prove most distressing to an already anxious, but innocent mother. Unnecessary skeletal radiography of the baby is to be deplored.

Even if the parents are exonerated, in the future the mother may still be too frightened to report the inevitable childhood injuries.—I am, etc.,

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Library Services in Hospitals

SIR,—The Department of Health and Social Security circular H.M. (70)23 under the above title purports to give official guidance on planning hospital libraries. We therefore based our proposals for a new multi-disciplinary staff library strictly on H.M. (70)23.

We are now told by officers of the Regional Hospital Board that the people in the Department who control building projects had never heard of H.M. (70)23. When they were provided with a copy, by the Regional Hospital Board, they rejected the advice of their colleagues elsewhere in the Department out of hand and cut the floor area of our proposed new library by half.

The lesson of this absurd episode must surely be to discard H.M. circulars without bothering to look at them. If they only represent the individual opinions of their writers, and not the collective policy of the Department, they are so much waste paper.—I am, etc.,

MARK HUGHES

Chairman, Library Committee

Postgraduate Medical Centre,
Royal Hampshire County Hospital,
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Treatment of Hypothyroidism

SIR,—Dr. P. B. S. Fowler's letter (11 August, p. 352) about the paper by our colleagues and ourselves (21 July, p. 131) raises several points which merit a reply.

After adequate treatment of hypothyroidism the serum cholesterol and triglyceride levels of our patients were very similar to those of control subjects. Though the mean value of serum triglycerides in the treated patients was higher than that in the controls as Dr. Fowler points out, the standard deviation quoted in our table makes it evident that this difference is not statistically significant.

The raised serum triglyceride level after treatment refers to the four patients whose levels rose after treatment but within the normal range, a finding as yet unexplained. The two statements quoted by Dr. Fowler are not mutually contradictory if the paper is read fully, but we apologize if our meaning was not clear.—We are, etc.,

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Cancer and the Patient

SIR,—This debate is important. May I make the following contributions?

(1) For most of my 30-odd years as a radiotherapist I could boast that I had

never told a patient that he or she had cancer—but many had told me their diagnosis. Times do change, public knowledge is increasing, and we are gradually moving nearer to the American situation. We know that the prognosis in some groups (skin, cervix, etc.) is often excellent, but the fact still remains that most cancers are still fatal, and the terminal stages in many cases—but by no means all—are very distressing.

(2) Last week a surgical colleague performed a mastectomy on an intelligent patient for an anaplastic growth. He prevaricated with the patient, who was tense and anxious and "knew too much," but told her husband the truth. The husband promptly passed it on to his wife. By the time I arrived the patient was composed and cheerful and willing to have any further treatment advised.

(3) My ward sisters tell me that patients told the whole truth tend to be the most "difficult." I have the greatest respect for, and pay great attention to, the opinions of experienced sisters.

(4) Suicide has been mentioned. I think most doctors know in their hearts that, on occasion, suicide can be sincerely rational and deeply charitable, but are naturally reluctant to say it aloud because of the obvious and admitted dangers of misuse.

(5) Since the correspondence began, I have asked the relatives in several hopeless cases whether they thought I should tell the patient the truth. In every instance they said "no;" they thought the patient suspected but would not like to be told outright. I am sure they were right.

(6) A final point which some may find surprising. It is not very rare for a patient or his family to be given the diagnosis and to insist later that they were never informed. Such is the (perhaps fortunate) capacity of the human mind to reject the unpleasant.

All this adds up to the not very original conclusion that there are no easy answers; we should move with the times but not force the pace. We must do the best for the individual patient as we see it—and that takes time.—I am, etc.,

J. WALTER

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Incomprehensible Explanations

SIR,—In order to inform the general public of the existence and function of the new Health Service Commissioner (or "Ombudsman") a leaflet¹ has been prepared by the commissioner's office and widely distributed. Several colleagues commented that they found the leaflet difficult to read and so it was decided to subject it to a measure of reading comprehensibility. Several methods are available to calculate the proportion or educational level of the population who might be expected fully to understand a given piece of writing.² Recently one such technique (that described by Flesch^{3,4}) was used by Ley *et al.*⁵ to measure the comprehensibility of various x-ray leaflets.

Using Flesch's formula a "reading ease score" is obtained which indicates that the commissioner's explanatory leaflet could be expected to be understood by 4.5% of the population and that its level of difficulty corresponds to that found in scientific journals. The score is derived from a consideration

of the average sentence length and the degree of polysyllabism. Whereas years of experience might well render members of the professions adept at pursuing a main verb through a maze of qualifying clauses, it is doubtless significant that the newspaper with the widest circulation are those with the simplest sentences.

