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

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.,

J. R. IVEY

Radiotherapy Department, Singleton Hospital, Sketty, Swansea

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 we 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
Oxnam, Library Committee

Postgraduate Medical Centre, Royal Hampshire County Hospital, Winchester

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 in fact mutually contradictory if the paper is read fully, but we apologize if our meaning was not clear.—We are, etc.,

DAVID EVERED
R. HALL

Department of Medicine, Royal Victoria Infirmary, Newcastle upon Tyne

Cancer and the Patient

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

(1) I should like to report that with 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.

(2) It is true that 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 better than the fact still remains that most cancers are still curable and the terminal stages in many cases—but by no means all—are very distressing.

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J. R. IVEY

Radiotherapy Department, Singleton Hospital, Sketty, Swansea

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 of aneuploidy 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 A.S.B. rates 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 and 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 discrepancy? 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 defect 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 the higher social classes are subject to lower stillbirth rates than women in the lower classes.1 One would suppose that

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(1) The mean social class level of area B is clearly higher than that of area A. Now the higher social classes are subject to lower stillbirth rates than women in the lower classes.1 One would suppose that
the same is true too of spontaneous abortion rates, and the suggestion is borne out by the only authoritative study known to me on this topic.\(^5\) 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

Department of Human Genetics and Biometry, University College, London N.W.1

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**Oxygen Therapy**

**Sir,—**In their article entitled "Oxygen Therapy in Chronic Respiratory Failure" (20 October, p. 144) Dr. Fowler and Dr. Chalmers 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 the unscrupulous 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 varied 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.,

A. M. HEWLETT
Brompton Hospital, London S.W.3

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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 from all our 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:

<table>
<thead>
<tr>
<th>Total numbers</th>
<th>Our results</th>
<th>Dr. Johnson's results</th>
</tr>
</thead>
<tbody>
<tr>
<td>Years on list</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0-9</td>
<td>66</td>
<td>73</td>
</tr>
<tr>
<td>10-19</td>
<td>20%</td>
<td>50%</td>
</tr>
<tr>
<td>20+</td>
<td>17%</td>
<td>25%</td>
</tr>
<tr>
<td>Average times each patient seen in 18 weeks after presentation</td>
<td>4.7</td>
<td>2.3</td>
</tr>
<tr>
<td>Average times each patient consulted doctor in two years before diagnosis</td>
<td>16</td>
<td>not known</td>
</tr>
</tbody>
</table>

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 psychotherapeutic 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 KIERMAN

Beardsden, Glasgow

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**Scotland's Drink Problem**

**Sir,—**Licensing laws are said by both the Erroll Committee in England\(^1\) and the Claytonson Committee in Scotland,\(^2\) referred to in your leading article (13 October, p. 64) to be partly or partly only the result of alcohol misuse. Both rightly stress the great importance of education and research—though one wonders why such research into taxation of knowledge should really be postponed until after 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 war—'in the views of many observers mainly as a consequence of recommendations of the Liquor Control Board.'\(^3\) In the view of G. B. Wilson\(^4\) the remarkable improvement was brought about by direct restrictions on the supply of liquor, by the administrative restriction on hours of sale, and by the complete ban of sales to wife of the soldier. The latter he felt 'particularly since 1913... later hours of opening, the midnight break, and earlier closing hours have been very ineffective factors.' In checking a rise in the aggregate consumption of alcohol a move in increasing individual consumption.\(^5\)

Similarly, Shadwell felt that among state control measures "shorter hours and higher prices" would prove of limited value. 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 influencing alcohol consumption, and they feel that the area of control of days and hours of sale deserves further research.\(^6\)

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.\(^7\) Such factors, however, also affected other nations, none of which (as far as I am aware) showed the sudden drop in drunkenness. Rather than for this country to follow the "civilized" drinking of Continental countries one wonders whether it is not the 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 Clasen will increase alcohol consumption remains to be seen. In the present climate any new device, providing it is not known. They certainly will increase availability, and health-related alcohol and drug problems may stem not only from individual (rather) susceptibility but also from availability of the "aient" (alcohol or other drugs).\(^8\) Recent research in Ontario has confirmed earlier findings of a relationship between a nation's per capita alcohol consumption and the prevalence of health-related damage and complications.\(^9\) In his recent talk to the 1973 conference of the Medical Council on Alcoholism, De Lint pointed to the evidence that the current trends in expenditure on the treatment of alcohol-related morbidity followed a growing acceptance of alcohol use in every...