others would certainly point to a myocardial element, although we would also question whether left ventricular dilatation and pulmonary oedema might, in addition to acute cardiac failure, reflect fluid overload.⁵ The pulmonary wedge pressures were not stated.

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- 1 Weston MJ, Talbot IC, Howorth PJN, *et al.* Frequency of arrhythmias and other cardiac abnormalities in fulminant hepatic failure. *Br Heart J* 1976;38:1179-88.
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AUTHORS' REPLY.—Dr Fagan and her colleagues agree with the basic issue that toxic myocarditis is seen in some cases of paracetamol poisoning. They also acknowledge that left ventricular dilatation and pulmonary oedema seen at necropsy reflect acute cardiac failure. We would like to assure them that our patient received the standard treatment for cerebral oedema, including intravenous mannitol and hyperventilation, but without success. Fluid intake exceeded output by less than 1 litre in 21 hours, and, allowing for invisible water loss and a mild degree of dehydration, the total fluid intake was appropriate. Fluid overload was therefore unlikely.

It is true that some of the features they refer to can be attributed to uncontrolled cerebral oedema, but cerebral oedema can itself be a consequence of hypoxia complicating arrhythmia and hypotension. Furthermore, they use the term "brain stem coning" rather loosely and suggest that the mere presence of cerebral oedema without tonsillar compression is equivalent to brain stem coning. This statement is a departure from accepted teaching in pathology. It raises another important question relating to the grade of cerebral oedema which they consider equivalent to brain stem coning. Such a flexible interpretation lacks precision and must therefore be rejected.

Nevertheless, some deaths from paracetamol poisoning occur without evidence of hepatic necrosis at necropsy. In a study cited by Meredith *et al* 61 out of 65 patients who died outside hospital showed no hepatic necrosis at necropsy.¹ Such an alarmingly high figure should not be attributed to factors like the concomitant intake of drugs or alcohol without considering other possibilities including that of paracetamol induced arrhythmia.² In a report on the failure of naloxone to reverse the cardiotoxic effect of dextropropoxyphene, a component of Distalgic, the authors recorded bizarre complexes of hypotension.³ Their patient later arrested and was successfully resuscitated. Unfortunately, they failed to recognise that paracetamol might have been the cause. An interesting feature which this patient shared with ours was the poor clearance of paracetamol from the plasma. We believe there is enough evidence in the case we reported to suggest myocardial toxicity from paracetamol.

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Charging patients for eye tests

SIR.—While there is much in the government's white paper *Promoting Better Health* which could improve the provision of primary health care there are potentially serious implications in the introduction of charges to patients for eye tests.

Diabetes mellitus is the leading cause of blindness in people of working age in the United Kingdom.¹ Treatment with photocoagulation of both proliferative retinopathy and maculopathy can prevent some 60% of blindness,^{2,3} but the changes must be detected early. Regular funduscopy is performed on patients attending diabetic clinics, but less than half of all known diabetic patients attend clinics, the remainder obtaining all supplies from their general practitioners.^{4,5} These patients are much less likely to have regular funduscopy.⁴ Moreover, on a general practitioner's list of 2000 patients there will be 20 diabetics, so that even if that general practitioner performs annual dilated funduscopy on all non-clinic attenders this will comprise only 10 examinations a year. As the prevalence of retinopathy is around 30% and of sight threatening retinopathy 10%⁷ the general practitioner will be extremely unlikely to obtain the experience necessary to provide an effective role in screening.

Two studies have shown that optometrists detect serious retinopathy with high sensitivity.^{8,9} These results and the anxiety expressed by general practitioners about their ability to detect retinopathy have encouraged many to work with local optometrists in providing comprehensive screening for their diabetic patients.

In this district we are piloting a study, funded by the British Diabetic Association, to evaluate the benefits of a computerised register of diabetic patients.¹⁰ The computer will be used to prompt general practitioners and patients about regular examinations and blood tests. Because of the anxieties of the participating general practitioners over funduscopy we have agreed with local optometrists that they will screen for retinopathy by funduscopy after the instillation of mydriatic drops.

In a district with a population of 200 000 half of the 2000 known diabetics would not be attending a hospital. The incidence of sight threatening retinopathy is 1.2% a year,⁷ which corresponds to 12 cases a year in these non-hospital attenders, and photocoagulation could prevent blindness in seven of these patients. Using Foulds's figure of £3500 as the annual cost to the state of supporting a blind person⁷ it is possible to perform a simple cost-benefit analysis of retinal screening by optometrists under the present system of reimbursing £9.30 for the eye test: costs of screening 1000 diabetic patients—£9300; benefits of preventing seven cases of blindness—£24 500.