Though reference has been made to the Health Service Commissioner's leaflet, similar remarks could have been made about many pamphlets which are believed to explain the nature of operations, investigations, or treatment procedures. It is, perhaps, particularly undesirable that a large proportion of patients might not fully understand a pamphlet that advises them how to lodge a complaint.—I am, etc.,

JOHN COUPER-SMARTT

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- ¹ *Health Service Commissioner for England, Functions and Scope*, London, H.S.C.E., 1973.
- ² Seels, B., and Dale, E., *Readability and Reading—an Annotated Bibliography*. New York, International Reading Association, 1966.
- ³ Flesch, R., *Journal of Applied Psychology*, 1948, 32, 221.
- ⁴ Ley, P., *Journal of the Institute of Health Education*, 1973, 11, 17.
- ⁵ Ley, P., Goldman, M., Bradshaw, P. W., Kinney, J. A., and Walker, C. M., *Journal of the Institute of Health Education*, 1972, 10, 47.

Spontaneous Abortion and Neural Tube Defects

SIR,—Dr. C. J. Roberts and Mrs. Setsuko Lloyd (6 October, p. 20) examined the spontaneous abortion rates among the previous sibs in cases of anencephaly and spina bifida (A.S.B.) and among the previous sibs of normal controls in an area of high A.S.B. rates (area A). They contrasted these two values with comparable estimates from sibships in an area of low A.S.B. rates (area B). They found that the rates of reported previous abortion in the high-risk area were lower than those in the low-risk area. To explain this, they offered the hypothesis that the overall A.S.B. rate at conception may be the same in the two areas, but that the survival of an A.S.B. fetus to term may be facilitated by some (presumably) environmental factor in the high-risk area.

Let us assume that the spontaneous abortion rates of non-A.S.B. fetuses in the two areas are equal. (I shall later suggest that this is a generous assumption.) Then if the authors' hypothesis is correct, the (total A.S.B. rate plus total spontaneous abortion rate) should be equal in the two areas. In fact, these two values are not equal but are about 13.96% in area A and 15.64% in area B. (Since the outcomes of the various pregnancies in a single sibship are not independent, I shall not cite a significant level for the difference between these values; but I shall assume that it is not due to chance.) What could cause this disparity? It seems there are two alternatives: either (1) the spontaneous abortion rate of non-A.S.B. fetuses in area B is higher than in area A, or (2) there is a reporting deficit of abortions in area A.

I shall treat these alternatives in order. (1) The mean social class level of area B is clearly higher than that of area A. Now women in the higher social classes are subject to lower stillbirth rates than women in the lower classes.^{1,2} One would suppose that

the same is true too of spontaneous abortion rates, and the supposition is borne out by the only authoritative study known to me on this topic.³ So the disparity (suggested above) in the data of Dr. Roberts and Mrs. Lloyd exists *in spite of*, not *because of* social class differences between the areas. (2) Dr. Roberts and Mrs. Lloyd show that if there is a deficit in reporting of abortions in area A it seems not to be dependent on social class. However, (a) their technique of controlling for social class is very coarse (they use only two social class categories, manual and non-manual) and as such could not be expected to eliminate class differences within the categories, and (b) this suspected reporting deficit may be dependent on factors other than social class.

Dr. Roberts and Mrs. Lloyd are committed to rejecting the alternative of this reporting deficit; they seem, therefore, to be saddled with the difficulty of explaining why non-A.S.B. spontaneous abortions (like A.S.B. spontaneous abortions) are less common in area A than area B. However, it is perhaps not appropriate to devote too much a priori argument to their hypothesis. After all, it is testable. I suggest that samples of spontaneously aborted fetuses in the two areas be examined. If the hypothesis is correct, there should be a higher proportion of A.S.B. fetuses in the low-risk area.—I am, etc.,

W. H. JAMES

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- 1 Chase, H. C., *International Comparison of Perinatal and Infant Mortality: the U.S. and Six West European Countries*. Public Health Service, National Center for Health Statistics, Publication No. 1000, Series 3, No. 6. Government Printing Office, Washington, D.C., 1967.
- 2 Spicer, C. C., and Lipworth, L., *Regional and Social Factors in Infant Mortality*. General Register Office Studies on Medical and Population Subjects No. 19. London, H.M.S.O., 1966.
- 3 Taylor, W. F., in *Proceedings of the Third International Conference on Congenital Malformations*, ed. F. C. Fraser, V. A. McKusick, and R. Robinson. New York, Excerpta Medica Foundation.

Oxygen Therapy

SIR,—In their article entitled "Oxygen Therapy in Chronic Respiratory Failure" (20 October, p. 154) Dr. R. Hume and others state that the M.C. mask "normally delivers about 60% oxygen." This bald statement, while ostensibly being covered by the words "normally" and "about," may give a false impression of accuracy to an uncritical reader who may subsequently have occasion to use the mask. The figure of 60% will then spring to mind unaccompanied by any conditions or reservations.

Devices of this kind have such a highly variable performance that it is important when referring to them to leave no doubt about the accuracy and variability of the resulting inspired oxygen concentration and the conditions affecting it. The only conditions which are easy to define are the flow rate of oxygen and the respiratory frequency. Unfortunately, these are usually less important than the tidal volume, the inspiratory flow rate, and the shape of the patient's face in relation to the mask. Moreover, because of the small volume of the mask there is, during the course of one breath, a highly variable inspired oxygen concentration. The mean PaO_2 thus produced is not

necessarily the same as would be produced by the inhalation of a constant mixture of the same concentration as the so-called "mean" from the M.C. mask.

