduction. Moreover, a simple study of the two birthweight distributions, in Yerushalmy's own study for example⁶, reveals quite clearly how an increased mortality among babies of smokers which is associated with a reduction in birthweight will lead to a lower mortality among babies of smokers if only the babies weighing 2,500 g or less are studied. For this reason all of Yerushalmy's analyses of babies weighing 2,500 g or less are irrelevant to the main issue.

Professor Burch quotes Yerushalmy's claim that there is no association between birthweight and age for mothers less than twenty-five years old. There are, however, studies other than Professor Yerushalmy's and with much larger numbers^{7,8} which do show that the proportion of low birthweight babies born to women under twenty is about 20% higher than that for women aged between twenty and twenty-four. Until due allowance is made for this, Yerushalmy's conclusions should not be accepted at their face value.

As Professor Burch observes, I am quite happy to "concede" that the scientific case for a causal relationship is not yet completely conclusive and among other possibilities we need to investigate the 'constitutional' hypothesis. The point at issue, however, is how the available evidence should be assessed, and what publicity should be given to any conclusions.

An impartial observer, it seems to me, would have little doubt that this evidence justifies efforts aimed at persuading pregnant women to stop smoking. Your correspondents, I suggest, have confused this practical, ethical issue with the other problem of scientific proof.

Yours faithfully,

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¹ Burch, P. R. J., *Nature*, **246**, 177 (1973).

² Hickey, R. J., Boyce, D. E., and Clelland, R. C., *Nature*, **246**, 177 (1973).

³ Goldstein, H., *Nature*, **245**, 467 (1973).

⁴ Yerushalmy, J., *Am. J. Obstet. Gynec.*, **114**, 571 (1972).

⁵ James, W. H., *Nature*, **246**, 235 (1973).

⁶ Yerushalmy, J., *Am. J. Epidemiol.*, **93**, 443 (1971).

⁷ Butler, N. R., and Alberman, E. D., *Perinatal Problems* (Livingstone, Edinburgh, 1969).

⁸ Chase, H. C., and Byrnes, M. E., *Vital and Health Statistics Series 3*, 15 (National Centre for Health Studies, Rockville, Maryland, 1972).

This correspondence is now closed.—ED.

Literature Citations

SIR,—The transcription of literature citations in scientific articles is rendered unnecessarily tedious by the diversity of arbitrary conventions adopted by different journals. References jotted down for subsequent bibliographical use must be differently transcribed according to the journal in which they are to appear. The differences are matters of form and style as well as of completeness.

We are writing to urge that commissions of editors convene information retrieval specialists, typographers, and some common readers to draw up broadly acceptable conventions that optimise clarity and simplicity in literature citations.

Some specific suggestions are: (1) Each scientific journal should adopt an abbreviation, if necessary, of its original or transliterated title that can be used without confusion anywhere in the scientific literature. The abbreviations of common words such as 'Annual' and 'Comptes Rendues' should be uniform. Local conventions such as the germanic use of lower case for adjectives or special self-reference terms ("This Journal") are to be avoided. (2) The presentation of author's names, initials, publication year, article title and key-words, volume number and pagination should be standardised as to sequence, punctuation and type font. Simplifications should be accomplished by deletions without rearrangement. (3) The title page of articles should bear the complete article reference as it should be cited. Fascicle numbers, issue dates and so on, not normally cited, should appear separately on the page if they serve a function for particular users.

The temporary inconvenience to journal staff in changing habits would be offset, in the long run, by diminished editorial labour in the revision of improperly submitted bibliographies.

Uniformity in these matters would cause little reduction in the marvellous particularity of individual journals and would reduce the burden on authors, readers, and especially secretaries.

Yours faithfully,

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Bomb Disposal

SIR,—Disarming and removing explosive devices is dangerous to police and army personnel, and also to the general public. I would like to propose a

method of deactivating bombs which would considerably reduce this risk.

Liquid nitrogen is relatively safe and easily handled. A bomb sprayed with it for several minutes would be cooled to an extremely low temperature. This would have two effects. First, the battery used to detonate the explosive would become ineffective. I have verified this in the following way. Three brands of dry cell battery (Ever Ready, Winfield, Ray-O-Vac) were cooled to the temperature of liquid nitrogen and tested in a small torch. None of the batteries tested caused the torch to light. When the batteries were allowed to warm up, however, complete efficiency was restored.