At a time when an effective treatment has been introduced for treating retinopathy and when the DHSS itself is looking at ways of screening effectively for diabetic retinopathy,¹¹ no major hurdle should be introduced to the screening role of the optometrist. The introduction of an exemption category so that diabetic patients would continue to obtain free eye tests would help to answer this problem, but other difficulties would remain. Some 8% of diabetic patients have retinopathy at diagnosis,¹² and changes in refraction produced by high blood glucose concentrations are responsible for many patients with undiagnosed

diabetes attending an optometrist. The fact that optometrists are often responsible for diagnosis in these patients means that they have an important role in screening for disease as well as detecting complications. It is therefore important that the free eye test should remain.

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- 1 Sorsby A. *The incidence and causes of blindness in England and Wales 1963-1968*. London: HMSO, 1972. (DHSS Reports on Public Health and Medical Subjects No 128.)
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Graduated compression and its relation to venous refilling time

SIR.—The disbelief of Professor A N Nicolaides and his colleagues at St Mary's Hospital (5 December, p 1484), that our "careful studies" (31 October, p 1087) did not agree with their own long term clinical experience requires objective evidence.

We are entirely satisfied that the techniques we used were reproducible, and all the investigations were performed by a single person. There would appear to be a basic scientific flaw in the argument they have put forward, since whatever failing they may claim in the use of photoplethysmography, this flaw must then be equally distributed among all the studies, and, as the technician concerned had no preconceived knowledge of the possible results and was thus effectively blinded, we do not see how these factors could have biased the results.

We have, however, considerably more data than those published in this paper and have confirmed the work by using photoplethysmography with no defect being necessary in the garment, and also with a small group of patients studied by volume refilling using a strain gauge plethysmograph. Our results have been confirmed by the group working at St George's Hospital, who also extended their data to include use of venous pressures.¹ It is most unfortunate that the extensive studies quoted from St Mary's do not seem to have been published.

We appreciate the potential limitations of our classification and can confirm that all patients included within the deep venous incompetence group had phlebographic evidence of incompetence at popliteal level. One might, however, speculate on the need to attempt a pure separation in the face of equivalent defects in venous function.

The clear statistical significance of the results, albeit in small numbers of patients, to us would confirm the clinical importance of these results.

Careful inspection of the mean values published in the paper show that it is feasible to take a series of garments from within a range and find that they perform in a satisfactory manner—that is, they provide linear graduation and are effective in terms of the particular aspect of venous function which we studied. Our critics, however, have failed to grasp the importance of the work if they do not appreciate that it is important that these criteria should extend throughout the range of garments and be applicable to any of the size ranges supplied by the manufacturer. Further study of the materials they quote² shows that it was necessary for a range of stockings to be manufactured specifically for this study so that graduated compression could be achieved across the patient group.

We agree that further work is required, and want to make clear that our study was carried out independently of any manufacturer support; it was undertaken in the interest of applied physiology and the desire to improve the quality of care available to this often neglected group of patients based on objective data that are applicable to the individual subject.

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Leg ulcers

SIR,—Dr P K Buxton (11 December, p 1542) perpetuates a myth about venous ulcer which helps to deny curative treatment for up to a quarter of patients—namely, that isolated saphenofemoral incompetence (primary long saphenous in his terminology) with normal deep veins is a rare cause. This is not the case. We found 20 patients (with 23 ulcers) in a series of 80 patients who had “venous” ulcers in this class.¹ Sethia and Dark identified 17 ulcerated legs with primary varicosities out of a total of 60 studied.² Though they were able to show that such limbs often had deep to superficial incompetence at other sites, the results of treatment showed that it was the saphenofemoral incompetence that mattered.

Because of these two studies with their very similar results we believe that up to a quarter of patients attending for treatment of a leg ulcer have the possibility of permanent cure by saphenofemoral ligation and that all patients with ulcers should have a proper evaluation by Doppler studies and if possible photoplethysmography. Varicose ulcers are still far too much Cinderellas of the surgical world at considerable financial cost to the health service.

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Comparison of two measures of waiting times

SIR,—In the third of their series of articles on waiting times Dr B Don and colleagues (14 November, p 1247) point out that the proportion

of patients on a waiting list who, at some census date, have waited for more than one year will be greater than the proportion who actually do wait for longer than one year. This is a well known statistical effect caused by so called “length biased sampling” in which the probability of a sample unit (individual) being included in the sample or study is proportional to the magnitude (duration) of its value (the waiting time).