These masks are not suitable for the administration of accurate concentrations of oxygen and it is submitted that in the context of this article it would have been better to talk only in terms of the PaO_2 that the device produced in a particular patient at a particular time than to quote unreliable figures for inspired oxygen concentrations.—I am, etc.,

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Treatment of Depression in General Practice

SIR,—The results and comments of Dr. D. A. W. Johnson in his paper on the treatment of depression in general practice (7 April, p. 18) seemed so different from our own experience that we were prompted to extract some figures of our own depressed patients.

Part of our practice is urban and based on Woodside Health Centre, Glasgow. We extracted information retrospectively from all patients diagnosed as depressed in our Woodside practice (3,400 patients) attending over a four-month period. This was easy to do as we use a system of morbidity and workload recording based on feature cards. The relevant results are as follows:

	Our results	Dr. Johnson's results
Total numbers	66	73
Years on list		
0-5	20%	50%
6-10	17%	25%
10+	62%	25%
Known to health visitor or social agency	20%	0%
Average times each patient seen in 18 weeks after presentation	4.7	2.3
Average times each patient consulted doctor in two years before diagnosis	16	not known

Our figures thus differ significantly from Dr. Johnson's. With the average depressed patient having been on our list for 17 years and having consulted us 16 times in the past two years we can fairly be said to be likely to have a knowledge of the patient and his background. We do not claim that our practice is a typical one, but neither had Dr. Johnson any reason to claim that the practices he selected were typical. Further, the depressed patients he selected were only those who had had no symptoms or psychotropic medication for the year before the present attack.

From a small group of selected patients studied under abnormal circumstances from selected practices, all apparently in one city in Britain, Dr. Johnson draws conclusions about all depressed patients managed in any city in Britain. This is an unjustified extension of his findings. Our own findings show that they are certainly not universally applicable.—We are, etc.,

KENNETH HARDEN
W. THORBURN
R. T. W. PRENTICE
DIANA KIERNAN

Bearsden, Glasgow

Scotland's Drink Problem

SIR,—Licensing laws are said by both the Erroll Committee in England¹ and the Clayton Committee in Scotland,² referred to in your leading article (13 October, p. 64) to play a minor part only in the control of alcohol misuse. Both rightly stress the great importance of education and research—though one wonders why such research into the gaps of knowledge should not be undertaken before any of the proposed changes in legislation. At any rate, it has certainly *not* been proved that restrictive legislation—for example, the limitation of opening hours—is of no avail. In fact, the change in England from a situation of widespread drunkenness to relative sobriety took place fairly suddenly during the 1914-18 war³—in the views of many observers mainly as a consequence of the recommendations of the Liquor Control Board.^{4,6} In the view of G. B. Wilson³ the remarkable improvement was brought about by direct restrictions on the supply of liquor, by the administrative restriction on hours of sale, by dilution of beers and spirits, etc.; and he felt that "particularly since 1913 . . . later hours of opening, the midday break, and earlier closing hours have been very important factors . . . in checking a rise in the aggregate consumption . . . and possibly in checking individual consumption."⁵ Similarly, Shadwell felt that among state control measures "shorter hours and higher taxation . . . have proved really efficacious."⁶ Most of the recent work in this field has been carried out by the Addiction Research Foundation in Ontario; they regard the price of alcohol in relation to disposable income as the most important factor in influencing alcohol consumption, and they feel that the area of control of days and hours of sale deserves further research.⁷

Clearly, as Erroll points out, other factors (for example, counterattractions, increasing affluence, etc.) contributed to the decrease of the drink problem in the first half of the twentieth century.³ Such factors, however, also affected other nations, none of which (as far as I am aware) showed the remarkable sudden drop in drunkenness. Rather than for this country to follow the "civilized" drinking of Continental countries one wonders whether it might not be a better idea for such countries—most of them with far larger alcohol problems (as yet?) than Britain—to study the conditions and methods used in this country. It is well known, for example, how far the alcoholism problem in France with all its "civilized" drinking habits outstrips the British problem.

To what extent the changes recommended by Erroll and Clayton will increase alcohol consumption or otherwise is of course not known. They certainly will increase availability, and health-related alcohol and drug problems may stem not only from individual ("host") susceptibility but also from availability of the "agent" (alcohol or other drugs).⁸ Recent research in Ontario has confirmed earlier findings⁹ of a relationship between a nation's per capita alcohol consumption and the prevalence of alcohol-related damage and complications.^{8,10} In his recent talk to the 1973 conference of the Medical Council on Alcoholism, De Lint¹⁰ pointed to the evidence that the current world-wide trends in excessive alcohol use and alcohol-related mortality followed a growing acceptance of alcohol use in every-