Second, the detonator and explosive might reasonably be expected to be unreactive at such low temperatures. A bomb deactivated in this manner could be safely transported to a disposal site in an insulated container partially filled with liquid nitrogen.

Yours faithfully,

GEORGE DAVIDSON

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Foraminifera

SIR,—The recent review of Dr J. W. Murray's book¹ by Dr J. R. Haynes

(*Nature*, **245**, 442; 1973) may have confused a number of palaeoecologists by seeming to advocate absolute abundance rather than standardised abundance for the plotting of species distributions. The latter method, now widely employed, is admittedly suspect because it makes the *a priori* assumption that all the foraminifera of an assemblage are competing.

This is not as unsatisfactory, however, as the absolute abundance method which is, of course, much affected by rates of sedimentation. As a result, plots of species using this method are often difficult to distinguish. Standardisation of data reduces this 'background noise' and furthermore enables the calculation of basic ratios (such as benthonic: planktonic or Textulariina: Miliolina: Rotaliina) which are now valuable aids to the palaeoecologist. Obviously, if the approach is biological rather than geological, then the biomass of a species per unit area is the better index.

Yours faithfully,

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¹ Murray, J. W., *Distribution and Ecology of Living Benthic Foraminiferids* (Heinemann, London, 1973).

As suggested in my review, paucity of data and lack of control of sample material may force us to express species abundances in percentages rather than absolute numbers per unit area of volume of substrate. The desperate inadequacy of this method, however, is shown by the tables in Chapter 1 of Dr Murray's book. Not only does this method impose reciprocal relationships but the percentage figures may show a rise for a particular species between stations when the absolute numbers have fallen and *vice versa*.

At present our understanding of the autecology of foraminifer species is almost nil. Progress towards an evolutionary ecology requires knowledge of the abundance of individual species both in their habitats and when dead as part of the sediment. The variations in numbers and sorting brought about by sedimentary processes which make plots "difficult to distinguish" are precisely what concern geologists and represent the challenge that has to be met before we can properly interpret the geological record. Even where it is thought valuable simply to estimate certain crude ratios these are as easily derived from absolute numbers.

JOHN R. HAYNES

Obituary

Leonard Carmichael

DR LEONARD CARMICHAEL, Secretary of the Smithsonian Institution from 1953–64, died on September 16 in Washington, DC, at the age of 74. He was the National Geographic Society's Vice-President for Research and Exploration at the time of his death.

A distinguished physiological psychologist, Dr Carmichael also achieved renown as a science administrator in the worlds of academia, museology and government. The central unifying interest in his career, which lasted 50 years, was research: his own research and the administration and funding of the research of others.

Dr Carmichael was born on November 9, 1898, in Philadelphia, where his father was a successful physician. In 1917 he entered Tufts University, where his grandfather, Charles Hall Leonard, had for many years been Dean of the Crane Theological School. Here he became interested in zoology and psychology, and later recorded that the

two men whose books had the most influence on him as an undergraduate were the biological ultramechanist Jacques Loeb and the proponent of emergent evolution C. Lloyd Morgan.

Following graduation from Tufts, *summa cum laude*, he began graduate work at Harvard in psychology, developing an interest in a basic biological approach to the science. He later wrote that the accidental finding of a German publication of William Preyer, *Spezielle Physiologie des Embryo, Untersuchungen über die Lebenserscheinungen vor der Geburt*, in the Harvard Library, was a turning point in his life, opening his eyes to a specific area of research in his chosen field of the sensory and neural control of behaviour on which he was to spend many years. "Here at last I saw a way to investigate the topic of major interest to me—the morphological growth of the receptors and nervous system in relation to changes in behaviour as responses are released at various stages during the early ontogenetic development in each mammal

before learning begins, or at least before it becomes important."

He completed his PhD studies and theses and did a year of graduate work at the University of Berlin before beginning his teaching career at Princeton in 1924. Later he taught at Brown, where he gained his first administrative experience as Director of its psychology laboratory, and at Rochester, where he was Dean of the arts and sciences faculty. At Brown, in addition to his teaching and administrative activities, Dr Carmichael began studying the prenatal development of behaviour in mammals. With Dr H. H. Jasper he developed the electroencephalograph and published in 1935 what is believed to be the first report of such work in America.

In 1938 Dr Carmichael returned to Tufts to become, at 39, one of the youngest presidents in the college's history, but during World War II he was summoned from the campus to fill various posts in Washington. As director of the National Roster of Scien-