This effect crops up repeatedly in health service studies, as, for instance, when lengths of stay are estimated from a ward census (an example which, ironically, appeared to be missed by the examiners for the membership of the Faculty of Community Medicine) or durations of illness are estimated from those ill on an interview day (as might be done using general household survey data). In fact, subject to some distributional assumptions, formulas are easily derived for correcting for this bias.¹

On the other hand, there is a potential bias that cannot be accounted for owing to the fact that some patients who may be in a census day study of waiting times can never be in an admissions study because they were admitted as emergency cases, or died, or “went private” before being admitted or were never admitted for other reasons. Thus, in this instance, the two types of study actually address different populations. The authors referred to this as causing an “inflation” of waiting list statistics (7 November, p 1197). It is, of course, nothing of the sort. The fact that waiting times can be so long that some patients, legitimately entered on to the lists, become emergency cases, die, or are forced by unendurable pain or distress to “go private” is actually a deflation of admission statistics. Implying that waiting list statistics are “inflated” by these people is Orwellian doublespeak.

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- 1 Cox DR. *Renewal theory*. London: Methuen, 1972.

AUTHORS' REPLY.—We are well aware that the principles illustrated in our third paper on waiting lists are familiar to epidemiologists and statisticians. For this reason we referred to a standard textbook of epidemiology. Both Mr Nicholl and we are referring to characteristics of prevalence data with wide application in epidemiological and health service studies. It is clear, however, that these characteristics are not generally considered in the use of waiting list statistics (and perhaps in the use of other health service statistics too). We seem to be in agreement on the need to draw attention to these points.

In the discussion of our second paper we covered possible reasons why the two measures of waiting list length may be discrepant. We included all the categories discussed by Mr Nicholl. We accept that the term “inflation” has a connotation that “discrepancy” does not. Our point was that the two sets of figures show a consistent discrepancy, a term which we use in the text, for which a number of explanations are possible. We point out that certain categories of patients on the waiting list will never appear in admission based statistics. Some of the patients in these categories will not appear in admission statistics because of the difficulties in obtaining a timely admission. As we say, more detailed studies than our own would be required to determine the contribution of each of several possible factors.

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SIR,—Dr B Don and colleagues rightly point out that a prevalence or census estimate of waiting time will overestimate the proportion of patients who have waited for a long time.

However, Hospital Activity Analysis fails to record the waiting experience of an appreciable number of patients placed on our waiting lists such as those who move outside the district, are admitted as emergencies, or die while on the list; those who are referred to adjacent districts; and those who no longer want treatment because their condition has improved or their attitude to it has altered. The authors' previous paper (7 November, p 1197) suggested that these patients constituted about 28% of the waiting lists they reviewed; they are likely to form a large proportion of the patients waiting a long time, since these outcomes are more probable with increasing time on the list. Thus Hospital Activity Analysis underestimates waiting times. We should be concerned about the waiting times of these patient groups.

A cohort method of estimating waiting time provides the most accurate description of patients' waiting experience and does not suffer from the inflationary or deflationary inaccuracies of prevalence and retrospective methods. This cohort technique follows a group of patients placed on the waiting list over a defined time period until all have left it and records the waiting experience of every cohort member (including those omitted by Hospital Activity Analysis). This technique is also the most appropriate incidence based method for measuring individual chances and risks of particular outcomes—for example, the chance of having to wait over one year or being admitted as an emergency. This is because it provides the appropriate denominator for such risk calculations, unlike the Hospital Activity Analysis technique suggested by Dr Don and colleagues.

Unfortunately no direct measurement of cohort waiting times is performed nationally, though a retrospective cohort method was described by Williams *et al*,¹ which would enable an individual's chances of having to wait for any given time period to be measured. Furthermore, the recent introduction of the Körner data sets will remove one of the main inaccuracies of this retrospective cohort method, caused by a mismatch between the two information sources used.

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SIR,—The findings of Dr M J Goldacre and colleagues (31 October, p 1105; 7 November, p 1197) highlight the complexities of compiling and interpreting waiting list statistics. We think that several other points should have been mentioned, particularly in their first article.

In our experience both cancellations and patients who do not arrive play a separate and important part in the waiting list equation and can therefore indirectly affect overall workload. The number of patients called for admission should therefore play a part in the calculation of waiting list statistics and trends.

Although in the reported study day cases have subsequently been removed from waiting list statistics in accordance with Department of Health and Social Security instructions, they represent a large proportion of the workload in some units and should therefore be audited and presented in a similar manner. Accurate waiting list statistics